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DEFLATIONARY METHODOLOGY AND RATIONALITY OF SCIENCE

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ABSTRACT

The last forty years have produced a dramatic reversal in leading accounts of science. Once thought necessary to (explain) scientific progress, a rigid method of science is now widely considered impossible. Study of products yields to study of processes and practices, unity gives way to diversity, generality to particularity, logic to luck, and final justification to heuristic scaffolding. I sketch the story, from Bacon and Descartes to the present, of the decline and fall of traditional scientific method, conceived as The Central Planning Bureau for Science or as Rationality Czar. I defend a deflationary account of method and of rational judgment, with emphasis on heuristic appraisal and cognitive economy.

1. *Introduction*

In the opening sentence of *The Structure of Scientific Revolutions* (1962), Kuhn predicted a decisive transformation in our conception of science if we took the new history of science seriously. Not even he could have predicted, nor did he entirely welcome, the dramatic reversals that would be achieved by historians, sociologists, anthropologists, psychologists, and a new breed of philosophers of science. For something like a "transvaluation of all values" has occurred. Before, a rigorous, general scientific method was thought necessary to (explain) scientific progress; now many agree that such a method would make progress impossible. Then, experiment was handmaiden to theory and of no special interest; today, its autonomy, creativity, and craft skills are central to many accounts of science. Similarly, justification, prediction, explanation, and engineering applications of scientific knowledge are recognized to be highly creative activities for which there can be no ironclad rules or fixed rationalities.

Accordingly, '*the* scientific method' becomes something of an oxymoron, rather like 'the artistic method'. Ironically enough, science itself turns out to be more art than science. Then, individual Great Men at Great Moments were the locus of innovation and the principal bearers of knowledge; today, the focus is on the daily activities of communities of ordinary scientists and technicians, and on society as a whole. Then, the task was to characterize science in terms of the logical structure of its products; now the processes, the practices, are central. Then, philosophers attempted to reduce knowledge-how to knowledge-that; today, they are tempted to try the opposite. Then, nearly everyone saw the sciences as becoming more unified over time, in both method and doctrine; today, many find increasing diversity instead. Unity gives way to diversity, theory to applications, generality to particularity, logic to luck, reasons to causes, beliefs to practices, final justification to heuristic scaffolding; and so on. "But many that are first shall be last; and the last shall be first."

This *volte-face* by most science studies practitioners and by some (but not nearly all) philosophers only confirms the ironic observation that although an explosion of scientific and technological knowledge has transformed the world, we understand scientific inquiry itself — how we did it — very poorly. Our self-knowledge here is knowledge of a low order. Nor does the great reversal indicate that we have got it right today. We are pretty sure that the positivists were wrong, but we cannot agree about who is right.

Two questions that we cannot answer with assurance are: "What is the role of the individual investigator in relation to the group?" and "How much variation in scientific work is rational?" The answers proffered to these questions by writers from Descartes to Feyerabend have differed markedly, and many social constructivists would simply reject the rationality question on the ground that rationality is a quasi-theological notion, irrelevant to scientific practice. Descartes only reluctantly admitted a community of investigators at all, and, like Bacon (or at least the Baconian stereotype), insisted that everyone should follow the same, rigid, universal, foundational method, with no serious variation. Insofar as rational individuals become methodological placeholders, a scientific community is simply a collection of such individuals. In that limiting case, methodological individualism is correct and there is no special problem about the scientific community.

When authors employed a theoretical account of the individual in relation to group authority in science, it was usually a Modernist political model rather than an economic model. The liberal individualism of the 17th- and 18th-century founders of modern politics and epistemology placed a premium on the individual as the carrier of knowledge, the locus of reason, and, accordingly, the center of inquiry — the point at which new knowledge comes into the world, although consent of others was necessary to license new claims.[2] This political model remains popular, especially among philosophers, who still regularly appeal to a *consensus* of belief in the relevant scientific community, sometimes in the guise of the "reflective equilibrium" of Rawls's *A Theory of Justice* (1971).

The 19th-century discovery of culture, the 20th-century discovery of 'deep' culture, even within scientific communities, and more recent studies of scientific practice have challenged this individualism. The result has been a shift in the historiography of science. In many quarters, individual biography is now out of fashion. Rather, it is said, we should study cultural generations, cohorts, or institutions, or at least write *collective* biography (prosopography). There is a strong temptation to 'correct' older, heroic histories by viewing the erstwhile heroes merely as products of their cultures, or at best as people who were in the right place at the right time to make opportunistic use of specific cultural resources.

However, some science studies experts consider it an overcorrection to classical individualism to lose the individual in the group. And few are comfortable with the Hegelian-sounding idea that the primary agents of human history are whole societies or cultures, much less abstract Reason. While cultural determinism may have been a natural, initial response to extreme individualism, there are now many attempts to restore human agency, albeit an agency located in rich, socio-cultural fields of action. Even scientific biography has not lost its defenders.[3] Several recent biographies are sophisticated productions that allow for agency without hagiography, e.g., without any special intuitive genius or foresight on the part of the principals. A currently popular model of scientists in action is actor-network theory (Latour 1987), according to which scientists are militant players in elaborate networks of power and resource relations. This approach blurs the old internal/external distinction between 'internal' logical and technical developments and 'external' factors such as religious and political context, idiosyncratic personalities, and funding.[4] For who is to say that Jones's thinking up a new hypothetical mechanism for a phenomenon is more crucial to the advance than Smith's development of the test probe that furnished crucial data, than Acme Amalgamated's provision of the purified sample to which the probe was applied, or than the National Council's award of the necessary research funds? We are forced to admit that appeal to scientific method can, at best, account for only a small part of what it takes for the sciences to flourish. But, then, methodologists never claimed to explain everything.

The current sensitivity to social context in social studies of science rarely issues in a simple consensus theory. Most writers, including philosophers such as Fuller (1989) and Rouse (1987, 1996), are critical of Modernist political models and reject folk psychological talk of beliefs and propositional content. Rouse warns us not to reify contents, and Fuller claims that a consensus of private beliefs is by no means necessary to account for observable scientific practice. There is more slack there, he says, than most philosophers would care to admit, insofar as the variation in beliefs makes no difference to scientific practice. We need new models of group formation and dissolution, community solidarity, and power relations.

2. *The Idea of a General Method of Science*

Let us begin our investigation of the role of individual variation in relation to the group by looking at some classical ideas of a rigid, general, scientific method. Philosophical thinkers and traditional sociologists of knowledge such as Mannheim (1936) and Merton (1973) treated scientific activity as special, relative to other enterprises, because guided by a distinctive, epistemically justified and justifying method, or at least by a set of epistemically beneficial social norms. Today, however, it is not science studies experts so much as laypersons, including college administrators, who believe in a single, definite scientific method — as something that every student should be taught. On this view, the chief agent of (mainly additive) scientific change is neither individuals nor social collectives but method.

What is this method? Surprisingly, given the supposed methodological unity of science, there are dozens of different conceptions of it.[5] On one view, method is the logic of inquiry, meaning the primary agent of scientific development, a kind of Central Planning Organon or perhaps a Central Intelligence Agency that has deciphered part of nature's secret code. A more modest view is of method as a Rationality Czar. It cannot direct inquiry but does uphold high logical and epistemic standards. Then there is method as an idealized, anonymous, disembodied investigator or lab director, or as the internalized rules of the scientific community. As such, method represents perfect, Chomsky-like research competence without the impediments of material, bodily implementation to limit its performance. (Note the insinuation, already, of an internal-external distinction.) Bacon and Descartes both viewed method as a kind of mental prosthetic that reduces human fallibility and levels wits. A less rule-based view holds that method is the interiorized, collective wisdom passed down from masters to apprentices, a kind of Freudian internalization of the teachings of the 'father', a scientific superego.

Suppose, then, that scientific work is directed by a rigid, comprehensive, step-by-step, stereotypical Baconian or Cartesian method. As a leveller of wits, such a method would have to be easy to learn and 'user friendly' in order to insure that everyone proceed in the same manner. Having learned the right method, any fool could do science just about as well as any other — and in exactly the same way.

There would be minimal scope for individual initiative or skill under such a regime. Perhaps some fact collectors would be more observant or experimentally proficient than others, but method is supposed to compensate for cognitive deficits and to reduce inference to a near-mechanical procedure. Hence the metaphor of method as a sausage grinder: pour in the facts and turn the crank! In the limit of a fully adequate method, we human beings become mere assistants supplying the epistemic demands of the method. The 20th-century positivists and Popperians retained a conception of method as general, a priori (or conventional), and hence ahistorical and content-free; but they claimed that such a method applies only to the justification of claims rather than to their discovery (and so falls short of complete generality in that respect). Today some artificial intelligence experts attempt to remedy this defect and return method to its former glory, by developing the information-theoretic counterpart to the sausage grinder in the form of computer programs.

In the 17th and 18th centuries, method supposedly streamlined inquiry not only by eliminating cognitive variation and the need for wit and imagination but also by eliminating any dependence upon luck. The very notion of *methodological luck* or *methodological risk* was an oxymoron.

For the whole point of method was to improve cognitive economy by supplanting the old history of sporadic discoveries made by luck with a new, 'masculine' history of systematic investigation routinely productive of discoveries.

