# DISCOVERY LOGICS

*Thomas Nickles*[1]

## 1. *Introduction*

Is there a logic of discovery? My position looks triply paradoxical against the background of the traditional discovery debate. I claim that (1) there is no Logic of Scientific Discovery, but there are logics of discovery! There is no logic of discovery in the sense of a single logic underlying Scientific Method; but there do exist many logics of discovery, even in the strong, historical sense of actual use. (2) While there is no *content-neutral* logic of discovery, there are many rather local, substantive or content-specific methods that merit the name 'discovery logics'. (3) The new discovery logics that emerge in times of major historical breakthrough nearly always postdate the breakthrough. Such a logic is not the cause or explanation of the corresponding discovery; rather, it is a methodological *part* of the discovery itself. Typically, discovery logics are rational reconstructions of results arrived at by more haphazard routes. They are worked out by critical reflection on how the substantive problem solutions were originally achieved and how these methods might be streamlined or otherwise improved. They are what I term *discoverability* logics. They are idealized discovery procedures - methods that could have been employed to make the original breakthroughs if (contrary to fact) we had known then what we know now. These discoverability logics reduce problem solving in that domain to routine and can sometimes provide the basis of new, original discoveries.

## 2. *Some Background*

The opening statement of Aristotle's *Posterior Analytics* reads, "All instruction given or received by way of argument proceeds from pre-existent knowledge." Discovery or *inventio* was a topic debated by Renaissance logicians and rhetoricians such as

Ramus. Their interest was mainly in how best to organize what was already known, for didactic and other "humanistic" purposes. Bacon, Descartes, and other luminaries of the "modern classical" period of methodology rejected this conception of discovery as sterile because nonampliative. This is understandable in the context of that time. For the discussion was pretty much confined to Aristotelian logic, which did not embrace mathematics. Further, when relatively small amounts of knowledge are organized in patterns as simple and transparent as the syllogism, it is hard to appreciate either that the "formal" organization of our knowledge itself may be an important and useful "substantive" addition to our knowledge or that deductive reasoning may furnish new knowledge. In other words logically nonampliative reasoning may yet be *epistemically* ampliative. Actually, our methodological ancestors may have dimly appreciated this point, for they sometimes wrote as if deductive reasoning could be ampliative, without clearly distinguishing logical from epistemic amplification. This conflation is still widespread today, but since we now know that no genuine proof is logically ampliative, it is often taken for granted that logical reasoning is nonampliative *period*.

Bacon and Descartes dreamed of a general method or organon of scientific discovery that would reduce problem solving to routine and disclose new and useful truths about the universe. Such a logic of discovery would be a great "leveler of wits" (Bacon), since the only expertise required to make discoveries in any field would be knowing when to apply the discovery method, and to what information. Since then, the search for a logic of discovery has been largely the search for a magic formula, a general methodological rule (or set of rules) for generating interesting new truths or for solving problems in any domain. Over time this method has been increasingly conceived as a "logic" in the sense of a constructive, proof-like or recipe-like procedure that could be applied to widely differing contents but that would itself be independent of the results of any particular discipline.

Some of the contemporary "friends of discovery" retain this conception of a discovery method. Some study analogical and metaphorical inference in detail, seeing it as a kind of inductive logic. Some friends, following Herbert Simon, recognize the importance of problem-solving *heuristics* and broaden the term 'logic of discovery' to include heuristics. This is fine, but some conceive heuristics in the same way as modern, formal logical rules - as affording general techniques of reasoning one's way to new claims or results. They treat heuristic methods as if they are topic-neutral deductive or inductive logics, only weaker and less reliable. However, discovery logics fitting this description turn out to be limited in power, as we shall see.

Meanwhile, the more humanistic friends of discovery who turn to historical case studies for illumination face their own difficulty. For historical studies disclose the great diversity of innovative work, a fact which implies the improbability of finding a strong logic of research that fits them all, even when (or *especially* when) 'logic' is weakened to include heuristics. Many philosophers employ historical information in a methodological "hypothesis testing" mode while retaining the assumption that there must be a single, general methodology of science (Nickles, 1986). Unfortunately, history is so complex that new epicycles are always necessary, with the result that the proposed methodological rules (including discovery procedures) become more and more abstract and/or complex. In either case methodology loses touch with real scientific research.

Recently, some AI experts have attempted to combine the two approaches, producing programs that allegedly solve some historical problems in a constructive manner and thereby explain how the historical figures did it.[2] But in demanding strong, constructive explanations of historical discoveries, these friends of discovery try too hard. For it is a plain historical fact that the discoveries they discuss - e.g., Kepler's, Black's, and Ohm's laws - were not made by such a procedure and could not have been at the time. The data and methods of the AI program were not historically accessible to the original discoverers. In my terms these AI experts are really talking about rationally reconstructed discovery procedures, about discoverability rather than original discovery.

Seeing that tight logical reasoning is rarely available to historical discoverers, many friends of discovery have retreated from logic to rationality. There is no rigorous logic of discovery, they admit, but upon closer inspection most historical discoveries turn out to be rationally motivated to a much greater extent than well-known positivist and Popperian writings allow. They use this rationality point to argue that discovery is a topic amenable to methodological treatment, whether or not discovery has a logic. Having shown the possibility of the exercise, they proceed to offer reasons why discovery must be an important part of methodological discussion.

This strategy (which I endorsed in Nickles, 1980a) is fine as far as it goes, but I now think that it gives away too much to critics ("enemies") of discovery and furnishes only a fuzzy and partial characterization of discovery. There are indeed many

reasons - historical, sociological, psychological, and logical - for thinking that a fully general logic of discovery cannot exist. However, some of these reasons are reasons for thinking that local logics, in a fairly swork into the
strong sense of 'logic', *can* exist. Once we admit that possibility, it is easier to recognize that several of them do already exist in each of the developed fields of science.

