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TENSIONS REGARDING EPISTEMIC CONCEPTS

JOSEPH MARGOLIS

Abstract: The paper argues that there is no logic of scientific discovery, but there is an inference-like pattern that we can model as a "logic," retrospectively, once a discovery has been successfully made. While accepting a kind of epistemological pluralism and opportunism, the claim will be advocated that a convergent and reasonably wide-ranging normative "logic" might be constructed, one that might even work reasonably well in selected applications and might (therefore) also lead us to make congruent judgments of irrationality or illogicality wherever it seems not to yield the "normatively appropriate" outcomes in otherwise comparable specimen cases.

Keywords: representationalist form of scientific realism; internal realism; incommensurability, logic of material inference.

I

I don't have an adequate theory of truth, knowledge, meaning, or scientific method. The pertinent convictions that I do have on these grand matters tend to be occupied with a lighthearted sense of a very large philosophical graveyard. I'm much clearer about the difficulties that beset famous efforts to provide a systematic account of these concepts, taken singly or together, than I am about a promising start that may actually approach completion in a manageable future, with assurances that it won't be disastrously derailed or rendered trivial. Furthermore, I find, in all candor, that I am subject to relatively unshakable doubts (that are not in the least depressive) about producing a valid, sound, reasonably complete or completable theory that, if not quite able to provide regular criteria among prevailing practices of inquiry as to whether and when pertinently contested claims may be resolved, decided, effectively settled, may at least provide bits of orderly guidance that loosely follow from the philosophical theories we favor and range over the concepts in question, and that may be fairly assigned a measurable advantage over competing theories. The strong objective seems beyond us; the laxer proposal threatens to dwindle into vacuity or a kind of welcome but banal tolerance. Here, I think of a decisive question regarding what has come to be called "material inference" (following a conjecture of Wilfrid Sellars's that has recently been pursued by Robert Brandom) but which might be construed (more interestingly) as pitting Brandom's search for the rules of material inference and (what was never thus intended) Wittgenstein's profoundly informal and improvisational treatment of our habits of participation in one or another language game. To be honest, I find myself on Wittgenstein's side here.

Without subscribing to Paul Feyerabend's provocative claims about scientific methodology—in his running debate with Imre Lakatos over twin lifetimes lived in a single span—I find myself drawn to versions of his broadside as well as to the somewhat differently

focused but cognate account of revolutionary science that Thomas Kuhn famously advanced in *The Structure of Scientific Revolutions*. Feyerabend of course is more in your face than either Lakatos or Kuhn—inclined to pithy verdicts close to the insulting. But, here and there, and often enough, Feyerabend isolates questions and constraints that are almost never explored by those who are attracted to progressive prospects located in a sanguine way between complete disaster and the somewhat tepid possibilities I've just suggested. Some sort of linearized progress is what is usually sought, which is not the same thing as a linear sequence of systematized theory somehow moving to completion in the analysis of any of the concepts first mentioned.

Here, my intuitions favor, with Kuhn, the idea that a linearized measure of progress is more likely than not to be captive or internal to a prevailing scientific ideology—manifest for instance in Imre Lakatos's conception of a "research program." Lakatos apparently believed or hoped he could escape such capture, believed he could progress (over time) toward a fully systematized account of any of the key concepts in question, which, since they are intended to define the "best" version of realism we can offer, might well override all narrow theories and thus not be an internal captive in the way the first would be. To my mind, the difference between internal limitations of the first kind and the absence of such limitations in the second is probably more apparent than real, for reasons I shall touch on shortly. In any case, if competing theories of the second kind are not easily rendered in any linear way and if progress of the first sort may be hostage to potentially extreme swings of conviction regarding the second, then the contrast between internal restrictions imposed by salient theories, practices, traditions, ideologies and the like and the absence of same in theorizing directly about what an inclusive and adequate account of our concepts would require may itself be an artifactual illusion of some sort. I take this to be the inexorable effect of historied thought.

In any case, Feyerabend has some very useful things to say about scientific method (and, in effect, about the other concepts mentioned, inasmuch as they clearly bear on the analysis of method itself); hence, if any of our concepts fail to reach a certain promising level of progress, probably all will be stalemated in related ways. Thus, Feyerabend, after remarking, in his familiarly sly way, that "astrological medicine employs strict standards and contains fairly uniform basic value judgments," raises the question of a "rational" comparison of "professional ideologies," "professional standards," "professional reconstructions," comes to a very telling question: viz., "To find the right method, one must reconstruct the *right discipline*. But what is the right discipline?" (Feyerabend 1978, 205). The history of philosophy strongly suggests that this is the site of the greatest swings of doctrine, in effect a variant of my second question: I mean the prospects of trying to define an objective methodology in terms of the constraints imposed by analyzing how concepts like truth and knowledge and our capacity to determine meaning contribute to such a methodology, as opposed to defining what, within a reasonably determinate practice, would count as progress.