Then, in the first half of the 19th century, a mighty change took place in the 'official' story of scientific method, as the hypothetico-deductive (H-D) method gained ascendancy over Baconian induction. The four main advantages of the H-D method are that (1) it permits us to propose deep theories that go behind the phenomena; (2) it can thrive in a sparse data environment, relative to induction; (3) it permits a division of labour between theory and experiment;[6] and (4) the method of hypothesis, rather like the old method of analysis in Greek mathematics, lets scientists use the assumed hypothesis in logical reasoning as if it were already established. It thereby primes the pump of scientific reasoning with virtual premises.

These benefits are purchased at some cost, however. For a hypothesis is really only a loan or promissory note that must be repaid through confirmation, else the enterprise fails. Whereas a conclusion derived from established knowledge can be detached and asserted, inferences involving hypothetical premises (or, indeed, any fallible claim) must be remembered and tracked. In this respect, hypotheses are like lies! In his *Notebook* for 1894, Mark Twain remarked, "If you tell the truth you don't have to remember anything."

Rather like capitalism, the H-D method permits, even encourages, risky entrepreneurship, and hence individual and group initiative and competition — and the associated private vices in science. We must expect most hypotheses to fail.[7] In most cases we hope only to learn enough from the exercise to launch a more successful, new venture, at lower startup cost. Indeed, scientists often introduce deliberately oversimplified hypotheses, in order that the data may speak more clearly. Such a 'failure' is often worth the cost, since we can learn from our mistakes.[8]

So in the era of H-D hegemony, ironically enough, the official method of science makes successful inquiry depend on luck! Since it is ultimately up to nature whether our hypotheses work, and since, at the frontier of research, we cannot reliably anticipate the order of nature, predictive success amounts to good luck. A successful hypothesis is good fortune indeed, while a fruitless hypothesis is bad luck.

An oft-forgotten consequence of the H-D method is that the success of science becomes highly contingent. Method cannot guarantee progress, unless we beg important questions by assuming in advance (a) that there are significant, lawful regularities to be found in nature and (b) that our method eventually will find them if applied with sufficient patience. Without such assumptions, we lose much of our former ability to explain the comparative success of science by direct appeal to method. This failure of method raises serious questions about what function it serves and about how methodological claims are to be justified. Yet the above assumptions were not widely scrutinized until the 20th century, e.g., by Popper (1934, 1972).

The direct, procedural cost of the H-D method is that the hypotheses themselves are no longer determined in advance by method, or rather, by method plus empirical factual input. Rather, to state the situation euphemistically, scientists are liberated from strict logic and are free to use rhetorical devices such as analogy and metaphor and, indeed, the resources of the free imagination. Good scientific work now requires a keen wit and originality. In this respect, H-D method is a sort of Romantic Reaction to Enlightenment methodology[9]. It places a premium on individual creativity. Today it is common for scientists to appeal to humanists by stressing the importance of 'the scientific imagination'.

A levelling of wits is precisely what the H-D methodologist does *not* want, for the method demands, as its input, in addition to a few facts, a multitude of novel hypotheses, most of which will fail. Method itself is now less constructive than before and works only as a filter, removing those hypotheses whose predictions fail. Accordingly, method proper now covers only a small part of scientific *activity*. For all scientists to think in the same, methodical way would reduce what is basically a massively parallel process of hypothesis production to a slow, dull, serial process. In the H-D regime, economy of research now depends on the very diversity that Descartes abhorred. In Romantic terms, it is a more organic economy in which, ironically, the abundant proliferation (of nature for the Romantics, of hypotheses for scientists) turns out to be economical. In terms of the more prosaic, financial-economic metaphor, since nothing can be established as the One Final Truth, then just as in the marketplace, where a clever inventor can carve out a niche by building a better mousetrap, a clever theorist can build an alternative hypothesis and hope to gain a 'market share' from the scientific community. Thus, on the H-D account, science must tolerate a certain amount of difference, even dissent, at least during the discovery/construction stage. This allowance for dissenting voices was the scientific counterpart to Tocqueville's and J. S. Mill's (1859) warnings against "the tyranny of democracy" and comfortable, dull consensus in the political sphere.

The upshot is that, by the old lights, H-D is in part a *non*-method; it is *anti*-method as far as the generation of novel hypotheses is concerned. It is method turned partly against itself. To state this point in a more familiar form: for H-D theorists, there is no *logic of discovery*, only a *logic of justification*. Or at least there can be no logic that justifies hypotheses in advance of testing. Hypotheses are risky gambits and can only be justified consequentially (not antecedently), after their proposal, by empirically testing the observably checkable consequences that we are able to derive.

Only in the 20th century did methodologists widely appreciate that there is also no general logical method enabling us to discover even the testable consequences of a hypothesis, not even when those consequences follow deductively from the hypothesis plus available auxiliary assumptions. As a result, the positivists, Popperians, and Bayesians contracted method even further, limiting it to the logical or probabilistic relations of the products of research and excluding the research process. For even the process of justification is shot through with discovery or search tasks. Oddly enough, their methodologies abhor discovery — and vice versa! On the dominant, 20th-century conception, method proper cannot thrive in creative contexts and certainly not near a frontier. And with this the social constructivists will agree. The more creativity, the more nonroutine construction, we find in scientific work, the less opportunity for general method to gain a foothold. Ironically, *scientific* method strictly so-called captures very little of scientific *practice*.

The H-D method, then, *allows* far more room for individual creativity than the inductive method, but without *engaging* such activity. The three primary loci of individual differences are: in the imaginative formulation of hypotheses, in the derivation of testable consequences, and in the ingenious design of novel experiments. (Recent writers would add the recognition of a good problem.) H-D method *allows*, even *requires* such creativity without, however, providing any resources to direct it. Insofar as it restricts method proper to method of justification, it still disallows methodological luck. Once we turn from the process of justification to the final product, the method is supposed to dictate the form and content of the answer. The logic of justification (theory of confirmation) is supposed to tell us whether and to which degree a given body of data confirms or disconfirms a hypothesis. Here there is no room for dissent. Although the final theory choice is underdetermined, rational persons cannot differ about degree of confirmation.[10]

We can now see that Bacon's inductive method combined logic of discovery and logic of justification into a single 'logic'. Correct method of discovery itself provided all the justification necessary — and possible. By contrast, the H-D division of labour separates discovery and justification into two components, then drops the discovery component as methodologically impossible — and unnecessary. Strong H-D theorists such as Popper went on to say that the actual, historical routes to discovery taken by important scientists could be of no epistemic interest.

Before proceeding with this thumbnail history of method, we should pause to ask the following question. How is such a change in official[11] method possible, if method is normative and self-certifying in the way that Bacon, Descartes, and others apparently claimed — and not itself a lucky product of consequential testing? While the H-D method is commonly seen as an advance on inductivism (and Cartesianism, etc.), this change is terribly disconcerting from the old point of view. First, how can an infallible method itself fail and be superseded by something else? Second, how can that something else be superior when, in an important respect, it is the very denial of method in the old sense? Third, from the new, more fallibilistic and luck-dependent point of view, the older methodology is actually irrational, because it cannot possibly achieve its stated goals or even make significant progress toward them.[12] Any change from a foundational method is pretty earthshaking, a sort of Platonic decline from the optimal. If you can't trust a foundational method, then what can you trust? The discovery of 'method' itself is beginning to look suspiciously like a great piece of H-D luck; moreover, Bacon's 'hypothesis' has now been refuted, or at least rejected on the ground of the greater *fertility* of the H-D method. Thus methodology as a whole becomes a matter of what in §5 I shall call *heuristic appraisal*, as in job searches.

Popper (1934, Ch. 2) considered methods to be mere conventions fixed by a consensus of the community, and thus changeable by that group, much as laws can be changed by politicians. As such, methods are social constructions; yet, oddly, he says that choice of "the rules of the game of science" is (again) to be based on judgments of relative fertility.

Since the 1960s, methodology as discussed by philosophers has become even looser. Putnam (1962, p. 216) denied that there is a logic of justification any more than a logic of discovery and observed that in science justification does not proceed linearly, from observation to theory, but in any direction that may be handy (cf. Batens 1992). In the wake of the new history of science, which challenged most of the methodological claims made by philosophers, the historical philosophers of science retreated from talking about a *logic* of scientific development to considering the overall *rationality* of science. The four leading proponents of this view were Lakatos (1970) with his methodology of scientific research programs; Toulmin (1972) with his evolutionary model of scientific rationality as adaptability to changing circumstances, as opposed to logicality within a fixed system of rules; Shapere (1984) with his historical chains-of-reasons; and Larry Laudan (1977) with his problem-solving model. Feyerabend eventually went further still to question the very rationality of science. In *Against Method* (1975) he suggested that some eras of science, including the present one, could use a good dose of irrationalist "medicine", in which scientists deliberately violate methodological rules. His hero was Galileo, who cleverly used rhetoric to bring an entire culture around to his new conception of scientific rationality. Feyerabend went far beyond those philosophers who relaxed 17th-century rigidity by acknowledging that rational persons may differ. In Feyerabend's grand vision, rationality itself was something that is created and destroyed, that comes into being and passes away through human effort. Rationalities themselves may differ!

