Hanson's discussion of the theory-laden character of observation was his most enduring contribution to the philosophy of science. In fact, it would not be an exaggeration to say that Hanson's novel account of observation amounted to a refutation of the Received View. Despite its significant role in the downfall of logical positivism, Hanson's own new model of the nature of observation failed to usurp the central place held by the previous conception. Instead, Hanson's account has persisted as a telling critical brief against the untenable positivist position.

In most epistemological accounts, observation and interpretation are treated as separate entities that are nonetheless often paired together. However, their pairing is taken to be a mere incidental matter, as with peaches and cream or fish and chips. However, Hanson argued that the apparent separability of the concepts of observation and interpretation results from the naivety of our language, for we think that where there are two words there are two objects, each capable of existing on its own. However, any process of observation inevitably comprehends both perception and comprehension: "I contend that observation and interpretation are inseparable—not just that they never do occur independently, but rather in that it is inconceivable that either could obtain in total isolation from the other." Thus, it is not possible to think of an observation as totally bereft of either sensational input or comprehension—both are necessary to the concept of observation.

Both in science and everyday contexts, observation and interpretation are found to be interdependent activities; in addition, our talk about either of these concepts cannot proceed very far until the other is inevitably invoked. Hanson asserted that models of the
“peaches and cream” type are much less revealing than models of the following kind: lift and drag on an airfoil, warp and woof in fabric, and matter and form in a statue. If we try to isolate either member of the pair from its mate, we find that we are left with nothing at all. Similarly, if we try to separate observation from interpretation, we lose the capacity to speak intelligibly about scientific observation:

Just as separating the warp from the woof destroys the fabric, and separating the lift from the drag on a wing will render aircraft uncontrollable—and just as “separating matter from form in a statue” describes nothing intelligible at all—so also, slicing the incoming-signals-of-sensation from appreciating-the-significance of those signals would destroy what we know as scientific observation. The Neo-positivistic model of observation—wherein our sensations data-registration and our intellectual constructions therefore upon are clef at all—is an analytical stroke tantamount to logical butchery. This results only in the expiry of the heart of natural science, the pulse of which is the struggle toward more intelligently encountered, reasonably comprehended, and theoretically appreciated observations.²

The lesson of Hanson’s account is that observation’s contribution to the “pulse of science” cannot be appreciated without our coming to fully understand the concept of observation and its relations to other important concepts in the epistemology of science.

There are two main goals in this chapter. The first is mainly expositional and historical; I wish to emphasize the intimate interrelations of the concepts of observation, intelligibility, objectivity, and discovery in Hanson’s philosophy of science and point out some prominent yet received errors in standard readings of Hanson. The second goal is to demonstrate that Hanson’s complete account of theory-laden observation is fully compatible with scientific objectivity and briefly indicate how some of Hanson’s projects can be revisited and extended by contemporary philosophy of science. In particular, I will show how Hanson’s distinction between psychological and inferential modes of representing theories is capable of explaining why theory-laden observation does not present an epistemological dilemma to the history of science, and I will use the case of the purported discovery of N-rays to make this point.

Why Does Theory-Laden Observation Still Matter?

Since the late 1960s, the prevailing view of philosophers of science as to the significance of theory-laden observation has been that it provides a challenge to the objectivity of science. For the most part, philosophers of science have desired to reconcile the theory-laden character of observation with scientific objectivity, and this reconciliation has been attempted through criticism of epistemological accounts of observation. The goal of such approaches has been a demonstration of observation’s theory-neutrality, with observation viewed at best as something of a necessary evil. While the compatibility of objectivity and theory-laden observation is a valuable thing to have proven, observation’s role in scientific epistemology is too diverse and critical to be reduced simply to its compatibility with objectivity. I shall argue not only that Hanson’s account of observation does not lead to scientific subjectivity, but also that the theory-laden status of observation is essential to an understanding of scientific intelligibility and discovery.

Hanson was concerned with observation’s total role in science’s complex framework of interrelations, but the important discussion of theory-laden observation he initiated got irreparably derailed in the late 1960s. A cornerstone of Hanson’s account is his theory of intelligibility: he argued that the theoretical loading of observation is what makes science intelligible since it provides the bridge between our knowledge and our seeing. Hanson also thought of the process of explanatory hypothesis as being intimately related to observation. More generally, Hanson’s analysis of the concept of observation offered a paradigmatic demonstration of the complicated and reciprocal coagulation of concepts characteristic of scientific thinking.