I am, I confess, one of those who has been smitten by the idea that there is, and cannot be, a "science of science"; that this is, indeed, the upshot of that grand revolution of "modern" modern philosophy that spans the efforts of Kant to work out, in his first *Critique*, a complete, closed, sound apriorist statement of the necessary and essential conditions of the "possibility" of knowledge and science and Hegel's critique of Kant's *Critique* (in his *History of Philosophy*, *Faith and Knowledge*, and elsewhere), which showed, in effect, that Kant failed, could not but have failed—as a result of what he omitted to supply, what he relied on, and what, for reasons having to do with the force of historicity (which was not readily accessible to Kant in Kant's own time)—now confirms, *faute de mieux*, that we simply can never count on an "absolute" sense of "knowledge" or, accordingly, of any of the other concepts in question.

So that *if* a proper analysis of the second-order concepts originally mentioned must be systematic, relationally defined (say) in the holist sense, then confidence in such an analysis must depend on our guess as to the approximative promise of one or another current holist theory relative to what, ideally, may be thought to be completely informed about what (following Hegel's usage) might be called the "absolute" system of the world. Hegel developed the idea in a particularly purple way: for instance, in the *Encyclopedia Logic*, which actually bears on the fortunes of the Sellarsian conception of material inference. But if I understand him aright, then Hegel never failed to keep in mind that the idea was merely "regulative" and artifactual, *never* "constitutive," or known to be, in any sense that would have vindicated a science of science in Kant's sense. The "Absolute" was meant to be timeless, unchanging, beyond historical innovation; but, for that same reason, it was never actually accessible to finite human inquiry!

Now if you take Hegel's critique seriously—I confess I do—then, for one thing, "regulative" analyses of truth, knowledge, meaning, method *are* regulative only in some captive, internalist, "normal," perspectived, or ideologically committed sense that accords more with our skepticism about ever reaching the ideal limit of systematicity than with any knockdown evidence (here and now) that we have hit on the right ideal system or may rightly anticipate we will approach it more and more closely in some imagined set of tomorrows. If you find Hegel's idiom too florid, then let me suggest that Charles Peirce's fallibilism captures Hegel's insight in an extremely lean way (in locating truth as the regulative but otherwise inaccessible limit of the infinite long run of inquiry—which we now treat as the first reasonably articulated form of pragmatism (see Margolis 2007). If you allow Peirce's adjustment, then you have at once a compelling argument in favor of the insurmountable informality of the "method of science" drawn from Hegel's "infinitist" critique of Kant, which appears in a notably neat form in Ernst Cassirer's discussion of contemporary physics, in his *Philosophy of Symbolic Forms* (1957, 475-476).

In any event, we now have a rather convincing reason, drawn from familiar reflections on the fate of truth and knowledge and the rest, as to why we can hardly hope to bring our philosophical analyses to an approximative close. No such hope could possible afford a sound reason for pursuing any such analysis! *The* reason, so to say, collects what I can only call our most persistent, most intuitive, most satisfying, most salient—but hardly confirmable or even disconfirmable—regulative convictions (you may call them "hypotheses" or "abductions" if you wish), without requiring that they be brought into systematically compatible or coherent relations among themselves. Call them the executive heuristics of our inescapably piecemeal, opportunistic, fragmented, even oppositional inquiries that hope or dream (but can do no better than that) regarding the final relationship between the finite and infinite aspects of human inquiry. Feyerabend (2000, 116), thinking along related lines, cites the following courageous advice Albert Einstein once offered on the analysis of epistemological concepts:

> The external conditions which are set for [the scientist] by the facts of experience [Einstein notes] do not permit him to be too much restricted on the construction of his conceptual world by the adherence to any epistemological system. He, therefore, must appear to the systematic epistemologist as a type of unscrupulous opportunist...

Absolutely splendid, to my mind. Except of course for the nagging thought that Einstein may himself have been captive to a limited conceptual scheme of the sort he's warning us about, when he opposed supporting quantum theory. In any case, *this* is probably the meaning of Feyerabend's notorious provocation, "anything goes"—the nerve of what he's called "epistemological anarchism." In a fair sense, Feyerabend never really changed his views from

their first sustained formulation in *Against Method*, once he'd settled on his anarchism. You'll find one version of it in the opening lines of the first chapter of *Against Method*:

> The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. We find then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other (Feyerabend 1978, 18).