Friends of discovery still unwittingly allow their opponents to dictate the terms of the debate by providing a very truncated account of the discovery process. They often treat discovery as a preliminary stage of research, prior to the "pursuit" and "final justification" stages. On this view discovery accounts merely fill a gap (the omission of the discovery stage) in positivist-Popperian treatments of science rather than transforming our entire conception of inquiry, as they should. Some friends also believe that in order to defeat their opponents, they must show that a certain kind of discovery process is required by logic of justification.[3]

Neither view fits the history of science well, and neither is necessary to a viable "methodology" of discovery. "Stage" theories of scientific development are historically naive. In fact discovery is a process that continues long after the original ideas are produced, published, and even "accepted." Most significant innovations possess an interesting history of further development and reinterpretation long after their acceptance. As these innovations are successively recharacterized so as to address new problems at the moving frontiers of research, they may eventually be transformed out of recognition. As examples consider the Kepler problem (the motion of one body around another) from Kepler to Newton. Consider the far-reaching transformations of Newtonian mechanics over a period of two centuries. Consider how Einstein and Ehrenfest in 1905-1907 completely transformed Planck's 1900 work, thereby inventing early quantum theory in the process (Kuhn, 1978), and how later work into the 1920s again transformed the early work.

## 3. *Is There a Logic of Discovery?*

Does a logic of discovery exist? *Could* such a logic exist? These existence questions are multiply ambiguous. Here I bring out the first level of ambiguity. For historians and historically-oriented philosophers of science, the question asks whether successful, historical scientific practice has been based on such a logic; and the answer is clearly "no." No explicit, general logic of discovery underlies either historical or contemporary scientific work, nor has any methodologist succeeded in formulating such a method despite centuries of attempts to distill out the essence of science in this fashion. Notice that it is no argument against the existence of a logic of discovery in this sense that scientists often employ insight to solve problems. For such a logic would be at best a sufficient, not a necessary, method for solving problems. Even if a logic of discovery were available, it would not necessarily (pace Bacon and Descartes) be the most efficient way to make discoveries. It is often more efficient to employ heuristics than available algorithms. On the other hand, such heuristics are closer to a logic of discovery than to flashes of insight, and I shall include them within logics of discovery, *sensu lato*.

To a more formalistic philosopher, the negative existential claim cannot be justified by appeal to historical practice. "To be is to be used historically" is an absurdly parochial criterion of logical existence, retorts the formalist. The question is whether or not a discovery logic exists in the abstract sense in which mathematical or logical structures are said to exist, whether anyone has ever thought of them in particular, much less used them to solve an actual problem. In this sense, as Kevin Kelly has observed, there are more things in heaven and earth than are dreamt of in your traditional philosophy of science, either formal (e.g., Carnap, Hempel) or historical (e.g., Kuhn, Laudan).

> Given a programming system, the hypothesis generation procedures specifiable in that system exist abstractly in the same sense that proofs in a given formal system exist. So the logic of discovery is an abstract study whose domain includes all possible procedures. (Kelly, 1987, p. 436)

How then can I be so sure that there is no fully general logic of discovery, even one which has implicitly underlain scientific practice? Perhaps Reason has been too cunning for us. I postpone answering the question to mention some preliminary quibbles. Somewhat ironically, the historicist will say, the nearer we come to considering abstractly the space of all logically possible procedures, as we can fathom it, the less accessible this space is to historical agents. Nor do we have the logically omniscience required to know what belongs to that space. We should expect that parts of the space of all possible procedures are not even historically accessible at any given time. Besides, any red-blooded methodologist of empirical science will want more than an abstract existence proof, even if such is to be had. It may be that zillions of abstract structures exist in the sense that they are consistent with our logical and mathematical assumptions; it is another thing to discover a structure that is practically useful to us. The discovery logic, too, must be discovered. Since it may be more difficult to discover a goose which lays a golden egg than to discover golden eggs, why think the effort to find a logic of discovery would not be better invested in directly trying to make the discoveries themselves?

This bundle of objections largely misses Kelly's point, which is that discovery procedures overlooked by Carnap, Kuhn, *et alia*, are accessible within the space as we conceive it now, today. Kelly agrees, of course, that discovery logics must themselves be discovered. He adds a second point against sole concern with historical practice, namely that the adequacy of a discovery logic (how much generality, efficiency, explanatory power, etc. we require) is a normative question, not an historical one. For him, logic of discovery is not an empirical, descriptive science. In favorable cases (relative to proposed adequacy relations that are recursively enumerable), the existence of logics of discovery is a mathematical truth, Kelly notes. But even in unfavorable cases (adequacy relations of various degrees of uncomputability), it remains possible that discovery machines exist; uncomputability does not entail nonexistence of a corresponding discovery logic. Additional arguments are necessary in such cases, either way.

Naturally, historicists will complain that the formal approach returns us to the attempt to express scientific thinking within a precise, oversimplified, formal-logical language and in terms of perfectly rational, ahistorical, and asocial agents. In one way the historical criterion for the existence of a logic of discovery is more demanding than the formal one. That a generator of confirmed hypotheses exists abstractly, relative to some formalized language and a set of adequacy requirements, does not satisfy the historical criterion, which says that a discovery logic exists only if someone has used it to solve an important kind of problem. The rational is not necessarily the historically real. Some historicists would go even further to demand that the genuine discovery logic furnish novel ideas, novel problem solutions that no one has so far produced.

To demand actual, historical novelty is too strong, in my opinion. I see discovery and discovery relations, like confirmation relations, as logical rather than historical-temporal relations between proposed problem solutions and their constraints. However, my conception of the discovery process remains more historical than formal in Kelly's sense.

### 4. *Is a Logic of Discovery Possible?*

Whether logics of discovery are possible and, if so, how to find them is a generative form of the problem of induction. The reason why a completely content-neutral (a priori) method of discovery is apparently not possible for empirical science is that such a rule could teach us nothing about our world. A logic of discovery is an amplification device. Apply it to some knowledge (or to hypothetical claims) and it generates further claims. Since a completely neutral rule[4] is one incorporating no knowledge about our particular universe much less about any particular scientific domain, we cannot expect a neutral, ampliative rule to improve on blind guessing. And anything deserving the label 'logic of discovery' must do that.

A neutral rule is the methodological counterpart of Hume's Adam, brought afresh into the world with no experience and hence no informed expectations about event sequences. All knowledge, including methodological knowledge, depends upon experience. Goodman (1955) went beyond Hume to show that good inductive methods require the use of "entrenched" predicates, factors or "variables" that previous inquiry has revealed to yield projectible (empirically confirmable) claims about our universe. Michael Friedman writes:

> There is no inductive method that is more reliable in every logically possible world than every other method; consequently, there is no method that is a priori best, there is no method that is a priori the most reliable. We have to know facts about the actual world if we are to know which method is best; and we have to know facts about the actual world to know even that any given method has any chance at all of leading to truth.[5]

And Laudan (1988, p. 127) remarks that "theories and methodological rules are on a par in terms of their empirical and contingent character."