In effect, Feyerabend extended the method of hypothesis to include all of science, including method and theory of rationality. Whereas the H-D method was anti-method only as far as denying a logic of discovery, Feyerabend went 'all the way', to a metamethodology that denied any possibility of an a priori, permanently fixed method.

Still more recently, Hacking (e.g., 1992), following Foucault and Crombie, has spoken of the emergence of culture-wide thought-styles or "styles of reasoning" that support new forms of discourse and practice that could not have made sense before. That there can be distinct styles of reasoning, in a logical as opposed to a rhetorical sense, was anathema to tradition. Hacking's most powerful example is the emergence of statistical-probabilistic modes of thinking in the 19th century. Before then, he says, most of the kinds of statistical and probabilistic statements we make today were neither true nor false, because the conditions for their assertion simply did not exist. Aristotle could not have made probabilistic claims in the modern sense. While thought styles liberate us from the idea that method is logically a priori, they do not liberate us from what we may term a *cultural* a priori. An individual who rejects the statistical-probabilistic thought style is not likely to get far in science.

Current science studies practitioners advocate a new *inertial principle*. Contrary to the old, idealist history of ideas and the teachings of philosophers, there is not one, self-evident system of logic or theory of rationality that exists eternally, whether recognized or not, like Plato's Forms. Rather, any system of reasoning, indeed, any stable practice whatever that makes a difference to human users, is a social construction that requires constant maintenance for its continued existence. It does not exist on its own, either in some abstract, logical realm or as immanent in nature. Moreover, the maintenance process is not fixed and automatic either, but dynamic and thus capable of altering the 'maintained' practice, over historical time. These points extend to the specific import of claims and their logical relations as well. These do not persist on their own, without a supporting style of reasoning.

3. *Evolutionary Naturalism versus Method of Discovery*

An additional attack on the possibility of a logic of discovery comes by way of the naturalistic turn in philosophy (Callebaut 1993) and its more explicit appreciation of the force of the Meno paradox at the frontier of research. "How is learning possible?" the paradox asks. For either we already know X, in which case we cannot now learn X, or else we are ignorant of X. But if we are genuinely ignorant of it, then how could we recognize X even should we stumble across it (dis-cover it) accidentally? Popper (1972) summed up the problem by noting that we cannot know now what we shall only know later. Popper and Campbell (1974) linked the point to what Campbell called *evolutionary epistemology*. Once we become thoroughgoing, naturalistic Darwinians and eliminate all traces of clairvoyance or foreknowledge, then at the frontier of inquiry we can only proceed blindly rather than intelligently. Absent any prior intelligence or design, all knowledge, indeed, all human creativity, can be produced only by a Darwinian process of blind variation plus selective retention (BV+SR).

So the BV+SR account replaces the scientific method by a blind, evolutionary process. Just as in biological evolution, variation is crucial. Were there no variation, there could be no evolution, and hence no scientific advance. Too much consensus or other uniformity at this stage would soon produce sterility. And just as in biological evolution, creativity tends to be the product of zillions of small variation-insights rather than one big insight (Gruber 1980, p. 128). The world at the frontier is a "bloomin', buzzin' confusion" that is endlessly rich in information. Cognitive economy demands that we filter or funnel or screen or chunk this information into manageable, intelligible pieces. However, there are any number of ways in which we might try to do this. Since we do not know for sure which ways will turn out to be fruitfully projectible to new cases, we can only try as many as possible and hope for some lucky strikes.

Once methodology shuns its pre-evolutionary doctrines in favour of BV+SR, we get an economy of research quite opposite that of the rigid logic of discovery considered above. The latter dictates an efficient order of discovery that everyone should follow, in order to avoid wasted effort, but that method obviously presupposes foresight. In effect, the method must already know that which we are seeking, indeed, must implicitly contain all potential *discoveries*, just as Meno's slave already possesses implicit knowledge of the Forms. Knowing the method, then, we implicitly know everything that the method can eventually produce when combined with the appropriate empirical data.[13]

The evolutionary account rejects all such accounts as impossible, on Meno-like grounds. True, the naturalistic turn permits some implicit knowledge relevant to human survival, as a product of evolution, but it disallows Plato's Meno solution — that we already harbour esoteric knowledge of the structure of the universe — the very thing that science seeks.[14]

If the evolutionary account is correct, then only an H-D, *consequentialist* methodology seems possible. In other words, something like the H-D method must be correct, at least for theoretical work. There can be no logic of discovery, at best a logic of justification applied to the plethora of new proposals (hypotheses, models, techniques, etc.) poured onto the table for examination. Justification must be *post hoc*, literally *post factum*.

Such a conclusion is too hasty. Campbell himself acknowledged that the heuristic routes to discovery remain possible, so long as they themselves are the products of previous BV+SRs. For example, we can project what we already know about constraints on molecular structure into new domains and thereby cut down the size of the effectively blind search space. We can employ heuristic appraisal (see below) to concentrate our efforts near those trials that look more promising than others. I myself defend a *multi-pass* conception of science that takes us quite far from H-D method and its purely consequentialist mode of justification (Nickles (1987a, 1988b, 1992, 1997a). The H-D model is too simple and linear, too single-pass: A hypothesis is proposed, it is tested, confirmed, and accepted, period. Innovation stops here. In real science a successful claim or technique itself becomes the focus of intense interest, rather like a novel phenomenon. It is problematized. A search space is constructed around it. Scientists 'wiggle' (vary) it and tinker with it in various ways until they attain a better understanding of the structure of that space. This enables them to streamline, generalize, and even to methodize the result by incorporating it into a new or revised research procedure. This process sometimes goes through several stages of refinement, in which the noise, blind alleys, unhelpful variants, logical gaps, and other deficiencies are eliminated.

Note that, on my view, a major breakthrough can rarely be explained in terms of an already extant method. Rather, the new method is a major part of the breakthrough, a methodization or proceduralization of substantive results. Scientific methods are nearly always retrospectively cleaned-up and expanded *discoveries* rather than the actual procedures used by the scientists in the forward mode. While discovery logics do exist (Nickles 1990), they tend to be rather local, routine procedures that do not, alone, produce breakthroughs. Rather, they are the products of previous breakthroughs.

For economic reasons, both cognitive and material, it is frequently the same people or the same research groups who refine the original claims, techniques, and derivations. For they are usually the ones with continued access to the needed resources and also those who possess the 'insider' knowledge of how the work can be streamlined and extended. Thus it is not surprising that different people and different laboratories come to specialize in particular kinds of research, developing rather distinctive techniques and styles of research. Familiar examples include Newton's optical investigations, Faraday on electromagnetic interactions (Gooding 1990), the T. H. Morgan research group's work on the genetics of fruit flies at Columbia, and the later work of the Phage Group on bacteriophages. Galison (1987) describes several instances of competing experimental schools (and their styles of experiment and argumentation) in physics, e.g., the high-energy physicists who searched for a single, 'golden event' captured in a bubble-chamber photograph versus those who trusted conclusions based on statistical analysis of a large number of events based on computer simulations.

Once someone develops technical skills, it would be a waste of resources, of human capital, not to use them. Such skills (even those learned in the military) are career investments and offer those persons the easiest path to scientifically valuable work. Long-term, intensive work on a set of problems or tasks produces a perceptual set in terms of which we interpret as many things as possible. We find those things most intelligible that we are able to engage in this manner,[15] things that we can grab onto and manipulate, so to speak. For evolutionary reasons, we human beings, like other animals, are endowed with a drive to make the world as intelligible as possible, a passion to make sense of things, a rage for reason (in this broad sense). Our previous work cognitively primes us in certain ways and denatures us in others. This is basically Kuhn's point about the role of exemplars in normal science (cf. Margolis 1987). Training and later work shapes our minds to fit certain aspects of the world and not others. (Trouble comes when new developments devalue one group's areas of expertise and valorize the new, as in a Kuhnian paradigm battle.) Fuller notes that Kuhn virtually replaced the idea that science has a distinctive method by the idea that normal science is characterized by a single, distinctive mindset (and hence of series of different mindsets over time, as paradigms change). So science has a distinctive historical structure rather than a distinctive logical structure. Contrary to the tenor of BV+SR, Kuhn (1977, Ch. 9), on the "essential tension" between tradition and innovation, contended that creativity in science, unlike the arts, is convergent (constricting diversity) and uncritical rather than divergent and critical. Feyerabend was Kuhn's most vociferous critic on this score, complaining that Kuhnian normal science is nearly as rigid and lacking in tolerance of variety as the general methodologies of yore.

We need competing, multiple mindsets, he said. And citing Mill (1859) on the intellectual value of freedom of speech, Feyerabend contended that *critical* opposition is necessary to the full development of any research (or political) program.

An individual's or group's toolkit of skills and capacities amounts to a potentiality, a *potential field*, *opportunity field*, or *opportunity profile*.[16] These potential fields will naturally differ from person to person and from group to group. A person looking for opportunities needs to match her own potentialities with those of available opportunities.

Another reason why contemporary scientific communities *must* allow considerable variation in opportunity profile is the cognitive overload resulting from the knowledge explosion. There is far too much knowledge for one person to master. Contrary to the models of Bacon ("I take all of knowledge as my province"), Descartes (a Faustian character), and Whewell ("Science was his forte, omniscience his foible"), no one can know everything. Campbell (1977, pp. 437ff) speaks of a "fish-scale" model of knowledge here. Like a fish scale, each individual's knowledge (including practical competence) overlaps that of several other people without being identical with any.