Regarding this, he adds in a prefatory summary: "The only principle that does not inhibit progress is: anything goes" (*ibid*., 23) This may be anarchism, but it's not chaos; it's a kind of ready wit of a practical cast, not a doctrine of any recognizable sort. The familiar accusation that Feyerabend holds that any two hypotheses or theories or beliefs are of equal value is a piece of malice or misreading. He's speaking (let us say) about the relationship between the finite and the infinite, as that is sometimes put regarding fruitful cognitive conjectures—and what he's saying is that all would-be principles of the epistemological sort are heuristic and provisional in their practical application, and cannot be more; that there are and can be no strictly universalistic truths in science or philosophy, though there's also no reason to avoid advancing a pertinent conjecture in its universalized form—for rhetorical emphasis or political effect.

Philosophy and science are inherently fragmented—however holistically packaged we may wish to make them out to be. We cannot say when we may need the weakest of these conjectures: it may always return in a new guise to better all the others for a season. There's the point about astrology and Aristotle's physics. Feyerabend's right about this, whatever may have been his worst extravagances. I think it's the same point as that of Kuhn's theme of "paradigm shifts" and the "unscrupulous opportunism" Einstein recommends.

Let me venture a counter proposal, here, from the side of the opposition—or, from what looks like the opposition but may not be: Harold Brown, who is an impressive advocate of Wilfrid Sellars's *systematic* account of "epistemic concepts"—the ones already mentioned—offers the following provisional distinction between "truth" and "ideal truth" along the lines of Sellars's relatively late account in *Science and Metaphysics.* Brown explains (1991, 338):

> *Truth* is whatever is semantically assertable in some conceptual system, and different conceptual systems yield different truths. *Ideal truth* is what is semantically assertable in a completely adequate conceptual system.

This may strike you as a startling—and altogether untenable—position, until you realize that Sellars had "defined 'truth' as 'semantical assertability': '*p*' is true [Sellars affirms] just in case the rules of the language in which '*p*' occurs allow the assertion of '*p*'," where, as he goes on, "everything we know or believe about a particular subject matter is built into a conceptual system" (*ibid.*). The trick is to see that material truths and beliefs (*and* even "our tendency to describe" objects in a certain way) count as "aspects of [our] concept [of truth]." But, of course, this means that "truth" can't be formalized in the inferentialist manner proposed, unless the material information to be included in the definition of truth can be shown to be formalizable as well—let us say, in the form of *rules* of "material inference." This, we realize, has become a rather fashionably recuperative program, endorsed quite recently by the combined efforts of Richard Rorty and Robert Brandom (see e. g. Brandom 1994).

Be that as it may, Brown means to improve on Sellars. He moves at once to avoid potential paradox by affirming, straightforwardly, that "there is no reason to use 'truth' to refer to

anything but 'ideal truth'"; he adds, in and for the rest of the "Epistemic Concepts" paper: "this is the only way I will use [the term] 'truth'." Fine. But Brown means (by all this) to use the concept (truth) "as an *ideal* in a sense that is close to Kant's"; and about this he adds:

> Kant considered the idea of the complete causal sequence responsible for an event as one example of a regulative ideal: we can never establish such a sequence, but the ideal directs us to continue the search for causes preceding those causes we have already discovered, and this quest increases the scope of our empirical knowledge (Brown 1991, 339).

If I read this correctly, Brown is committed to the "ideal" sense of "truth" in accord with the somewhat unexpected consequence that "ideal truth" is itself no more than "truth" in the weak sense that (clearly) risks being problematic and paradoxical—and even "relativistic" in a sense both Sellars and he, committed "scientific realists," would heartily reject. (Brown finds Sellars's accommodation of the two senses of "truth" distinctly inconsistent.) My own criticism is that the supposed "increase" in realist knowledge is *always* captive or "internal," and that "ideal truth" cannot be realist in any sense opposed to Peirce's fallibilism—which deliberately sets truth beyond the reach of any human inquiry and which is unwilling to commit to asymptotic assurances of any kind beyond rational hope.

If, now, you read the problem of truth—*a fortiori*, knowledge—in the way Brown recommends, then the correspondence theory has no obvious prospect of being strengthened: because, for one thing, the "relation" between (say) facts and propositions has not yet been explicated beyond what looks to be no more than a vacuous role; and, for another, the problem is worsened, if anything, by advancing it in the context of "ideal truth," which, after all, is humanly inaccessible. Nevertheless, I don't think the correspondence theory *is* useless; and I do believe Brown has considerably strengthened Sellars's important proposal! Behind correspondence, however, there looms the lesson of the grand transformation effected by Kant and Hegel: namely, that the disjunction between the "subjective" and the "objective" cannot be made out any longer, except in internalist terms, within one conceptual system or another. There's the deeper problem about truth and knowledge.