It follows that if there are any logics of discovery, they must incorporate empirical knowledge, or at least they must be empirically justified. They must be "substantive" rather than "neutral." But evolutionary epistemologist Donald Campbell appears to scotch even this possibility when he writes that:

> real gains [in knowledge] must have been the products of explorations going beyond the limits of foresight or prescience, and in this sense blind. In the instances of such

> successful gains, the successful explorations were in origin as blind as those which failed. (1960, p. 380)

Or as Popper says, we cannot know now what we shall only know later. Accordingly, Campbell (1960, 1974) and Popper (1974) reduce all genuine knowledge amplification to a process of blind variation plus selective retention (BV+SR), a process of the same form as Darwinian evolution by natural selection. Banished are all forms of illuminationist "theology," which purport to give us advance knowledge of the world - innate ideas, intellectual intuition, uninformed insight into nature's workings, a harmony between the human microcosm and the macrocosm, etc. We can only proceed by BV+SR. From this perspective, a generative methodology of knowledge-extension would seem to be impossible, and we are left with a purely consequentialist methodology that can proceed only by blindly producing conjectures and testing their observable consequences.

Not even BV+SR is content-neutral in any specific instance, of course, for any useful selection mechanism must embody some knowledge about the world (including the needs of the inquiring organism). How was it possible to acquire the first bit of knowledge then? How was the first inquiry possible? Here it is evolution to the rescue! Deliberate human inquiry, such as we find in science, never begins from scratch, for biological evolution has in fact tuned us to the world. Given sufficient time and a reasonably stable environment, BV+SR can do as well as a good, nondeceiving designer-God in creating us in harmony with nature. Our sensory and decision processes work reliably about ordinary things, else we should not be here. However, there is no reason to believe that each of us is a microcosm that reflects the *deep* structure of the macrocosm, as classical rationalists had it. Since our biological attunement is limited mainly to capacities for processing surface phenomena and provides no specific beliefs about the world, we may suppose with Campbell that BV+SR is the only method available to humans in what we might call "the original epistemic condition." Any powerful heuristics we employ today only represent knowledge previously gained by BV+SR, and any further extension of them can only be blind.

If we assume that Campbell is correct, as I shall, it appears that there can be no genuinely ampliative method that is more reliable than BV+SR, no logic of discovery. Apparently then, the argument against the existence of completely content-neutral logics of discovery extends even to content-laden logics of discovery. In each case the "logic" cannot reliably carry us beyond the knowledge already incorporated therein (zero in the case of content-neutral logics); hence, it is not a logic of *discovery.* We must live forward, but we can only crystallize logics out of our practice in retrospect.

Fortunately, there is a powerful objection to this pessimistic line of thought, namely that it conflates epistemic amplification with logical amplification (Blachowicz, 1989). Epistemic amplification does not entail logical amplification. Strict deductions can yield epistemically novel results, else much mathematical discovery is trivialized. For many deductive systems, constructive proof procedures and other algorithms exist for generating sometimes novel results. At least *this* sort of discovery logic is possible, and the point can surely be extended to heuristic reasoning. These discovery logics must themselves be discovered to be used, but that is a separate question. Their possibility (or abstract existence) is not in doubt.

One might try to extend this argument into the empirical domain as follows. By now powerful logico-mathematical methods are available to solve routinely certain kinds of problems that our empirical inquiries frequently pose. Once the empirical information is suitably expressed in formal terms, we need only turn the mathematical crank. (One thinks first of the calculus, then of statistical reasoning.) But the methods of deductive logic and mathematics are content-neutral if anything is, so a content-neutral logic of discovery is possible after all. We can separate empirical substance from pure logico-mathematical form, as it were, and proceed neutrally from there.

Which side is correct? Is there any logic of discovery at all? Is there an empirical logic of discovery? Is there a general, neutral logic of discovery? Yes or no?

Campbell and Popper can reply as follows. The empirical amplification argument presupposed that not all the logical consequences of our empirical knowledge count as part of our current knowledge. This is correct but it undermines the logic of discovery point, for there is no guarantee that the newly drawn logical consequences will be true. Since we can never be sure that our current "knowledge" claims are true, we cannot know in advance whether their logical consequences are true. On the contrary, it is just by testing such consequences ("predictions") that we determine the mettle of the conjectures from which they are derived.

This reply can be blunted by noting that we no longer require a logic of discovery to be infallible, only to yield new ideas that are plausible, interesting, and/or suitably related to present "knowledge."[6] In this weaker sense, it seems clear that heuristics and logics of discovery can exist at some level, as

Campbell would surely agree; but there is still no reason to think that there exists one *master* logic of discovery. Campbell contends, in effect, that the knowledge incorporated in successful ampliative methods must be still more specific than that required by Hume or Goodman or Friedman:

> The many processes which shortcut a more full blind-variation-and-selective-retention process are themselves inductive achievements, containing wisdom about the environment achieved originally by blind variation and selective retention.

Since I follow the naturalistic epistemologists in denying the existence of a completely neutral logic of discovery, I must respond to the suggestions that portions of mathematics constitute such a general logic. As always in modern epistemology, the status of mathematical knowledge is problematic. Our case calls to mind nineteenth-century controversies over "embodied" mathematics and also Einstein's (1923) famous remark that "As far as the laws of geometry refer to reality, they are not certain; and insofar as they are certain, they do not refer to reality." In an empirical problem domain we employ those parts of mathematics that have been found to work in that domain. I am not about to deny the impressive generality of certain mathematical techniques, or even that these techniques are essentially syntactical (content neutral), given that the problem is expressed in a certain way. But there's the rub - the syntactical instrument can be usefully applied only to empirical domains of knowledge (or conjectures) that are already highly organized in just the right sort of way. So it would be highly arbitrary to deny that this organization is part of the logic of discovery (see §7 below). On the other hand, if the mathematics is considered to be empirically interpreted from the start, it is obviously substantive and not content-neutral. We no longer assume that Euclidean geometry necessarily expresses the empirical structure of the world. Even to apply cardinal arithmetic to a domain supposes that there is something there to be counted. Few would any longer claim that there is a body of mathematical knowledge and technique which both furnishes a logic of discovery and is literally true of all possible worlds.