4. *When Is Discovery?*

Most philosophers writing on discovery in the decade around 1980 treated it as the initial stage of research, a breakthrough that was followed by some type of justification process that preserved the nature of the discovery. Although they used terms such as 'social construction' and 'negotiation' in place of the sometimes question-begging[17] 'discovery', sociologists of science made much the same mistake of looking at the early phases of scientific research, especially laboratory research, and concluding that this initial effort forever stamped the character of the products of that labour. This 'inertial' conception of discovery was still too much in the *Gestalt switch* and *Aha! Erlebnis* traditions. For in many cases, most of the content or meaning of a discovery accrues to it long after it is initially published and even accepted by the community. I have argued this point elsewhere (Nickles 1988b, 1992), my favourite example being the rederivations of Planck's black-body radiation law by Debye, Einstein, and others in ever more illuminating ways over the three decades after 1900. The later refinements, rederivations, and connections to other results that I mentioned under the rubric "multi-pass" in §2 rarely leave a discovery claim untouched. The deeper understanding achieved by later work often transforms the original claim almost beyond recognition. For a long time, Planck rejected the very claims that others were crediting to him (Kuhn 1978).

If we look at justification and decision-making in science as a process of inquiry rather than as a finished logical structure, we come to appreciate how inextricably intertwined justification and discovery really are. In general, they cannot be separated into distinct logical or temporal stages of inquiry, as simpler versions of the H-D method imply. In the next section I shall show that discovery and justification are even more inextricable than this, in the ongoing process that I call *heuristic appraisal*. But first I wish to extend the point of the previous paragraph, by asking what theories are.

Despite all the recent work on experiment and the specialized work in philosophy of physics, biology, and cognitive science, philosophy of science today remains pretty theory-centred. For good or ill, 'theory' (or 'hypothesis') remains the central unit of (and for) philosophical analysis. For theories — the bigger the better — are supposed to be the primary carriers of scientific knowledge and the official scientific pronouncements on what the world is like. Hence our consuming interest in relativity theory, quantum theory, evolutionary theory, and the rules-and-representations 'theory' of the mind as like a digital computer. After all, isn't it the very point of philosophy — and of science — to generalize as much as possible, to 'chunk' or 'lump' things so as to achieve a general picture of the universe that is also economical? Furthermore, until fairly recently, most philosophers of science made a clean distinction between theories and their applications in explanations, predictions, etc. An application was considered interesting only if it bore on the logical or epistemic status of the theory. A novel prediction expanded the *known* empirical content of a theory.

This emphasis on comprehensive theories was challenged already by Kuhn's *Structure*, especially the postscript to the 1970 edition, where Kuhn likened the laws of mechanics to spare schemata, whose real content entered 'from the bottom', through knowledge of specific force laws and how to fit them to the phenomena. Kuhn stressed the role of *exemplars* — exemplary problems (or puzzles) and their solutions in normal science. Normal research is puzzle solving, he said, and this proceeds by directly modelling one's current puzzle on one or more, sufficiently similar, exemplary problems-plus-solutions already at hand. The most basic exemplars in classical mechanics include the simple machines, projectile motion, the Kepler problem, and the simple harmonic oscillator. A student who had memorized all of the theories and laws from the textbook did not really know much science, Kuhn pointed out. Genuine scientific knowledge is more practical, a matter of know-*how*, of being able to solve problems, thus demonstrating knowledge of what to do with exemplars (Nickles 1988a, 1997b).

The 'semantic conception of theories' made its appearance in the 1950s and '60s in the model-theoretic work of Evert Beth and set-theoretical work of Patrick Suppes, and has been further developed since, sometimes along Kuhnian lines. Today it is the major alternative to the positivist view of theories as empirically interpreted axiomatic calculi. Briefly stated, the semantic view identifies the theory directly with its intended models rather than with propositional generalizations stated in some language. Roughly speaking, this move reverses the relation between theory and (privileged) applications.[18] Novel applications now become epistemically interesting. The theory now *is* the (somewhat idealized) exemplary applications, or models. Thus something like Kuhnian exemplars become part and parcel of the theory.

Nancy Cartwright's book, *How the Laws of Physics Lie* (1983), highlights and extends this broadly Kuhnian reversal (although she expresses reservations about the semantic view of theories). Even Kuhn, it might be said, treated paradigm-articulation puzzles in an overly routine way as mere mop-up work following the 'real' discovery. On Cartwright's account, universal theories are schematic oversimplifications that are false and, by themselves, do not explain much. Scientific knowledge and truth lie closer to the natural phenomena in the "simulacrum models" that fit our scientific knowledge to the known phenomena. More scientific effort is devoted to tinkering with such models than to constructing and testing grand theories. And most scientific knowledge is embodied in the practice of successful applications. While still necessarily idealized in some ways, the applications achieved by the use of simulacrum models are far more sensitive to the concrete details of particular, local problems.

Now what does all this have to do with discovery? The point here is an extension of my previous one — that much or most of the content of discoveries typically enters *after* the initial stage, even after the solution or theory is proposed and accepted by the community. If Cartwright is correct, then every further, nonroutine application of a theory adds to its content (and not just to its known content). Similarly, mine is a pretty constructivist position on the content of 'discoveries'.[19] The earlier point about the individual initiative and imagination necessary even to work out test predictions obviously holds also for this wider class of applications.

In a sense Kuhn agrees that theories or paradigms take a long time to build. They are not discovered all at once, and many people and many varieties of work are involved in the ongoing discovery and articulation of a paradigm. A paradigm is the product of a community of individuals distributed over various subspecialties, experimental and theoretical. Every application of an exemplar to construct a new exemplar provides a connection that tends to enrich the content of the original exemplar. (Kuhn could have placed more emphasis on this point.) We must not see exemplars as fixed 'atoms' or 'jewels' of scientific knowledge. They, too, have a history. Their character changes, sometimes notably, over time. Innovation continues, however undramatically.

However, Kuhn remained too theory-centred, or rather paradigm-centred, for Cartwright. She also goes beyond Kuhn in contending that the different applications, for different purposes, need not cohere harmoniously. They may even involve incompatible assumptions, e.g., different approximation techniques. Here then we have something of a return to a Ptolemaic versus a Copernican conception of science. Ptolemy did not hesitate to employ incompatible techniques to determine, say, the position and the size of the moon. Copernicus was scandalized by this and sought one, smooth, comprehensive world picture, one that would work uniformly in all applications. Thus was born the modern, theory-centred conception of the aims of science: to produce a single, accurate, monolithic representation, in a single mathematical language, of the universe (or scientific domain) as a whole, a kind of spectatorial attempt to tame nature by aestheticizing it.[20]

If Cartwright is correct, inconsistency — the worst possible sin for positivists — has been overrated. Two further reasons for this conclusion are: first, the applications she discusses are local and quite task specific, and this pragmatic relativity to task and context renders the incompatibilities relatively harmless (Batens 1992). Second, few philosophers today (and fewer science studies scholars) are committed to the first-order predicate calculus, with its material conditional, as the *lingua franca* of science studies.

The inconsistency point represents a rather extreme example of the current emphasis by some philosophers on the diversity or disunity of science (Dupré 1993, Galison and Stump 1996), in contrast to the received view of the methodological unity of science and its possible doctrinal unity in terms of reduction to a single, final, physical theory of everything.

I conclude this section by pointing out that much of the current work on scientific *experimental* practices extends the interest in discovery and invention and construction to the experimental level. In an earlier day, it was common to speak of 'observational evidence' or 'data' as simply given, as a matter of opening one's eyes and observing, or reading the numbers off the instruments. The claim of historical philosophers, that empirical data is theory laden, problematized this naive view somewhat; but it was not until the work of 'laboratory life', social-constructivists such as Latour and Woolgar (1979) and Knorr-Cetina (1981) and of philosophers such as Hacking (1983) and Ackermann (1985) that processes of data generation and analysis have been examined in detail. The discrimination of robust signals from background noise and the reliable production of phenomena to be explained and manipulated are complex and diverse tasks across the various sciences. Ironically, what was once taken as the direct voice of nature is now seen as the product of much data massaging. What was thought to be given turns out to be (largely) generated. And where there is innovative construction, general method is pretty useless.

5. *Heuristic Appraisal*

Since the 1970s, it has been fashionable to divide all of research, like Gaul, into three parts: discovery, final justification, and an intervening stage variously termed "pursuit", "preliminary evaluation", and "heuristic appraisal" (HA). I shall call it HA. Final justification is epistemic justification, the product of epistemic appraisal (EA). On the standard view, HA tells us which of the novel theoretical ideas produced by the 'discovery' phase are worth pursuing, that is, worth testing and developing toward the final goal of EA. At that point, EA completely supersedes HA. The heuristic scaffolding comes down, so to speak, as the finished product is presented to the world.