For my part, I'm entirely willing to take the impossibility of "ideal truth" as an uncontested given—an insuperable constraint on the prospects of *finite* inquiry. What it signifies, to be entirely candid, is that there is an obvious threat of relativism (that Brown himself flags) that scientific realism cannot overcome. *I* believe that realism and relativism are compatible, but I'm aware that scientific realists claim, not uncharacteristically, that realism and relativism are incompatible and that relativism is itself self-contradictory. I'm not convinced. In any case, on Sellars's theory (and Brown's improvements) and, doubtless, on independent grounds, scientific realism (and other standard forms of realism) face a deep dilemma. For example, as far as I know, Hilary Putnam, who is a vigorous opponent of relativism, neglects, in urging the compatibility of realism and pluralism, to demonstrate that his own pluralism can preclude (a self-consistent version of) the kind of relativism Brown admits Sellars's treatment of "truth" would entail.[1]

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[1] See, for example, Putnam (1987). I do not believe Putnam has ruled such a possibility out in any of his later publications, though it's possible that I'm mistaken. Of course, he holds that relativism is inherently self-contradictory; but I know of no argument of his in which he shows, compellingly, that it's impossible to formulate a consistent form of relativism, something that might be reasonably admitted to be a bona fide version of relativism. On the contrary, his well-known claim about the *Grenzbegriff* (of truth) seems to require a more robust picture of "ideal truth" than either Sellars or Brown has ventured.

II

Here, I suggest we turn in an entirely different direction. We must take Feyerabend's original question more seriously: "What is the right discipline?" The admission that our epistemic concepts cannot be merely formal, must incorporate material information and material inference, points unmistakably in the direction of embedding our abstract concepts in what we hope will prove to be a perceptive grasp of the actual history of cognitive (or scientific) inquiry. The lesson to grasp is that the valid rigor of any relatively formalized inferential analysis of knowledge (say) cannot be firmer than our informal (but substantive) conjectures about the systematically decisive discoveries the actual history of science will be able to vouchsafe—and *that is a question that must be addressed to "ideal truth*." The formalists of Sellars's stripe have hardly touched on that complication. To be plain: they're looking in the wrong place!

I can mention at least two compelling saliencies that regularly tend to yield changes of methodologically important sorts that, though focused on theoretical revision, confront us in a decidedly practical and concrete way. One is obviously associated with Kuhn's notion of a crisis of paradigms: where, more or less ad hoc, what is judged to be an important problem here and now in the running of a "normal science" appears to be confronted with the imminent exhaustion of the resources of that science's "paradigm" in attempting a resolution. We might almost suppose that Kuhn had Einstein's "unscrupulous opportunist" in mind. As Kuhn notoriously maintains, we have a sense of a conceptual gap here, and, in overcoming the perceived gap, we find ourselves tempted and pressed into favoring, opportunistically, a theorizing leap that puts at risk all of the accumulating "progress" of a normal science, without adequate assurance that we will be able to retrieve what we would not wish to lose in the bargain. This is precisely what Feyerabend finds has been ignored or slighted by Popper and Lakatos (for instance)—and, doubtless, would be deemed to have been too comfortably accommodated by Sellars and Brown and Brandom.

The other sort of saliency is very persuasively illustrated in Ian Hacking's work, not only in terms of undoubted cases but in terms of a general objection to any "representationalist" form of scientific realism:

> In physics there is no final truth of the matter, only a barrage of more or less instructive representations. . . .That is why I turn from representing to intervening (Hacking 1983, 145).

Hacking (*ibid.*, 130) means, for one thing, that representationalism is essentially idealist rather than realist, which accounts for his "pity" for Hilary Putnam, "once the most realist of philosophers," more recently committed to "internal realism" (which he has also now abandoned), that is, a form of idealism (see also Putnam 1987). Secondly, he means that divergent representations and diverse interpretations of such representations may be fitted to what we deem to be real, so that we cannot then bring such a plurality to a unique resolution. But, thirdly, he also means, speaking as a pragmatist, that *our* refusing the familiar "dichotomy between acting and thinking"—"from which such idealism arises"—itself leads us to see that the "final arbiter in philosophy [regarding what is real, for instance] is not how we think but what we do"—what we do experimentally (say) or observationally on such occasions, in effect by intervening in the world (*ibid.*).

Here, the nerve of the argument is easily missed. Because it's true enough that Hacking opposes idealism, especially the insouciant form that teaches that "all observation is loaded with theory," from which it follows that "we seem completely locked into representation, and hence into some version of idealism." I don't believe, however, that Hacking means to deny

that there is an important conceptual link between theory and observation; he holds rather (if I understand him aright) that there are countervailing considerations bearing on *intervening* (particularly, intervening experimentally) that assure us that what we thus alter and perceive (what is thus affected) *is* real in a sense that cannot be captured by the idealism of representations (*ibid.*). What, in thinking of our inquiries, we represent (or model) as real remains idealist and instrumental; but the real itself, however, difficult to specify, is not actually constructed (as real) but is more nearly what (rightly construed) we simply encounter in our interventions (and represent, constructively, there, in our model of reality).