My point is not that empirical content always precedes formal expression. Life is more complicated than that. Increasingly in mathematical physics, for example, mathematics provides a strong heuristic for research in the sense that the formalism may largely precede the interpretation (Cushing, 1982). This reinforces the earlier point about harnessing deductive but epistemically ampliative reasoning to empirical problem-solving. Fairly standardized modeling techniques coupled with deductive power can provide powerful methods of hypothesis generation.

## 5. *From Blind Variation to Logics of Discovery*

Consider the status of both evolutionary epistemology and of discovery logics in the light of Plato's paradox of the *Meno.* The puzzle is how inquiry, learning, the acquisition of new knowledge, is possible. It has the form of a dilemma. Either we already know or we do not. If we do, inquiry is not possible, for we already know. But if we do not already know, then inquiry is again impossible, for we could not recognize the answer even should we stumble upon it blindly. Aristotle would not have recognized molecular genetics or quantum field theory as the solution to any of his problems even if the seminal papers had dropped at his feet.

Let us distinguish two levels of the Meno problem. The first is how we could possibly get from zero knowledge to some knowledge. The second is how we could get from some knowledge to the vast scientific knowledge we have today - and so quickly. Since Campbell is right that any heuristic or logic of discovery already encapsulates knowledge about our world, no logic of discovery can solve the first stage of the problem. Neither can BV+SR, for the same reason. But as noted in §4, we need not worry about the first stage. The basic physical forces plus evolution of the universe from the Big Bang to *homo sapiens* have solved that problem for us. The fact that our innate capacities are fallible and do not reflect the theoretical deep-structure of the universe sought by science is beside the point. From these spare beginnings, given enough time, BV+SR can produce as much knowledge amplification as you like.

But is there enough time? Does BV+SR suffice to explain the explosion of modern scientific knowledge? Charles Peirce thought not and appealed to a special attunement of the mind to nature.[7] Perhaps Peirce thought of this harmony as an evolutionary product. At any rate Nicholas Rescher (1978, p. 61) explicitly replaces it by heuristic methods that are the products of previous trial-and-error processes. Peirce himself had always recognized that the growth of scientific knowledge includes amplification of methods as well as multiplication of results such as facts and theories. In a paper in which a Darwinian theory of inquiry plays a large role, Peirce (1877, §1) wrote that "each chief step in science has been a lesson in logic" and that "questions of logic and questions of fact are curiously interlaced."

The contemporary point of all this is that there is no necessary opposition between Campbell's BV+SR perspective and that of logicians and heuristicians of discovery as represented by Herbert Simon.[8] While Campbell rejects all forms of prescience and asserts the BV+SR mechanism as the source of all knowledge, he allows for the gradual selection of a whole hierarchy of "vicarious selectors" that are in fact heuristics.

Bacon famously said that knowledge is power, an insight that has been developed in terms of problem-solving power by artificial intelligencers such as E. A. Feigenbaum.[9] Stated in these terms, the Popper-Campbell argument is that *lack* of knowledge is lack of power, lack of routine solution-generating ability not to mention a generator of essentially novel claims. If we reverse the reasoning here, however, we can be somewhat optimistic. For knowledge *is* power. In fact, in every day life and science we employ very low-level logics of discovery all the time, as when we multiply and divide and routinely perform more advanced mathematical operations in order to solve problems.

Most problem solving is not fully determined by such recipes, algorithms, and quasi-algorithms, of course, but they suggest how partial knowledge can constrain without fully determining the search for even highly novel problem solutions. We can know something without knowing everything, and this something may be embodied in powerful, fairly reliable heuristics that amount to discovery logics. Indeed, whenever we have a problem that is clearly enough formulated to make investigation possible, we have some constraints on the solution and hence some degree of heuristic guidance.[10] Insofar as these logics fail to determine the problem solution completely, we could call them *partial* logics of discovery. Such logics become possible only when our knowledge has sufficiently matured.

## 6. *Local Versus Global Logics of Discovery*

Campbell and Popper are right that any viable heuristic or logic of discovery for empirical science must embody knowledge about the problem domain in question, at least in the sense of requiring empirical justification. The Campbell-Popper argument and the response to Goodman's problem of projectibility have a more local thrust, however. Rather than our empirical knowledge of the world helping us to determine the overall connectedness or repetitiveness of events in our world - and hence to fix a single value of in Carnap's continuum of inductive methods (Carnap, 1952) - domain-specific knowledge imposes constraints on new knowledge, sometimes in the form of explicit heuristic procedures or even discovery logics. Again, I turn the Campbell-Popper point around. Instead of using it only to argue *against* a fully general logic of discovery, I employ it to argue *for* the possibility of local, domain- and context-specific logics of discovery, by which I mean more or less routine problem-solving methods. The global-local distinction represents a third level of ambiguity in our questions about the existence of a logic of discovery.

Broaching the local possibility renders nugatory some other general considerations against a global logic of discovery, such as (1) no general logic of discovery is in use or has been identified and (2) there is no single method, let alone a magical method of discovery, that adequately captures the diverse activities of the many sciences or that sharply demarcates scientific activity from other forms of problem solving. If we ask whether there are local logics (plural) of discovery rather than a single, global logic, an essence of science, we surely stand a better chance of getting an affirmative answer. In fact, the short response to the "whether possible?" questions is that such logics do exist and are already in widespread use. And 'does' implies 'can'. ("Do you believe in baptism by total immersion?" "Believe in it! Why, I've seen it done!").

There is something of a controversy today over local versus global, related to the above-mentioned problem over the status of mathematical methods.[11] No one anymore is a complete globalist, and everyone agrees that a problem-solving method so local that it only applied to one problem, or one exceedingly narrow and peripheral type of problem, would be uninteresting. But between these extremes there is room for disagreement. One group of philosophers, including historicists as well as formalists, still fancy philosophy as the study of what is general, as such, and quickly lose interest insofar as a topic "goes local."