This tripartite characterization is an unfairly narrow, essentialistic construal of HA, for HA is not a well-defined set of responses to a homogeneous domain of problems. HA is neither a natural nor an artificial kind. Accordingly, I employ 'heuristic appraisal' as an umbrella term, as a catch-all name, for all manner of assessments of the comparative prospects, the promise, the likely fertility, the opportunity profile of just about anything in science — a problem, piece of equipment, procedure or technique, research design, grant proposal, model, hypothesis, explanation, proposed conference, institution, science policy, person or research team, etc. It is the collective HA by the relevant scientific communities that defines the frontier of research and thus determines the overall direction of research in the immediate future, not to mention how individuals and groups are going to invest their lives. HA is the central activity of grants committees, journal editors, and referees as well as lab directors. It is the means by which scientists make decisions about which problems to work on, which procedures to use in this case, which opportunities to seize and which to by-pass. Obviously, HA spans a wide range of things and is by no means confined to what philosophers and historians used to call internal, technical considerations as opposed to external factors.

As the most pervasive form of justification in science, much hangs on HA, and it must be done carefully and responsibly. Yet by their very nature, HA tasks are among the most difficult to address. HA is an especially risky business because the reliability of the advice it offers depends on forecasting the future direction of knowledge increase — on forecasting future discovery. HA tasks seem impossible because they straddle the Meno boundary itself, the current frontier of knowledge. Since, by definition, we rarely have enough knowledge for EA at the frontier, the 'ethics of belief' or 'ethics of practice' there differs from that of post-frontier refinement. Thus HA appears to violate the virtual tautology of Popper and Campbell, that we cannot know now what we shall only know later. Yet what looks logically impossible is a necessary task for scientists and administrators! The positivists and Popper distinguished legitimate prediction (logically conditional deduction from a hypothesis) from the dubious enterprise of forecasting. Unfortunately, much HA is perilously close to forecasting, for its predictions typically are based on nothing so solid as an established hypothesis. No one can know for sure what the eventual outcome will be, for that is an empirical question, a matter of *fortuna*.

Now to have a fairly definite frontier at all, there must be a good deal of agreement of HA judgments (as well as of EA judgments) and of corresponding practices, but even the most compact and mature sciences must allow some room for divergence on this score. This is confirmed by studies of funding agencies, which show that who is awarded grants in a given round is a highly contingent affair (partly a matter of luck), depending heavily on the particular 'draw' of peer reviewers and funding panel. Although individual researchers may feel that they have been unfairly treated by the grants process, it would be impossible (not to mention hopelessly wasteful) to fund every applicant; and it would be foolish, given the uncertainty and risk of the judgments, for the scientific community to risk everything on a single research program — or a single, tight, scientific method!

In short, HA has one leg in discovery and one in justification, with much of the weight on the discovery leg. It attempts to convert hindsight (experience of previous cases) into (a fallible, limited) foresight, or at least to convert past successes into heuristics for future research. In this most important area of scientific judgment, we find that discovery and justification issues are inextricable.

For this reason, too, we should not expect there to exist a precise, uniform *method* of HA. Rather, HA is likely to issue in particular judgments (usually practical decisions or simply practical responses) informed by training and experience, sometimes 'case-based' judgments backed by citation of relevantly similar cases (see §6). Knowledge of past cases can at least inform us what *might* or might not happen, in a more realistic way than logic can. Obviously, we here again have room for a good deal of individual variation. People vary by background and experience as well as in their aversion to risk. Here again, rational persons may differ. As Lakatos (1970) pointed out, it is not irrational to play a risky game as long as one is aware of the risks.

Now assessment implies the use of some sort of *accounting system* (though it does not imply the use of a rule-based procedure of making individual judgments; the accounting system may be loose and informal). Logic of justification is one sort of accounting system, one designed to filter out detectable errors as well as to check for the presence of desired epistemic features, mainly interesting truth or truthlikeness. Its function is epistemic appraisal (EA). HA is another type of accounting system (actually a whole family of them), for the different purpose of assessing opportunities and opportunity costs.

HA and EA operate in modally distinct domains. To put the difference in academic terms, EA assesses actual achievement, whereas HA assesses aptitude. EA affirms that something or other was done or 'undone', that it succeeded or failed under empirical test or argument; whereas HA is more concerned with whether something is do-*able*, whether it is possible, whether something is a genuine opportunity, or a more inviting opportunity than something else. HA is concerned with possibility rather than actuality. Despite (or because of) this difference, there is a tension between HA and EA. HA devalues exhaustive searches through huge, abstract, possibility spaces seemingly required by those versions of EA that search for 'the one true theory', in favour of estimating the 'reserves' of more limited possibility spaces constructed around actual achievements. Yet this last phrase suggests that HA does need anchoring in a feasible EA.

Here a troublesome ambiguity comes to light. Is EA concerned only with assessment of past performance, or is it also concerned with reliable projectibility of past successes to future applications? Understood in one way, EA provides a retrospective summary of past performance but provides no appraisal or advice for the future.[21] Understood in another way, EA overlaps HA in estimating future projectibility, at least the inductive extrapolation of old results if not an extension to analogous cases. Epistemologists and philosophers of science often have conflated these questions (as Popper did in his attempts to skirt problems of induction). A separate question is whether an inflationary (undeflated) account of EA — that is, a *general theory of confirmation* — is even possible. Scientists themselves make reasonable local judgments about future applicability; but do these local judgments themselves fall into equally reasonable, projectible, rule-like patterns that can be explained and normatively corrected by a transcendent philosophical logic of confirmation? Is science itself a sufficiently homogeneous domain to admit of general rules or to call for a general theory? To characterize science in terms of EA alone (without HA) is to assume an affirmative answer to these questions.

To further appreciate this difficulty over the projectibility of EA, consider Kuhn's (1962) point that we can always find a set of rules that more-or-less fit past scientific practice, without our having any reason to believe that these rules will hold in the future, or even that the past practice occurred *because of* the rules. If that is the status of logic of justification, then there is no genuine logic of justification anymore than a logic of discovery. To add a reflexive point: insofar as the new experimentalists (Hacking, Cartwright, Rouse *et al.*) are correct that our most reliable knowledge resides in domains that we have experimentally manipulated, reconstructed, purified, isolated, etc., then a reliable, general methodology of science would have to be the product of similar experimental manipulation of *scientific* activities by methodologists — isolating this sort of scientific practice, wiggling these posited methodological variables to determine the effects, and so on. History, sociology, and anthropology of science can provide limited evidence of the needed kind, as Feyerabend noted; but how a simple logic of confirmation could achieve normative status on such a slender basis is difficult to see.

Another reason for doubting the adequacy of general rules of confirmation to account for scientific judgment is that how well a result is justified depends very much on the context and on the specific *purpose* to which we apply it. Furthermore, HA, as I construe it, includes in its accounting individual and social cognitive economies in ways that standard versions of EA do not.[22] It is natural to combine the purpose and economy considerations. Economy of research suggests that we satisfice (Simon 1953), that, especially at the frontier but also in most applications elsewhere, we require no more justification than necessary to get on with the business at hand, including foreseeable applications by other groups. The HA question is not the EA, semantic-epistemological, 'product' question, "Is it true?", but the pragmatic, 'process' question, "Will it work here?" However, what is good enough now, at the present frontier, for presently foreseeable purposes, may not be good enough later, for others. So projection of past success onto the future is risky for quite specific, practical reasons as well as for the more familiar and more global Humean reasons. Confirmation theories in the traditional mode tend to omit the pragmatic and economic variables.

HA canvasses possible objections, not in the Cartesian EA manner of trying to anticipate all possible criticisms but in the more local sense of addressing those objections that are likely to be raised, difficulties specific to this sort of enterprise, difficulties that therefore have some purchase rather than general problems that undercut everything at once in the way that global doubts do.[23] No one wants to look stupid to the next generation of graduate students.

A further reason for taking HA seriously and for rejecting the view that EA is assessment enough is that *rhetoric* plays a major role in actual scientific judgment but is allowed no role in traditional confirmation theory. For example, rhetoric is as important as logic for scientific assessments of future applicability. No strict logic or set of conventional rules of confirmation could fit all work at the frontier, where meanings and practices are still fluid. Deciding when and to what extent projection to specific future practices is warranted is the HA side of justification, one often based on judgments of *similarity* of past and present cases rather than on general rules of acceptance. Rhetoric is surely a better indicator of the conceptual and experimental growth points of science — the frontier — than is logic, which presupposes an already formed, stable, clear terminology.

It is worth noting that rhetoric plays a double role in HA. First, simile (similarity), analogy, metaphor and other rhetorical tropes are crucial to heuristic judgments. Second, HA must persuade. It must persuade committees to fund or not to fund; it must persuade scientists that a task is do-able (or not), that it is worth doing, that it is more worthy of commitment than competing projects; and so on. HA must instill optimism (or pessimism), even enthusiasm insufficient for commitment. This is one role that Kuhn assigned to his paradigms: they 'guarantee' that puzzles are solvable within the paradigmatic framework. That is what promise in the robust (emotional as well as intellectual) sense amounts to, engendering confidence that the goal can be achieved, and in this way rather than that.