The pretty point in all this is just that our grip on the real is most compelling (if we have any grip at all) in events of deliberate and experimental intervention *and* that, there, our sense of *having hit on* something decisively real is more hospitable to diverse and competing interpretations than are theoretical representations. Intervention commands the attention of competing theories whereas our *thoughts of* what transpires in any experiment tend to favor one quite orderly "idealist" play rather than another. You realize, therefore, that *if* there is also some play between our sense of the presence of an as-yet undetermined existent "something" and what, construed determinately, according to competing theories, we may rightly predicate of it, there can be no antecedent determinate rule for resolving such an indeterminacy (or, better, such a determinability) and, therefore, *any* relatively entrenched, normal social practice of "material inference" (to speak altogether of the different views of Sellars, Brown, and Brandom, under the generic title Brandom has effectively endorsed) will be rendered logically informal, improvisational, ad hoc, profoundly open to diverse extension, subject to incompatible and incommensurable options, not reliably committed to bivalent values or the like.

This obviously introduces an important complication that (I feel) is not satisfactorily addressed by either Brown or Brandom (though they address their share of the pertinent issues in what look to me to be very different ways indeed): Brown treats Sellars more as a Kantian; Brandom, more as a Hegelian. That difference alone profoundly affects our sense of the "logic" of pertinent claims and arguments. I myself pose, against all three discussants, problems that strike me as analogous to Kuhn's well-known challenge, though they are more garden-variety issues than Kuhn's paradigm-crisis and appear to suit the improvisational fluencies (without rules) that Wittgenstein favors among his "language games." Games, you realize are sometimes defined by rules, and sometime have no rules at all. On the argument just sketched, "practices" do not always justify the expectation that *they* harbor "implicit rules" that are readily made explicit. This goes against the views of all those figures just mentioned, but, most notably, Brandom's—who reads Sellars along Hegelian lines.

It's in this spirit that Hacking offers a paradigm example drawn from J. A. Millikan's 1908 attempt to measure the electrical charge on the electron. Hacking brings the story more or less up to date in terms of attempts to establish the existence of quarks, which Hacking treats as "fractional electrical charges." That is, Millikan's technique, which involved suspending and then dropping a small negatively charged oil droplet between electrically charged plates under variably controlled conditions (that is, with the electric field switched off, switched on, applied with different charges), successfully supported the reasoned calculation that the charges on the drop (which implicated rough assumptions about the effects of gravitation, the sphericity of the drop itself, the viscosity of the air, and the like) "are small integral multiples of a definite quantity." That quantity, e, is deemed to be "the name of that [unit] charge," the electron.

The possible free existence of quarks, entities having a conjectured charge of 1/3 e, was then successfully confirmed by an application of Millikan's basic strategy. Hacking acknowledges the usual philosophical challenges to the realist standing of electrons and quarks (for instance,

whether the extension of the Millikan procedure, or indeed the original procedure, actually measured what it purported to measure). But Hacking's own emphasis lies elsewhere. Given the short and rare life of quarks, the extended experiment introduces balls made of a convenient element, niobium, which are then sprayed with oil drops 10^7 times larger than Millikan's drop, in the hope that a free quark might get stuck on one or another of them. Repeated experiments were judged to confirm the estimated fractional charges.

The explanation offered Hacking by a friend who reports the experiment has it that "we spray it [the niobium ball] with positrons to increase the charge or with electrons to decrease the charge." Hacking's response (*ibid.*, 22-24)—which explains his own commitment to scientific realism—simply holds: "So far as I'm concerned, if you can spray them then they are real". This is not meant in a crude way; none of the appropriate philosophical doubts is shortchanged by this "healthy reaction" (as Hacking puts it). It's meant rather to provide a strong reason for going realist about theoretical entities—a reason not unfairly linked (in my opinion) to something very close to Charles Peirce's pragmatist intuition about Secondness, which has the appealing virtue of overriding subtle, specifically targeted anti-realist objections to this or that conjecture regarding the nature of what has been encountered, all the while favoring the sense of actually having encountered something real. The beauty of this line of thinking is that it disturbs as little as possible the entire run of all the presumed indeterminacies of truth and knowledge and method, the determinability (up to some agreed-on measure of precision) of what we suppose we have encountered, and the isolation of a seemingly independent and fundamental ground, *not* preemptive in any cognitive way addressed to determinable values, on which to affirm the presence of a hitherto unacknowledged reality. I find this very prettily constructed.