On the global-local issue, developments in AI are instructive. To grossly oversimplify a short but complex history, the field of AI has evolved from the early attempt of Newell and Simon (reported in their 1972) to develop a general problem solver (GPS) that employed general, content-neutral heuristics (including as its weakest heuristic, "generate and test," which constitutes the entire method of science, according to Popper!), to knowledge-based, expert systems. Although GPS was designed to apply to simple, well-defined, idealized contexts rather than to real world problems, and although it was intended more as a theoretical tool than as a practical program, its performance was disappointing. Despite its intended generality, it could easily handle only a few, simple problems. Efforts to extend its generality succeeded only at the cost of limiting its abilities in other directions (Ernst and Newell, 1969).

It gradually became apparent that GPS-type systems are weak in problem-solving power. Other approaches, oriented to detailed, real-world applications, built in information quite specific to the discipline in question. This information did not consist merely of "premises" to be transformed by a topic-neutral, general logical apparatus, although such an apparatus was often present. It included also heuristic rules and problem-solving strategies specific to the domains and the problems in question. In some cases these rules were elicited from experts in those fields, a practice that is now common. Although restricted in scope, these so-called "expert systems" or "knowledge-based systems" could solve some difficult problems efficiently. In the words of Goldstein and Papert (1977),

> Today there has been a shift in paradigm. The fundamental problem of understanding intelligence is not the identification of a few powerful techniques, but rather the question of how to represent large amounts of knowledge in a fashion that permits their effective use and interaction.... The current point of view is that the problem solver (whether man or machine) must know explicitly how to use its knowledge - with general techniques supplemented by domain-specific pragmatic knowhow. Thus, we see AI as having shifted from a *power-based* strategy for achieving intelligence to a *knowledge-based* approach.[12]

With some exaggeration, Feigenbaum summarized the development thus:

> There is a kind of 'law of nature' operating that relates problem solving generality (breadth of applicability) inversely to power (solution successes, efficiency, etc.) and power directly to specificity (task-specific information).[13]

As I like to put the point, AI research epitomizes the history of science. AI experts over the past couple of decades have explicitly formulated what nineteenth-century scientists demonstrated implicitly: that powerful and effective problem-solving methods do exist but tend to be local not global. That is, they are discipline- and problem-specific and cannot be aggregated into a

General Methodology of Science (GMS), whether inductive, hypotheticalist, a priori, conventional, or whatever.

Before we get carried away by such quotations, a word of caution is in order. There are many types of problems and many varieties of bodies of information (some highly integrated, some highly diffuse, etc.). Some of the knowledge-intensive programs alluded to above were applied to different types of problems and domains than those for which GPS was targeted. The reader may consult sources such as Winston (1977) and Barr and Feigenbaum (1981) for distinctions between declarative and procedural AI systems, and the like.

Returning briefly to the question, How local is local?, consider the generalizing argument that runs as follows. Even if first-order discovery methods are content-specific, we can note the kinds of content-specificity that are relevant, for example the ways in which various kinds of information are conveniently organized. Then we can generalize these formal aspects of the concrete cases to other cases of the same type. A typology of problems together with different but routine methods of handling each problem type yields an overall method that is, in a sense, general and content neutral. In short, if there are content-specific, first-order logics of discovery, then there surely (or probably) are second order and somewhat more abstract discovery logics for discovering first-order discovery logics; and so on up to a master logic that is as abstract as you please.

Formalists may find such programs attractive. They tend to think that the distinct kinds of problem will eventually be discovered, that most of them are discriminable now, that such a "timeless" classification scheme can be accomplished in principle, and that routine solution methods will be found for most problem types. Formalists think that we can have both content-specificity at a local level and something approaching content-neutrality at a more global level. Historicists will deny that such a program leading to a methodological Big Switch - determine the problem type and turn the dial to the correct method! - can be carried very far, for two reasons. First, the master method would be applicable in any possible world, contradicting the considerations of Friedman and others, cited in §4. Second, how far up the formalist scale we can go is an empirical question, not a purely logical one. The question is one of degree. Those historicists who believe that our world is messy rather than neat or that historical processes will continue to transform our current outlooks and to produce essential novelty predict that we shall not get far beyond the content-specific logics at the first rung of the ladder of generality.

## 7. *Triple Paradox*

It is time to review the triply paradoxical position that I laid out in the opening paragraph. How can I deny that there is a single discovery logic while simultaneously asserting that there are many; deny that the key to discovery resides in a powerful logic yet nevertheless claim the existence of powerful logics of discovery; and deny that there is a logic of original discovery while asserting that retrospectively constructed logics can nonetheless be both powerful and important to discovery?

An example lends concreteness to my account.[14] James Watson (1968, p. 38) reports that Pauling's discovery of the alpha-helical structure of certain proteins frightened him and Crick into thinking that Pauling had discovered a new and powerful mathematical method of discovery that would quickly yield the structure of DNA. When they got their hands on Pauling's paper, however, they found that "Pauling's accomplishment was a product of common sense [and "reliance on the simple laws of structural chemistry"], not the result of complicated mathematical reasoning," much less of some ingenious, logic-of-discovery trick. I certainly do not wish to deny the importance of complex mathematical reasoning or the brilliance of much scientific work. The point is, there was no special logic of discovery operative here in the content-neutral sense of 'logic'. Stereochemical model building was the key. For the Watson-Crick sort of problem, model building provided a particularly convenient way to organize and access the vast body of chemical knowledge already available. Other constraints were also operative, of course, but model building was more important than any logic in the formal sense.

Actually, the model-building method of doing chemistry was and is also important in a *formal* sense (although not the usual logico-mathematical one) in that it induces a certain structural organization on the body of chemical knowledge, and provides a particularly concrete way to explore the space of possible chemical structures. Chemical knowledge imposes definite limits upon this space, hence, constraints on model building in general. Any specific structural problem imposes additional constraints, as does any hypothesized structural information; so model building is a way to represent the particular research problem with its full array of defining constraints (cf. Nickles, 1981). Model building also provides a relatively easy way to search for errors in the models as they are constructed, and can even disclose errors in chemical knowledge itself.