EA alone, even if considered a process rather than a product, does not help us understand very much about science. EA leaves out too much. For the later positivists and Popper, theory of justification was supposed to provide a nearly complete account of the growth of scientific knowledge, but we now appreciate how much their accounting schemes leave out.[24] A major difficulty is that an interesting general methodology asserts or entails the strong methodological unity of science, and many commentators now believe that consequence to be false. At any rate, a general methodology must ignore the important differences among and within the various sciences. Given the enormity of these differences, a general methodology will be too vapid to be very interesting. Ironically, general methodology violates its own injunction to consider only interesting knowledge claims. It is informative only to the extent that it can sharply demarcate the sciences from other forms of inquiry. (We already have noted that it cannot provide a convincing explanation of scientific progress.) If we are interested in describing, explaining, or regulating the differences, the diversity, that we find in the sciences, then a general methodology is completely useless; for, being global, it applies to all equally and thus can get no purchase on the differences.

Traditional EA deliberately omits the 'external' factors that often determine the do-ability of scientific work (e.g., availability of rare samples or new software, new funding sources, a new enthusiasm of the lab director). Clearly, most of the economy of research, including many of the key 'growth factors', is external to such a narrow point of view.[25] Shifting our focus from final product to process challenges the internal/external distinction. Yet another type of omission from traditional accounting schemes is the value of nonpropositional achievements *internal* to science (e.g., the development of a new experimental technique or piece of equipment, or mode of visual display of information).

The overall point is that 20th-century philosophy of science did not possess the resources to answer even all of the questions that it itself raised about the growth of scientific knowledge. EA either ignores pragmatic, economical, and rhetorical issues or conflates them with traditional semantic and epistemic concerns. Proper emphasis on HA goes some way toward remedying this deficiency. Much of the disagreement between philosophers and sociologists and among the various factions of the new science studies has been over which kinds of accounting systems are more appropriate for which purposes. Often, one HA scheme will conflict with others.

So HA is much wider than EA as standardly conceived. HA also addresses the motivational side of inquiry (as Merton's social norms did but confirmation theory did not). To be effective in actual human practice, 'reasons' must be embodied so as to have some causal purchase. HA can include all those factors that enter into rational deliberation about life decisions, personal commitments, and future work. Actions that are eminently rational in the EA accounting may be irrational in an HA accounting, and vice versa. For example, standard EA enjoins scientists to replicate experimental findings, but HA counsels that simple replication will advance neither one's career nor the standing of one's research group. If any sort of check is done, it will most likely be a different sort of experiment (Collins 1984). So once again there is a premium on variety.

Objection: "In this case the variety will be epistemically relevant as well, since agreement of distinct procedures yields robustness. There is no conflict between HA and EA, between the logic of individual rationality and the rationality of science itself. On the contrary, we get EA simply by applying stricter, more internal constraints to HA. Indeed, as epistemologists we *should* impose the stricter conditions, thus collapsing HA into EA and rendering otiose a distinct HA when epistemological questions are at stake."

A full response to the objection, with illustrative examples, is impossible here. I do not deny that strict EA is appropriate to certain, narrowly-defined epistemological contexts, nor that it informs HA. I do deny that HA is simply a more relaxed version of EA, a view which conflates the kinds of accounting systems I have tried to distinguish. Consider also the following points. (1) True claims do not necessarily have either higher utility or more fertility than false claims, nor is utility or fertility a reliable mark of truth. (2) When realtime and other resource constraints are added to the logical constraints of the philosophers, we can get direct conflict between pragmatic and spectator-theoretical accounting systems (Rescher 1977, Ch. 4). (3) Cartwright (1983) turns upside down much of standard confirmation theory and the strategy of inference to the best explanation. That a general theory has widespread explanatory success, or what Whewell called a "consilience of inductions", argues for its *falsity*, she contends, not its truth! Producing practical, causal explanations and applications is a very different scientific activity than constructing general, theoretical representations of the world, and there is an unrelieved tension between causal explanation of experimentally controllable processes and theoretical explanation. Theoretical explanatory success, she says, is no guide to the truth (p. 4).

(4) Even *interesting* true theories may not be as fruitful, may not furnish the opportunity for further work, that deliberately oversimplified models or faulty research projects do. For example, the former may involve mathematical functions that are too difficult to calculate. Again, HA asks not "Is it true?" but "Is it new?" "Is it something we can do?" "Will it work in this case?" "Will it get us past this pothole on the road

to inquiry?" Traditional EA conflates semantic with pragmatic questions and simply assumes, despite its fallibilism, that truth rather than workability is what drives inquiry. It reverses the priority of process and product and collapses HA into EA. The result of whiggishly understanding the process (in an idealized, rationally reconstructed form, insofar as process matters at all) in terms of the alleged product is a distorted conception of both process *and* product.

(5) A related point is that EA is a 'logical' accounting system that, unlike HA, does not take into account the actual cost of a decision or practice, the 'externalities', so to speak. E.g., Popper simply assumes that it is easy to think up any number of interesting hypotheses, as the work of the 'free' imagination.[26] And as far as the logic cares, further experimental testing and further theoretical tinkering are also cost-free. Popper (1934, Ch. 2) also says that no claim that has proved its mettle can be dropped from science without explicit refutation, and he implies that every moderately interesting and testable claim should be tested. Unfortunately, such a 'logic of scientific discovery' cannot be materially embodied or causally realized in scientific practice as we know it.[27] That would be economically impossible, given the fluency with which Popperian scientists can produce interesting hypotheses. In actual practice (one could argue), many claims are dropped simply because they attract no one's attention or because something with a better opportunity profile is available.[28] Why waste effort on claims that no one sees any great use for? Here an evolutionary model again comes to mind. Variants often die out not by being killed by a competitor but simply because they fail to reproduce in sufficient numbers. And we again encounter the new 'inertial principle' that reverses the history-of-ideas or logic-of-science principle that Popper maintains. New ideas and procedures do not remain 'on the table' until refuted. Rather, it requires active effort to keep them on the table (or on the front or even back burner). They soon disappear unless they become embodied in scientific practice. Popper, in effect, rejected this alternative, on normative grounds. However, radical logical proposals to reform scientific practice are every bit as suspect as radical political proposals (e.g., the French Revolution) to reform society and to change human nature. It is odd that Popper rejected the latter while embracing the former.

Searching only for true theories, or at least theories that have not been disconfirmed under severe testing, is a *maximizing* strategy typical of philosophers, who repeatedly launch searches for 'the best of all Xs', the *summum bonum* of its type. For philosophers, such searches may seem cost free, but in the real world they are so expensive that maximizing is often not possible (Simon 1953). By contrast, as noted above, pragmatists with a robust conception of HA view much scientific work as satisficing. One way to state Peirce's complaint against Descartes's economy of science is that a foundational epistemology is maximizing rather than satisficing and, as such, is not just unnecessary to progress but impossible for it, since it actually blocks the road to inquiry.

In the spirit of Cartwright's reversals, should we go so far as to say that EA is only a handmaiden to HA, rather than vice versa? Decades ago, Simon and colleagues were already replacing logic of discovery by heuristics of discovery. Does his concept of satisficing similarly supplant logic of justification? To a large extent, yes. Wimsatt (forthcoming) has long contended that "science is heuristics, all the way down". I should say, more guardedly, that EA is often more important for its contribution to HA than as a final product of research. Many truth claims in science are more important to fertility assessment and to defining the research frontier than as an addition to reliable human knowledge. Their importance is more heuristic than epistemic. Recall the role of successful predictions in Lakatos's methodology of scientific research programs. There is an ongoing controversy about the epistemic importance of novel prediction. I claim that novel predictions carry no special epistemic weight, no special EA weight; but they do carry special HA weight, as indicators of fertility, of new phenomena to study, new problems to solve, new opportunities for interesting scientific work (Nickles 1987b).

The last thirty or forty years of science studies have shown how distorting it can be to study abstract logical characterizations of science absent concerns about realtime and real-world implementation. Most methodological schemes proposed by philosophers have turned out to be hopelessly unrealistic for anything recognizable as science as we know it. Human beings, institutions, and communities are more than reifications of abstract ideas. Ideas without an appropriate 'causal backing' (as we might call it) go nowhere. The more radical, naturalistic critics of the received methodology would reverse even the status of causes and ideas, or 'reasons', denying that the ideas or 'theoretical-representational content' should be central to our accounts of science at all.

6. *Is Science Rational?*

Several of the above points show why HA is relevant to the question of the rationality of science. In this section I make my position more explicit. First, I take a deflationary line on rationality. There is no overarching Rationality of Science anymore than there is a General Method of Science. The domain of choices is too heterogeneous for that. Science as a whole seems no more or less rational than many other enterprises, e.g., law, industry, higher education. It is not even clear what it means to ask about science's rationality as a whole. Nonetheless, there is a lot of rationality in a quite ordinary sense in the more local judgments and decisions of HA (Fuller 1989). In many cases HA involves calculating a kind of return on investment. Sociologists and philosophers have proposed various accounting systems to explain patterns of decisions. And just as in the business world, overall trends are determined, not by a central planning agency, not by a master method, nor by Hegelian Reason working itself out, but simply as the unintended by-product of zillions of local decisions.[29] Methodologists are often rather like economic experts commenting on the stock market, reacting to every significant swing with a battery of mostly circular, rationalistic 'explanations'.