The argument, I would say, is notably persuasive, as persuasive as *any* argument could possibly be under the circumstances: it catches up a deep tendency among humans to yield in a realist direction. But it is, of course, an insuperably informal argument. Peirce, I might add, treats similar encounters as what, in his idiom, we might call an "instinctive abduction," which is not intended to end dispute unfairly but, rather, to approach what (perhaps) Wittgenstein calls "bedrock" in disputes brought to bear on reflections regarding our deepest habits of life or inquiry. May I call that the pragmatist solution to the problem? It's not in the least foundationalist: it answers (if it answers to anything at all) to that phenomenological feature of episodes of Secondness in which "intervention" (in Hacking's sense) overwhelms doubt about having encountered "something real." Inquiry about what to take to be the nature of that "real" continues in the usual way. In this very plausible sense, realism does not ultimately rest on the confirmation of any determinately realist *proposition*: it couldn't possibly! But then, surely, our theories of the *nature of what is real* presuppose a deeper realist commitment that those theories pertinently address—or are guided by.

Here we find ourselves in uncertain territory, familiar and yet inconclusively mapped. I have a suggestion about how to make sense of all this, in a way that fits very nicely what I've sampled and alluded to in the work of Feyerabend, Kuhn, Hacking, and Wittgenstein. My own discussion is much too preliminary to validate the conclusions I'm drawn to. But if we qualify the convergent themes of the four figures just mentioned by the additional themes I've barely aired, by tracing Hegel's contribution through the leaner focus of Peirce and Cassirer, which ultimately derive from Hegel's own critique of Kant's philosophy—I mean themes like these: the indissoluble unity of the subjective and the objective, the indemonstrability of a science of science, knowledge treated as a transient artifact of history, ineliminable practical concerns in the description of the real world, the continuum of the finite and the infinite in any inquiry committed to objective truth, the impossibility of any cognitive mastery of the inclusive (or

"absolute") totality of all that "is," and the *faute de mieux* standing of all the above—then the conclusions I would like to strengthen may be seen to draw on the convergent lesson of these two specimen cohorts, directed (for essentially didactic, even heuristic reasons) against the overly sanguine, rule-governed (or algorithm-minded) proposals regarding the analysis of the logic of material inference that collect the fresh views of Sellars, Brown, and Brandom (for example). But to concede the validity of the contest sketched is to grasp as well the sense in which the Sellarsian company is kin to other figures like Lakatos and Popper and Carnap and (ultimately) Kant. So the contest is a serious one, bound to ramify through the entire span of "modern" modern philosophy.

Allow me, therefore, to propose a second cognate intuition to match Hacking's sense of the presence of "existent reality" in considering the effects of intervention—Secondness, in Peirce's sense.[2] Peirce offers a thought-experiment rather than a report of an actual experiment, as in Hacking's reflection; but it's entirely reasonable that he should do so since his intuition concerns the *realism* of general predicates rather than an existential encounter with *real things*. (Peirce is as perceptive on predicables as anyone in the literature, though I think he is mistaken in claiming that his Scotist or "scholastic realism" validly *represents* the realism of "real generals.") Peirce means by scholastic realism "*that general principles are really operative in nature"* (Peirce 1935, 5.101).

Peirce regards "the general [as] essentially predicative and therefore [as he puts it] of the nature of a representamen." Broadly speaking, he means by this that a "predicative" term is a representation and, when valid, a representation of a "real general"; but, of course, he also claims that "the universe is a vast representamen, a great symbol of God's purpose, working out its conclusions in living realities" (Peirce *ibid.*, 5.119). This last is not my concern at all.

I have in mind an important lesson bearing on the would-be laws of nature: you'll see the upshot in a moment. To make a start, I find the following quite a remarkable—pertinent and compelling—pronouncement, the full meaning of which is hardly clear on a first reading:

> A law is in itself nothing but a general formula or symbol. An existing thing is simply a blind reacting thing, to which not merely all generality, but even all representation is utterly foreign (*ibid*., 107).

Peirce brings this important but widely neglected distinction to bear at once on the matter of inference: a man "cannot really *infer* without having a notion of a class of possible inferences, all of which are logically good" (*ibid.*, 108). But of course what is *good* in the way of inference must, on Peirce's own (and on every other responsible) view, *depend on the validity of whatever is collected under the auspices of the generality of the predicates we invoke*! But how is that to be decided? It's just *there* that Peirce makes a perfectly stunning contribution that (as far as I can see) completely undermines the force of his own scholastic realism, and puts all predicative generality at mortal risk, but *not* the artifactual force of an admittedly idealized generality that may be brought into line (by improvisation or incentive conjecture) with our sense of our brute encounters with whatever exists. I need your patience here.

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[2] Peirce holds:"[R]eality means a certain kind of non-dependence upon thought, and so is a cognitionary character, while existence means reaction with the environment, and so is a dynamic character, and accordingly the two meanings . . . are clearly not the same" (Peirce 1935, 5.503). Existence answers to Secondness, therefore: Peirce is making room for the "reality of generals," that is, for the effectiveness of "real generals" in the world. But his sense of "existence" suits Hacking very neatly.