For more than a century now, interest in the spatial "form" of molecules, their configuration and conformation properties, as well as their chemical constitution, has imposed an important new form or organization on chemical knowledge. New principles have emerged to constrain and guide future research. Obviously, this formal element is content-specific.[15]

After 1953 the Watson-Crick sort of work underwent many refinements so that by now much problem solving in the gene-structure domain has been reduced to routine. Knowledge-based AI systems such as HEURISTIC DENDRAL and MOLGEN were the first to show that analytical and synthetical problem solving in some areas of chemistry and biochemistry can be automated. Today a more mundane appreciation of mechanization is gained by looking no further than the nearest laboratory facility. Sophisticated equipment automatically performs procedures that once required the greatest efforts of human experts. With automation, we do find a certain of leveling of wits. Laboratory technicians can perform operations by means of machines without having to fully understand what the machines are doing. Ditto for computer programs.

These automated procedures are (or physically realize) "logics" of discovery in our broadened sense – powerful problem-solving methods. Yet as Kenneth Schaffner (1980, p. 135) pointed out, their power often derives more from the organization of the "rich tapestry" of specific knowledge and considerations than from the use of special inferential moves or logical postulates. Logically speaking (now in the usual sense of formal logic), the discovery logics tend to be quite ordinary. I do not mean to deny that powerful logical and mathematical methods are often essential to discovery logics. Rather, I emphasize the complementary point that the "magic" or power of discovery logics often resides in the organization and accessibility of already achieved, specific knowledge.[16]

Note that the organizing principles themselves may be elements of knowledge (as with the periodic table, stereochemical regularities, and Feynman diagrams) or they may be hypotheses. This point is important because it shows how hypothetical models can help us make the most of what knowledge we have, in the face of Campbell's limit. Sticking to what we *know* about the organization of knowledge is a conservative procedure. If we do not know much about the organization, we may have no convenient way to relate apparently scattered bits of knowledge into a coherent picture, or no way to control large masses of knowledge. A hypothetical model can do this. Any hypothesis is risky, of course, and represents blind groping, but epistemic progress does not require genuine knowledge. A relatively poor hypothesis may enable us to make better use of what we do already know than no hypothesis at all.

That the organization of already achieved "knowledge" into conveniently accessible forms has been the key to extant logics of discovery is the main point of the paper. The most useful methods, procedures, and equipment are "compiled" knowledge and provide easy access to more knowledge. The process of scientific "rational reconstruction" that accomplishes this distillation is an essential part of the production of knowledge.

From this point of view, my opening paradoxes lose their perplexity. There is no single, neutral logic of discovery, but there exist many local, content-specific logics of discovery. These logics are more than fuzzy appeals to rationality. They are formal but not *strictly* formal, as I have explained. Their formality consists in their organization of a specific domain of knowledge. Insight typically involves a restructuring of information. Very crudely, as mind (intelligence) resides in the form of the body and its behavior, so a discovery logic (creative intelligence) is the form of a body of previously gained knowledge.

This metaphor can mislead because it is too conservative. When combined with a specific problem, discovery logics may gain sufficient leverage to alter the organization of previous knowledge (Koertge, 1982). It is a two-way street: the prior organization of knowledge partly determines the logic of inquiry (problem solving), but a locally successful logic can also reorganize a larger field. The organizational changes may be only temporary or cosmetic. For example, adopting a certain point of view in problem solving highlights the relevant information, thus improving our access to it. However, insofar as a group of exemplary problems and solutions comes to redefine a field, the methods or logics distilled from studying the original solutions may be employed to restructure "permanently" the previous knowledge of the field.

History discloses that such logics tend to emerge in scientific practice only by second-order reflection on problem-solving successes already in hand. The "enemies" of logic of discovery are correct that original, highly novel problem solutions are rarely guided by a strong logic. Thus I agree with Kuhn (1962) against AI experts that exemplary problem solutions are prior to rules. But Kuhn misses the extent to which rules may emerge later in a way that can reduce to routine entire domains of problems. In important cases exemplars do not retain an entirely tacit function; rather, they are (sometimes repeatedly) recharacterized and employed as centers around which to crystallize well-structured problem-solution spaces. Contrary to AI experts, well-structured problems and solution spaces and powerful discovery logics are not available for essentially novel problems; but, contrary to Kuhn, they often do become available later for more normal research. This idea that an exemplary breakthrough guides work which leads to its own routinization and canonical justification is one sort of bootstrap process that deserves more study.

## 8. *Discovery Logics Versus Discoverability Logics*

The idea that discovery logics are *post hoc*, that logics of discovery are usually logics of discoverability, or derivative from them, gives the Campbell-Popper argument its due. As they say, life can only be understood backward but must be lived forward. So here I must face the objection that my account of discovery logics is backward: the logics post-date the discoveries rather than guide them! Why then term them logics of *discovery* at all?

There are four reasons. (1) Since discovery is an achievement, discovery includes justification. (2) More substantially, as the original problem solution is employed to better define the solution space, the solution becomes better justified - generatively justified, not only justified by consequential testing. The strongest form of justification is an idealized discovery argument (Nickles, 1985, 1989) and discovery/discoverability logics provide that. But what has this talk of justification to do with *original* discovery?

My answer is, (3) the analysis of original problem solutions and the improved definition of the problem space can lead to routinized methods of problem solving that can handle any problems of certain types, including specific problems that have never arisen before. A method that reduces the treatment of an entire domain to routine (and to possible mechanization of problem solving) is worthy of the name, "logic of discovery." Future research need never again stall on problems of this kind. Still, this is *novel* problem solving in a pretty thin sense, so let my try once more.

(4) The reconstructive activity that reorganizes available knowledge in a powerful and accessible way is at least a significant step toward a logic of discovery, in the way that chemical modelling constitutes a powerful heuristic method and in the way that Lagrangian and Hamiltonian mechanics provided not only standardized ways of handling classical mechanics but also

provided a basis for handling many quantum problems (e.g., find the classical Hamiltonian and substitute quantum operators for classical variables). It is not only old facts and principles that are recharacterized for application at the frontier; old problem-solving *methods* are recast and refined also. At the very least, establishing the "discoverability" or generatability of exemplary problem solutions provides valuable models for further research. Here I think Kuhn is right that research proceeds initially by modeling new solutions on old, only I should place more emphasis on the extent to which the old solutions have been routinized. I have no space to spell out these points in detail, but the reader may reflect on the mentioned scientific developments and many others.