Second, I agree with Husain Sarkar (1983) on group versus individual rationality. At that time Sarkar was one of the few philosophers to address the problem of group rationality. His main point was that, given the uncertainties of research at the frontier, it is not rational to put all our eggs into one basket. Group rationality demands that there be variation over the individual members and subgroups. Science should have a broad investment portfolio. This point of view is supported by several of our considerations above: (a) The blind-variation evolutionary model applies, by default, where heuristic knowledge runs out. (b) Too much emphasis on one option is unreasonable when heuristically promising alternatives exist. (c) Differences in scientific expertise, including both natural differences in aptitude and differences due to problems of cognitive overload (the fish-scale model), constitute a resource the wise use of which requires a variety of projects to run in parallel. (d) Besides, we possess no general method or accounting system that would provide for a centralized rationality. There are many reasons for this. E.g., most methods are not neutral (and hence not general) but are laden with empirical content. And risk is essential to scientific practice, as we have seen, yet no viable decision theory dictates that every scientist can and must operate at the same level of risk. That, too, would be a bad thing in any case. My principal uneasiness with Sarkar is that his account is theory-centred and also simply assumes the rationality of science in a strong sense. In fact, he makes the rationality of science an a priori constraint on any account of science rather than a possible conclusion. But much of what he says can easily be adapted to my emphasis on HA.

Third, rhetoric is as important as logic to the rationality of scientific work. Rhetoric is essential to HA but is officially excluded from traditional logic of confirmation.

Fourth, HA is necessary to capture what rationality there is in the ongoing process of science as opposed to the narrow, retrospective rationality of epistemic acceptance treated by standard confirmation theory. And the viability of HA, unlike that of EA, does not depend on maintaining an absolute (purpose-free) epistemic internal/external distinction. Contrary to the old, idealist, history of ideas tradition, contemporary science studies questions the relevance of abstract logical systems that lack specific causal backing. Continued use of the term 'pure science' (like 'free inquiry') misleadingly suggests that 'real science', as distinct from 'applied science', is conducted under internalistic rules that define an economics-free zone.

Fifth, HA enables us to make more sense of appeals to concrete *historical precedents* than do alternative accounts of scientific justification. This is important because HA is a form of reasoning based on casuistry, on consideration of precedents, rather than rules (cf. Jonsen and Toulmin 1988). The prevailing view seems to be that historical cases either have a purely illustrative value or else they must be used, in a theory-centric manner, as evidence (empirical data) for or against some proposed methodological rule. Moreover, research in psychology and artificial intelligence (AI) increasingly suggests that a good deal of intelligent behaviour is not, or need not be, guided by rules. In many cases we are hard pressed to find rules that fit the behaviour, let alone direct it in a manner projectible onto the future. The short history of AI is instructive here. Early, power-based, general problem-solving programs in AI (notably, Newell and Simon's General Problem Solver) quickly gave way to "expert systems" (which attempted with very limited success to extract problem-solving rules from the experts), which are in turn now yielding to "case-based systems" (in which new problems and their solutions are modeled on problems-plus-solutions already in the memory-library). Particular judgments about new cases are made on the basis of past cases, not rules. And at the opposite end of the spectrum from rules, skilled practice seems to be shaped by collective experience in an even less explicit manner. Here one cannot usually recall specific cases; one 'just knows' what to do by a kind of experienced intuition.[30]

Given our experience in economics — that information overload reduces 'centralized rational planning' of a national economy to an oxymoron — it is surprising how many philosophers cling to the idea of a Centralized Method of Science as a kind of Rationality Czar — as the main defense of the rationality of science!

Once again, some of these ideas were anticipated by Kuhn on exemplars. Scientists, he said, solve puzzles by directly modelling them on exemplars by means of an acquired similarity relation. In so doing, scientists rely upon an intuitive, perception-like expertise rather than upon rules. But if such a view is defensible at the level of first-order working science (and recent work in psychology and AI has made it more defensible than in Kuhn's day), then why not at the metalevel? Why shouldn't methodology deflate from a rule-based Theory of Science (where the rules are either a priori or are backed by the evidence of historical cases) to informed, case-based judgments? On this view, single, well-done historical case studies now acquire methodological value in their own right, and much methodological reasoning becomes case-based rather than rule-based.[31] To be sure, each case will be idiosyncratic in some ways, yet sometimes (as in discussion of science policy) the richness and historical situatedness of real cases is just what we need, in order to get a feel for what might happen, one way or the other. Patricia Kitcher (1992) makes this worthwhile point when she contends that her study of Freud lays out the richest available case of an interdisciplinary theory of the mind, against which we can gain some perspective on the likely strengths and weaknesses of contemporary cognitive-scientific programs. By contrast, the Blacksburg project, "testing theories of scientific change" (Donovan *et al.*, 1988) analyzed the theories, and sometimes the historical cases as well, into small, abstracted components; and it reduced historical cases to positivistic data points.

To sum up, Peirce's main complaint against Descartes's maximizing, foundational economy of science applies also, in muted form, against EA as the sole account of scientific rationality. Failure to recognize the central importance of HA blocks the road to inquiry.

7. *Concluding Unfinished Dialogue*

What, then, of the prospects for a general theory of justification? A better question, I suggest, is: What business is left for (a deflated) EA, once we recognize the importance of (a deflated) HA? A striking fact about current science studies is that, outside of philosophy of science, there is almost nothing that would count as traditional logic of justification or confirmation theory! The situation is practically the reverse of that of 20th-century philosophy of science to about 1970. Then logic of justification was everything and discovery was nothing. Then all the interest was in the final products of scientific investigation, and there was minimal interest in the processes (which the principal tool, symbolic logic, was inadequate to characterize). Now, most of the interest is in the processes, in scientific practices, and there is less interest in the products, per se. Understood very broadly, discovery wins over justification and history defeats logic! Philosophers have made a major contribution to this reversal. After all, it was the historical philosophers of science who initiated it. And not one of the big system builders — Kuhn, Feyerabend, Lakatos, Toulmin, Laudan, or Shapere — retained anything resembling traditional confirmation theory. Meanwhile, troubles have mounted for confirmation theory even among the more traditional philosophers of science, often in the guise of attacks on the H-D method. Some problems can be blamed on first-order logic and the material conditional, but many science studies practitioners question the relevance to real science of tinkering with formal logics of confirmation.

The tendency over the past two decades has been toward deflation, toward seeing justification problems as local and concrete. However, there is a major exception that deserves our notice, namely, Bayesian confirmation theory. On the basis of early, impressive results, Bayesianism has emerged, over the past three decades, to a position of dominance among those philosophers still committed to a general account of scientific research, or at least confirmation (Howson and Urbach 1989).

While the question is still open, this apparent exception may only confirm the above reversal. Despite technical advances in some quarters, the Bayesian program now seems mired in serious difficulties, and many philosophers of science doubt that it can fulfil the positivist dream of a general theory of confirmation. In *Bayes or Bust?* (1992), John Earman, a Bayesian by inclination, sees Bayesianism as the last hope for a comprehensive confirmation theory; but he so fairly exposes its flaws that reviewer John Worrall (1994) concludes that Bayesian confirmation theory is 'bust'. Meanwhile, Deborah Mayo (1996) rejects Bayesianism in favour of classical Neyman-Pearson-Fisherian approaches to problems of statistical inference, precisely on the ground that the latter's error-analysis route to experimental knowledge is more central to actual scientific practice than the Bayesian project. Philosophers are attracted to the Bayesian Way, she suggests, not because it saves the phenomena of scientific practice but because it saves so much of traditional confirmation theory and promises a general methodology of science. Like logical positivism, Bayesianism is more about *philosophy* of science than about *science*. Much of the criticism of the theory-centred H-D method applies also to Bayesians, who regard themselves as the probabilified successors to H-D theory.

I would add that many Bayesians, for good or ill, retain the old, liberal political consensus model of justification first introduced by Hobbes, Locke, and other 17th-century social contract theorists. Initially, Bayesian theory allows considerable freedom of thought in the choice of 'personal' or subjective prior probabilities, but then conditionalization on the evidence, as it comes in, is supposed to produce a sharp convergence of opinion. As Mayo points out, the posterior probabilities furnished by Bayesian method amount to a kind of opinion poll, as constrained by the evidence. They tell us *what the scientific community thinks or believes* (with what probability), after the evidence is in, but without a detailed account of how the scientists manipulate layers of data to eliminate errors, etc., and without being able to say which results may be reliably employed in ongoing investigations. Bayesianism remains in the tradition of the theory-centred, political consensus model, albeit in the consequential wing as opposed to the Cartesian, generative-foundational wing.

In their time Baconian and Cartesian foundational methodologies attempted to eliminate all conceptual and doctrinal slack from the start. (The same was true of methodologies as recent as operationism and some strains of positivism.) It was crucial that everyone's knowledge claims (and even concepts) be identical, and true, *and arrived at in the same way*. The H-D method and, later, Bayesianism, permit as much slack as you please at the early stages, and see nature, through empirical testing, as forcing us toward consensus in the longer run. But it remains controversial whether even this much convergence of *belief* is required.

From a still wider perspective, we can view general theory of confirmation, including Bayesianism, as an application of general, Modernist epistemology and logic. Bayesianism is part of the grand narrative of empiricism, of how we learn from experience by observation of an independent, objective nature, largely unaffected by our presence. Certainly Bayesianism is more congenial to a spectator theory of knowledge than is Mayo's account of experimental practice.