> Here, now, is Peirce's thought-experiment, the one I signaled a bit ago: Take any two possible objects that might be called *suns* and, however much alike they may be, any multitude whatsoever of intermediate suns are alternatively possible, and therefore as before these intermediate possible suns transcend all multitude. [That is, "sun" is itself a general term which, supporting inference, must extend beyond finitely many specimens to hitherto unknown instances—and, ultimately, as a law of nature, to an infinitude of possible specimens.] In short, the idea of a general involves the idea of possible variations which no multitude of existent things could exhaust but would leave between any two not merely *many* possibilities, but possibilities absolutely beyond all [actual] multitude (*ibid.*, 103).

Here, Peirce introduces, it seems to me, a crucial distinction between *mere* generality and *real* generals, which, when cast as laws of nature, must be exceptionless—even substantively necessary—that is, must be, or must accord with, true universals (if there be any). But, for one thing, on Peirce's view, even "real generals," fitted (let us suppose) to a finite "multitude" of existent things, cannot support *inferences* to *further* natural possibilities without implicating reliable laws—universal uniformities—which they cannot possibly capture; and, for another, idealized laws that admit no exceptions cease to be constrained by brute Secondness (see e. g. Cartwright 1983). If so, then the "realism" of scientific realism cannot mean the same thing when applied to the existence of real things and when applied to the validity of the laws of nature. Peirce collects the treatment of the first under the category of Secondness and the treatment of the second under the category of Thirdness (which means, reflectively, under some form of idealized, even practically skewed interpretation). But the distinction seriously affects the prospects of synthetic inference, inasmuch as it affects the course of extending a general predicate. A mere commitment to realism hardly touches the issue.

If anything remotely in accord with this sort of argument holds true, then, at the very least, there cannot be any determinate rules of "material inference": they would violate the very condition under which predicative inference extends beyond any aggregate of finite exemplars we may choose. I take this to be one of the most strategically placed challenges possible to *any rulelike* picture of material inference, no matter how attenuated any actual practice may be, that still dares to claim the right to count as a workable "logic." I think there is no such logic, though I don't claim that material "inference" is arbitrary for that reason: it's just that its informality (its informal rigor) is *sui generis*; induction, by contrast, is best viewed as one or another determinable strategy internal to normal or well-formed practice. (That's surely part of the point of Peirce's contrast between induction and abduction.)

I'll venture two further comments. First of all, *all* of the figures whose views I've been comparing are (if I've read them correctly) committed to the thesis (which is ultimately Hegel's, directed against Kant) that theoretical science is grounded in something like practical intervention: hence, in ways sympathetic to Hacking's and Peirce's alternative forms of "pragmatism"; hence, also in accord with construing predicative generality in terms of the practical success of our deliberate responses or interventions with what we are persuaded are existent things encountered in the way of Secondness).

This completely outflanks the all-but-useless classic appeal to predicative universals or (say) Platonic Forms, which, if they were indeed accessible, would provide a ground for a strong account of material inference. But nothing of the kind has ever been convincingly discerned. There's more to the lesson. If you recall Mendeleev's remarkable predictions about the numerical and qualitative properties of at least two hitherto unknown elements—in spite of the fact that his guesses at his fledgling periodic table were seriously off the mark—you see how it's possible that valuable gains may sometimes be made by guesses at what, on the strength of

a very shaky theory, may indeed exist, even where the predicable nature of what is conjectured to exist is seriously in error! That's worth pondering. But sound material inference depends on sound predication.

There's a double source of error there: the extension of general predicates and the substitution of a more nearly correct run of predicates in place of an earlier poor guess show how the "logic" of material inference cannot be made to rely entirely on a merely "internal" practice (or, "linear" improvements to such a practice). Too much may go wildly wrong if we are not close to guessing what (pertinently) exists and if our predicables are not in close accord with valid laws of nature.

Think, here, for starters, of inferences in strongly historicized or idiosyncratically personal contests: in politics and art and social relations, for instance.

Brandom believes he has a suitable answer:

> I take from Hegel [he says] the idea of a rationalist expressionism. By "expressionism" I mean the idea that discursive practice makes us special in enabling us to make *explicit,* in the form of something we can *say* or *think,* what otherwise remains *implicit,* in what we do. Calling it "rationalist" points to the crucial role of *inference,* in determining what *counts* as *explicit*. At the base level, this means the theorist must explain what we have to be able to *do* (what sort of practical know-*how* we have to have) in order to be claiming or intending *that* something be so (a kind of knowing that). The inferentialist answer is: engaging in a social practice that has the structure of giving and asking *for reasons* (in Testa 2003, 561).

The idea certainly makes a sensible beginning. But in spite of the fact that the phenomena in question involve "something we can *say* or *think*," an inferential model fitted to what occurs—a measure of success in reasoning by way of such a "logic"—I think it's too much to say that we are in the presence of anything as well-formed as a determinate logic, a logic of material inference.