## 9. *Concluding Thoughts*

The discovery problem represents more than a lacuna in other accounts of science. It is of general epistemological importance. It is the constructive form of the problem of induction. It is the Meno problem. It is the problem of the growth of knowledge. It is the problem of intellectual economy. It is the questions whether, how, and to what extent our ability to learn improves *as* we learn. Does our "methodological intelligence quotient" increase over time, coordinate with our gains in knowledge?

My position attempts to give proper emphasis to the fact that in science we do learn to learn (Shapere, 1980), that the art of discovery advances as discoveries advance (Bacon, 1620, Aph. CXXX), that each chief step in science has been a lesson in logic (Peirce, 1877). Science produces not just factual and theoretical results but methods, not just problem solutions but problem-solving methods and other investigative procedures and practices. As pragmatists have long emphasized, scientific results are important not only as contributions to knowledge, but because they can be converted into knowledge-amplification devices. Philosophers have noted that low-level facts and laws (e.g., this piece of pure lead melts at 273° C.) can be used as general "inference tickets," but they have been slow to extend this point to higher levels of knowledge integration and organization.

Science is sometimes characterized as a body of research findings, sometimes as a method; but it is neither and both of these. Viewing science as a collection of findings neglects the tremendous growth in our procedural knowledge and skills and runs into the problem that many of today's findings will later be rejected or revised. We may not have absolute truth, but we do undeniably have investigative power and hence knowledge. We can *do* it. Yet we cannot simply deny the existence of a body of positive scientific results and escape by saying that science is really method, as Popper (among many others) attempts to do. For no informative and viable conception of method can divorce it from substantive results. Traditional statements have left the empirical method curiously independent of empirical knowledge!

While there is no logic of discovery that will effortlessly produce revolutionary scientific advances, this denial is *not* equivalent to saying, with Popper and others, that we are left only with the method of bold conjectures and refutations. Since this is virtually the weakest, least efficient method in the scientists' toolkit and the one they fall back upon only when nothing stronger is available, this pure-consequentialist position overemphasizes our ignorance and undervalues the extent and power of our knowledge. It practically reduces the growth of knowledge to the accumulation of factual and theoretical results (which Popper hardly wants to do, as just noted) and denies that we have acquired and improved upon constructive methods of inquiry. It denies that we have learned to learn. I do not see how such a view can explain the exponential rate of increase in knowledge since the seventeenth century. This is the second level of the Meno problem. An adequate account of discovery, including learning theory and learning-to-learn theory is needed to solve it.

The friends of discovery agree that the traditional discovery programs of Bacon and Company were impossibly ambitious. For one thing, we should allow strong heuristics to count as logics. (Bacon may have allowed this, had he our terminology.) And until recently methodologists tended to be infallibilists. We no longer demand that a successful logic of discovery produce near-certain knowledge claims. Third, a logic of discovery may be local rather than global. Fourth, it may be content-specific, theory laden or fact laden.

Effective heuristics and logics of discovery incorporate knowledge. They must be justified empirically, and they depend heavily on the previous organization of knowledge and contribute to that organization. Contrary to Bacon, Renaissance treatments of discovery, as having to do with the organization and presentation of knowledge already in hand, were not entirely on a wrong track. The irony of Bacon's logic of discovery is that he attempted to formulate it at the very moment that he denied that we yet have much genuine scientific knowledge. And the irony of the common view that logic of discovery was abandoned to the method of hypothesis in the nineteenth century is that by then several fields were reaching sufficient maturity that many logics of discovery were emerging.[17] Since these logics tended to be local rather than global, philosophers looking for a General Logic of Scientific Discovery have missed them. Although these methods for routinely solving problems were low-level methods, relative to the revolutionary novelty which often came later, they were not at all trivial by the standards of Kepler, Galileo, Gilbert, and Descartes - not to mention Bacon. Genuine discovery logics began to appear in diverse fields just when philosophers were no longer prepared to find them.

Department of Philosophy
University of Nevada, Reno

NOTES

1. I am indebted to the U.S. National Science Foundation for research support.
2. See Langley, Simon, Bradshaw, and Zytkow (1987) for the BACON series of programs and Buchanan (1983, p. 129).
3. See Nickles (1985) for a discussion of "the *per se* thesis," the view that discovery *per se* is relevant to justification. That there is a substantial symmetry of formal theories of confirmation (e.g., Carnap's and Hempel's) and generative loigcs is surprising, given the opposition of most confirmation theorists to logic of discovery (Kelly, 1987). But of course the confirmation theories do not *require* that the hypotheses be generated in this way.
4. Whether there are any completely neutral rules depends on the status of logic and "pure" mathematics. For present purposes I shall take them to be neutral, i.e., analytically true, if true, but in the longer run I would adopt a Quinean or Goodmanian viewpoint. Without pursuing the matter here, we might imagine that a rule could be non-neutral or "substantive" either by expressing some identifiable empirical content (as when natural law claims are turned into inference rules) or by virtue of surviving an empirical justification process (a neutral rule that is found to work well in our world); or both.
5. Friedman (1979, p. 371). Compare Laudan's (1988, pp. 126f) attack on the conventionalist view of methodology. I do not suggest that all useful heuristics are empirical. "Begin with simple models" probably is not, and yet its rationale lies in the empirical fact that we often learn more by evaluating

simple models than by beginning with complicated ones.

6. In fact the reply reminds us of the importance of discovery tasks within a hypothetico-deductive methodology, even in context of justification. To find a sufficient number and variety of testable consequences of an hypothesis already on the table is no trivial task. Finding inconsistencies of an hypothesis with known empirical or conceptual constraints are also tasks worthy of a logic of discovery.
7. See, e.g., Peirce's *Collected Papers* 7.220.
8. For this point, see Wimsatt (1980). Simon (1977) and Langley *et al.* (1987) is representative of Simon's work on discovery.
9. This is one theme of the controversial Feigenbaum and McCorduck (1983).
10. See Nickles (1983). Of course, saying that inquiry is possible whenever we have problems to investigate only raises the prior question, Where do problem come from?
11. Glymour and associates (1987) and some formal learning theorists are more cosmopolitan and less local than I.
12. Quoted by Duda and Shortliffe (1983), a useful comment on AI's "turn."
13. Quoted in Feigenbaum, Buchanan, and Lederberg (1971).
14. We could easily multiply examples of the organization of knowledge and the way in which this organization is exploited to solve problems. Think of the periodic table of the chemical elements, similar classifications in particle physics, Feynman diagrams, biological classification (and the controversies surrounding its proper relation to evolutionary theory).
15. A fourth level of ambiguity is concealed in our initial existence question (and in much of the ensuing discussion) if we allow logics of discovery to be material as well as formal. That is, we include in the class of discovery logics not only those which are local rather than global but those which are laden with the specific content of a field.
16. In some cases it will be "six of one and half a dozen of the other," as our brief discussion of mathematics in §4 suggests. Had I more space, some major qualifications would be in order.
17. The now-classic expression of this view is Laudan (1980).