What can we conclude from all this? If the dozens of science studies on which I rely are correct, there is little need to speak of a general methodology of science, for purposes of academic philosophy of science or fine-grained science studies — as opposed to more popular contexts such as elementary school science classes and courtrooms, where such simplified rhetoric may sometimes be appropriate. (A good pragmatist will be sensitive to these contextual differences.) When studied in a fine-grained, academic manner, we find that science is not a tight, unitary culture, governed by a clear set of rules. In direct contrast to the Modernist picture, according to which sciences increasingly resemble physics as they mature and even become reducible to physics, the sciences actually become more diverse as they mature (Dupré 1993, p. 199, Galison and Stump 1996). This despite the fact that many areas of science are now highly interdisciplinary. The basis of interdisciplinary cooperation on research teams is not so much a seamless, unitary world picture as an opportunistic patching together of what is needed to get on with the project. Scientific culture, too, to adapt the words of the early anthropologist, Robert Lowie, is better regarded as a "thing of shreds and patches" than as a unity. As he writes in the famous concluding paragraph to *Primitive Society* (1920):

> To that planless hodgepodge, that thing of shreds and patches called civilization, its historian can no longer yield superstitious reverence. He will realize better than others the obstacles to infusing design into the amorphous product; but in thought at least he will not grovel before it in fatalistic acquiescence but dream of a rational scheme to supplant the chaotic jumble. (Lowie 1920, p. 441)

Applied to science, belief in a strong methodology would be a superstition. But society is unlikely to invite future philosopher kings to redesign science along more 'rational' lines, since such an attempt could be expected to straitjacket science, in violation of the Peirce principle. The traditional philosophers' idea of rationalization is out of touch with cognitive- and financial-economic reality.

So must we conclude that science is an irrational, chaotic jumble? No, for such a conclusion commits precisely the same mistake of overlooking the detail. If we are interested in describing and explaining or regulating the differences, the diverse initiatives among the sciences or within a given science, then a global methodology is precisely what we do not want. By applying to all sciences and all styles and projects equally, it gets no purchase. And for scientific work to succeed, rational persons and groups must differ! At the microlevel, there is considerable rationality (as that term is normally used in human discourse) involved in the economic planning by individuals and groups engaged in heuristic appraisal. Even at the macro-level, we find some patterns of behaviour that help to convince us of the general reliability of scientific results (practices as well as claims) — not their truth as The Final True Story of the World but their reliability for the present contexts of scientific work. After all, any contentious claims or practices that make a real difference to ongoing research are likely to be challenged from some quarter and their status clarified. Ironically, the most general pattern is surely that of opportunistic competition within a pragmatic framework!

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NOTES

1. My thanks to Dr. Gaye McCollum for suggestions that improved this paper. I am also grateful to the U.S. National Science Foundation for present and previous support on projects of "knowledge pollution" and "heuristic appraisal".
2. See Hoffman (1996) on the self as a power center in early Modern epistemology and political theory.
3. Chief among them, F. L. Holmes (1981, 1984), the most prolific scientific biographer of our generation. Among the spate of recent, serious scientific biographies of Darwin, Freud, Einstein, Schrödinger, Heisenberg, Dirac,

Fritz London, Barbara McClintock, and others is Holmes's two-volume biography of Hans Krebs (Holmes 1991 and 1993)).

4. The same thing is happening in other fields. Movie credits now include not only actors, directors, and special-effects companies but also the chauffeurs, caterers, and security people, as well as the bankers who provided the loans.
5. There is surprising variation in scientists' own beliefs even about specific techniques. Techniques are notoriously difficult to impart in words and usually must be learned in practice, by direct example and expert guidance.
6. In these paragraphs, I am expounding rather than defending the H-D method. See Laudan (1981) and Nickles (1987a and b, 1989a and b) for details. The economic metaphors are mine.
7. For John Herschel and William Whewell, who are usually credited with successfully introducing the H-D method, a fully successful hypothesis produces more than predictive success, namely, what I call "generative justification" sufficient to elevate the hypothesis to the status of a theory.
8. The same is of course true in business. It would be more accurate to think of each hypothesis as a business project or product line than a whole new business venture.
9. As those two movements are commonly portrayed. McCollum informs me that most Romantic poets rejected the idea of free imagination.
10. McCollum suggests that some method discourse pays mere lip service to the idea that rational people cannot differ. Reference to common method serves to maintain one's status as a rational agent. Ironically, however, such ritual invocation of method can actually serve to obscure and thus to permit more actual difference than is officially acknowledged to exist. The latent function contradicts the manifest function!
11. As before, I distinguish the dominantly proclaimed method of science from the methods actually used in practice.
12. Laudan (1977) would later level this charge against Popper and others.
13. If method is a priori, then method implicitly contains only the logical structure of potential discoveries; the empirical content is to be supplied by the data. In science, however, most strong methods incorporate substantive claims about the world; and so the urgent question arises as to how we could possibly discover and validate the method without already knowing the basic structure of nature. For example, Galileo's, Descartes's, and Newton's versions of the method of analysis and synthesis all presuppose that reality is hierarchically organized and nearly decomposable into its parts.
14. Social constructivists will object to using the Meno paradox itself as the basic problem of inquiry, since it appears to presuppose a realist solution.

15. This is the practice-idealist thesis of Rouse's *Engaging Science*.
16. See §5 below on heuristic appraisal. A person, too, is a (human) resource whose heuristic potential can be appraised.
17. Question begging because 'discovery' implies that we have found something pre-existent in the real world as opposed to engaging in social construction or invention. Some constructivists think this way of speaking begs epistemic questions (questions that, I believe, a multi-pass methodology partly answers). Others do not impugn the reality of scientific results but point out to what extent they are manufactured or fabricated through considerable human effort that alters nature (cf. Knorr-Cetina 1981; Pickering 1984a, 1995). In some sense the phenomena produced by atomic accelerators and by biochemical labs are artificial, not found in unprepared 'nature'. I agree that 'discovery' becomes a problematic term in some contexts, but I shall continue to use it, for convenience, in a broad and fairly relaxed sense that includes construction and invention.
18. This reversal, together with the current emphasis on Baconian, experimental manipulation rather than on grand theoretical perspectives as the basis of genuine, reliable scientific knowledge, can be read as a version of the Master-Slave reversal discussed by Hegel and later writers. The grand theorists with their God's-eye view only think they are the masters of the universe. The real masters are the lowly experimentalists and technicians who know how to make nature do what they want. Thus can we fit current, anti-Modernist trends in science studies into a grand historical narrative! Actually, science studies face a problem of reflexivity here, for how can they claim to 'know science' unless they have actively experimented with different implementations of scientific research?
19. Van Fraassen (1987) holds that the semantic view of theories is neutral on the issue of realism.
20. Peirce, James, and Dewey distinguished Cartesian, 'spectator' theories of knowledge from the pragmatic inquiry (as doing and making) that they advocated (Fisch 1951, 28). Although Kuhn is critical of standard accounts of theories and scientific inquiry, it is worth recalling the role of the Copernican revolution in the formation of his own views on scientific paradigms and revolutions. The Copernican revolution was the subject of Kuhn's first book (1957). Kuhn keeps one foot squarely in the aesthetic, theory-centred tradition.
21. Actually, there is a similar ambiguity in HA. We can assess the fertility of past ideas and procedures independently of their truth status while judging them sterile for specific future applications. Their potential has been exhausted. A once rich mine has 'played out'.
22. It is possible to tease apart logical from more broadly economical versions

of HA. When Popper and Lakatos discuss fertility and opportunity, they usually mean fertility in the logical sense — novel prediction.

23. Again, compare Kuhn on direct modelling of new puzzles on exemplars. The account works also for the making of novel experiments and the construction of new kinds of apparatus. See, e.g., Pickering (1990) on Luis Alvarez's construction of the first, large, hydrogen bubble chamber.
24. Bayesians will claim that many facets of HA are covered by their admission of subjective probabilities, but several are clearly not.
25. Here we are back to Latour's crediting multiple 'external' agents for discoveries.
26. Cognitive economy will not allow us to assume infinite reservoirs of resources any more than the financial economy will in today's environment-sensitive atmosphere. Also, the 'freedom' of thought and imagination here is reminiscent of the negative versus positive political freedoms of Western democracies, for the method provides no positive resources for the production of new ideas.
27. Similarly, Kuhn reacted negatively to Feyerabend's proliferation methodology, the idea that we should abandon a monotheoretic methodology (such as Kuhn's) for a pluralistic methodology involving intense competition among deep theories. On the material realization of science, see Radder (1996).
28. This happens even in the realm of abstract theory. See, e.g., Pickering (1984a and b) and Cushing (1990).
29. Patterns of patronage can make a significant difference, e.g., the impact of the Rockefeller Foundation on 20th-century biology. However, the priorities of such agencies must be pretty broadly based, for their wealth gives them no purchase on a definite future. Here we meet HA problems again: agencies cannot know now what they will only know later.
30. These are not intuitions of the self-justifying, Cartesian variety but, rather, the intuitive, fluid performances achieved by skilled practitioners. See Dreyfus and Dreyfus (1986), Margolis (1987), and also several articles on parallel processing in Rumelhart and McClelland (1986).
31. See Nickles (1997b). Compare the autonomy of experimental work. This is not a retreat from the rule of universal law to a prelegal society of warlords but rather a change from statute law to case law. See Toulmin (1972) and Jonsen and Toulmin (1988).

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