There is no logic of scientific discovery, for instance, but there is an inference-like pattern that we can model as a "logic," retrospectively, once a discovery has been successfully made! Both Peirce and Popper have considered the question. There is no logic of dreams, but Freud was able to formulate plausible (not usually true) reconstructions (including various enabling processes that Freud "invented") of how his neurotic patients may have thought in their waking life in order to extend his model into their dreams.

There are, also, important bits and pieces of reasoning that cannot be denied. For one thing, there are actual deductive inferences implicated in material inference. For another, there are minimal linkages of rational thought that are so deeply characteristic of human thinking that they cannot be contested except on extraordinary grounds: for example, that no one will attempt to do what he believes to be impossible, though he's never thought of the linkage explicitly. There is no general model of rationality, contrary to what Jürgen Habermas (or even John Rawls) believes regarding normative matters, either individually or in terms of community life. Fine-grained models of rationality may be quite local to their would-be concrete manifestations; certainly, that is characteristic of ideological thinking. Surely there is no logic of "normative progress" involving, for instance, Axel Honneth's notion of "struggles for recognition" as to how to "constitute the logical infrastructure of the moral progress of society"—for instance, regarding "what historically individuates the unity of reference [social classes, genders, or what?] that is relevant, at a given moment, to individuate which normative relations have to be judged along criteria of justice."[3]

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[3] This is Italo Testa's question to Brandom, in (Testa 2003, 566).

Brandom's answer to the above remark (put originally as a question) is decidedly instructive—in a way I daresay Brandom had not intended: "The question as put [Italo Testa's question] presupposes [he says] that in any given situation there is such a thing as *the* way of individuating the unity of responsibility and authority that matters for assessments of justice. But they *all* matter. By exercising our discursive expressive capacities, we keep discovering new descriptions, new ways of describing and individuating potentially morally and politically significant units, and hence, new forms of possible and actual injustice. This is progress" (*ibid.*, 567).

But, to challenge Brandom: for one thing, *if*, in Brandom's answer, "all" signifies "all possible conceptions of justice" (which it obviously does not), then it would require a form of "absolute knowing" that Hegel in his better moments takes to be completely beyond finite human inquiry; and if it is less than that, then it fails by exceeding (in Peirce's words) any finite "multitude" of possibilities—and, there, there is no asymptotic "progress" to be had. As far as I know, Brandom nowhere addresses in a sufficiently fine-grained way the grounds for supposing that the inferential logic attributed to one concrete strand of thinking *is* demonstrably *pertinent* in judging the normative standing of our treatment of another such strand somehow assigned to the *same* "implicit" logical process (which is likely to introduce different "material" considerations). Might not the "explicit" *relevance* be innocently cooked? I think it must be very seriously considered, especially where we generalize more and more widely about the normativity of our would-be "logic." (I see no asymptotic relief here.)

For another thing, the collision of the "possibilities" that may arise in this connection may produce such a profound form of incompatibility or incommensurability that no ideally inclusive logic would make any sense: I think this may in fact be part of the bitter lesson of the suicide bombers of our day, or bin Laden's *fatwa* validating the justice of the wholesale slaughter of would-be "innocent" infidels, or indeed speculations allegedly reported from Iran that a nuclear war that would annihilate both Iran and Israel would be justified (would be rational in the way of material inference and worth the effort) on the grounds of its consequences for a life beyond death. And, for a third, the only way Brandom's argument could be compelling at all would require *our* discovering how some one individual or community thinks and feels and acts in very narrow contexts of investigation, where such "patterns," themselves idealized along the lines already sketched, may be rightly deemed regular enough, ranging over a significantly larger interval or population, that it would make sense to construe it as one form of material inference at least.

But then, of course, the method would to too powerful, because it would already have smoothed out the doubtful inferential analogies and similarities of different concrete cases, so that a convergent and reasonably wide-ranging normative "logic" might be constructed, one that might even work reasonably well in selected applications and might (therefore) also lead us to make congruent judgments of irrationality or illogicality wherever it seems not to yield the "normatively appropriate" outcomes in otherwise comparable specimen cases.

I do see the empirical importance of fathoming how people think—individually, aggregatively, as communities and societies, or through historical change. But I don't see the significance of speaking of a reasonably determinate logic of material inference said to hold true of significantly large populations or populations significantly ordered one way or another—say, by race or gender or nation or the like—under conditions like those I've mentioned. I don't find good prospect for any such undertaking. I see the effort, rather, as an analogue of Mendeleev's conjecture applied to the question of rationality. Here, "making it explicit" may be no more than an artifact of a very large number of conspiratorily coordinated "implicit" sub-processes, however innocently assigned to the same overriding "logic."

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Department of Philosophy,
Temple University,
728 Anderson Hall,
Philadelphia, PA 19122
USA
E-mail: josephmargolis455@hotmail.com