## REFERENCES

Achinstein, P. 1968. *Concepts of Science.* Baltimore: Johns Hopkins UP.

Bacon, F. 1620. *Novum Organum.* Translated as *The New Organon* by J. Spedding, R. Ellis, and D. Heath. London, 1863.

Barr, A., and E.A. Feigenbaum. 1981. *The Handbook of Artificial Intelligence.* Reading, MA: Addison Wesley, 3 vols.

Blachowicz, J. 1989. "Discovery and Ampliative Inference." *Philosophy of Science* 56, 438-62.

Campbell, D.T. 1960. "Blind Variation and Selective Retention in Creative Thought as in Other Knowledge Processes." *Psychological Review* 67, 380-400.

Campbell, D.T. 1974. "Evolutionary Epistemology." In Schilpp (1974), pp. 413-463.

Carnap, R. 1952. *Continuum of Inductive Methods.* Chicago: University of Chicago Press.

Cushing, J. 1982. "Models and Methodologies in Current, Theoretical High-Energy Physics." *Synthese* 50, 5-101.

Duda, R.O., and E.H. Shortliffe. 1983. "Expert Systems Research." *Science* 220 (15 April), 261-268.

Einstein, A. 1923. "Geometry and Experience." In *Sidelights of Relativity.* New York: Dutton, pp. 27-45.

Ernst, G.W. and A. Newell. 1969. GPS: *A Case Study in Generality and Problem Solving.* New York: Academic Press.

Feigenbaum, E.A. 1968. "Artificial Intelligence: Themes in the Second Decade." *Proceedings, IFIP68 International Congress*, Edinburgh.

Feigenbaum, E.A., B.G. Buchanan, and J. Lederberg. 1971. "On Generality and Problem Solving: A Case Study Using the DENDRAL Program," *Machine Intelligence* 7, 165-190.

Feigenbaum, E.A., and P. McCorduck. 1983. *The Fifth Generation. Reading,* MA: Addision-Wesley.

Friedman, M. 1979. "Truth and Confirmation." *Journal of Philosophy* 76, 361-382.

Glymour, C., R. Scheines, P. Spirtes, and K. Kelly. 1987. *Discovering Causal Structure.* Orlando: Academic Press.

Goldstein, I., and S. Papert. 1977. "Artificial Intelligence, Language, and the Study of Knowledge. *Cognitive Science* 1, 84-123.

Goodman, N. 1955. *Fact, Fiction, and Forecast.* Cambridge: Harvard.

Kelly, K. 1987. "The Logic of Discovery." *Philosophy of Science* 54, 435-52.

Koertge, N. 1982. "Explaining Scientific Discovery." *PSA 1982*, Vol. 1, pp. 14-28.

Kuhn, T.S. 1962. *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

Kuhn, T.S. 1978. *Black-body Theory and the Quantum Disconti-*

*nuity, 1894-1912.* Oxford: Oxford UP.
Laudan, L. 1980. "Why Was the Logic of Discovery Abandoned?" In Nickles (1980b), pp. 173-183. Reprinted in *Science and Hypothesis.* Dordrecht: Reidel, pp. 181-191.
Laudan, L. 1988. "Are All Theories Equally Good? A Dialogue." In *Relativism and Realism in Science.* Edited by R. Nola. Dordrecht: Kluwer, pp. 117-139.
Newell, A., and H.A. Simon. 1972. Human Problem Solving. Englewood Cliffs, NJ: Prentice Hall.
Nickles, T. 1980a. "Scientific Discovery and the Future of Philosophy of Science." In Nickles (1980b), pp. 1-59.
Nickles, T. 1980b. *Scientific Discovery, Logic, and Rationality.* Dordrecht: Reidel.
Nickles, T. 1981. "What Is a Problem that We May Solve It?" *Synthese* 47, 85-118.
Nickles, T. 1985. "Beyond Divorce: Current Status of the Discovery Debate." *Philosophy of Science* 52, 177-206.
Nickles, T. 1986. "Remarks on the Use of History as Evidence." *Synthese* 69, 253-266.
Nickles, T. 1987. "From Natural Philosophy to Metaphilosophy of Science." In *Theoretical Physics in the 100 Years since Kelvin's Baltimore Lectures.* Edited by P. Achinstein and R. Kargon. Cambridge, MA: MIT Press.
Nickles, T. 1989. "Truth or Consequences? Generative Versus Consequential Justification in Science." *PSA 1988,* Vol. 2 (in press).
Peirce, C.S. 1877. "The Fixation of Belief." *Collected Papers* 5.358-87.
Peirce, C.S. 1901. "The Logic of Drawing History from Ancient Documents" (draft). *Collected Papers,* Vol. 7. Cambridge: Harvard UP, 1958.
Popper, K. 1974. "Campbell on the Evolutionary Theory of Knowledge." In Schilpp (1974), pp. 1059-165.
Rescher, N. 1978. *Peirce's Philosophy of Science.* Notre Dame UP.
Schaffner, K. 1980. "Comment on Achinstein." In Nickles (1980b), p. 135.
Schapere, D. 1980. "The Character of Scientific Change." In Nickles (1980b), pp. 61-116.
Schilpp, P.A. (ed.). 1974. *The Philosophy of Karl R. Popper.* LaSalle, IL: Open Court.
Simon, H.A. 1977. *Models of Discovery.* Dordrecht: Reidel.
Watson, J.D. 1968. *The Double Helix.* New York: Atheneum.
Wimsatt, W. 1980. "Reductionistic Research Strategies and their Biases in the Units of Selection Controversy." In *Scientific Discovery: Case Studies.* Edited by T. Nickles. Dordrecht,

Reidel, pp. 213-259.
Winston, P. 1977. *Artificial Intelligence.* Reading, MA: Addison-Wesley.