Hanson’s philosophy of science is unified by his conception of scientific theories as fluid complexes of concepts and expectations. It is important to emphasize that these complexes undergo continual extension, revision, and refinement; the principal stimuli toward growth for scientific theories are the forecasting and evaluation of consequences. While Hanson strongly emphasized the centrality of context in scientific thought and the looseness of scientific patterns and concepts, he clearly did not regard science as an irrational, relativist, or even sloppy affair. Rather, he saw science’s looseness as
responsible for the breadth and versatility of scientific thinking. Hanson's account was meant to demonstrate the fluidity of scientific conceptual structures, not their arbitrariness. Fluidity is entailed by the thesis of theory-laden observation, whereas arbitrariness is only implied by the stronger thesis that the patterns through which observations are shaped are in no way testable.

After Hanson's premature death, the appearance of Kuhn's *Structure of Scientific Revolutions*, and the publication of Israel Scheffler's *Science and Subjectivity*, philosophical concern with observation became exclusively bound up in the question of whether attempts to test theories were not invariably given to question-begging. The motivations behind Hanson's original thesis were ignored, in large measure, and philosophers went on to interpret the thesis of theory-laden observation as presenting the traditional epistemological problem of foundations. According to this traditional approach, theoretical influences on observation are looked upon as potential contaminants of the system of knowledge, and once their innocuousness is made clear, we have an end to the matter.

In addition to Scheffler's contribution, the works of Kordig and Shimony are prominent examples of objectivity critiques that view theory-laden observation as inevitably leading to circular justifications. Scheffler provides the classic expression of the view that theory-laden observation robs science of its objectivity and intelligibility:

If seeing is indeed theory-laden in the sense described [by Hanson], then proponents of two different theories cannot observe the same things in an effort to resolve their differences; they share no neutral observations capable of deciding between them. To judge one theory as superior to the other by appeal to observation is always doomed, therefore, to beg the very question at issue. We may call this the paradox of common observation. It has the effect of isolating each scientist within an observed world consonant with his theoretical beliefs.

According to Scheffler, not only do theoretical influences on observation appear to make it impossible to determine whose view of the world is objectively right, but the views of rival theorists seem to be mutually unintelligible. From the studies of Scheffler, Kordig, and Shimony, a consensus emerged that theory-laden observation is not problematic provided that the theory under test is not influencing the observational tests.

More recently, a group of thinkers including Harold Brown, Allan Franklin, and Alan Chalmers critically investigated the claim that theory-laden observations cannot test theories and, mostly through analysis of historical cases in physics, these researchers forged a new consensus: circular justifications based on theory-laden observations can be legitimate if certain conditions are met.

These two branches of research into the epistemic character of theory-laden observation have been very informative, for philosophy and history of science alike, but they have mostly been content to toil in the interest of solving the problems of a traditional epistemology. For many, once it had been shown that the theory-laden character of observation does not necessarily compromise objectivity, the issue came to be thought of as settled. However, as I shall show in what follows, such analyses have not only entirely missed the importance of theory-laden observation in discovery, but they completely ignore the features of Hanson's epistemology that guard against subjectivity.

Hanson's notion that observation is theory-informed established a reciprocal relationship between observation and theory: observations are useful and relevant to theories because they contain theoretical content, while theories grow out of observations and must answer to observation when their predictions do not square with it. Between the extreme positions holding that either observation or theory absolutely hold court over the other, Hanson attempted to forge a via media: observation and theory are complicatedly interdependent and not wholly separable. The virtue of this approach is that it explains the richness and provenance of scientific theory and the role of observation in testing and discovery; in logical terms, we have an account of where our premises come from and how frameworks are tested. Hanson's following statement illustrates his commitment to the view that theory-laden observations are the basis for tests of theory:

The philosophical "middle way" must always be the one which recognizes significant observations within a science as those which at once meet the criteria of relevance embodied within extant theory, while also being capable of modifying that theory by the hard, stubborn recognition of "what is the case," of the facts. Science does not make the facts, however much it may shape, color, and sort them.
INTELLIGIBILITY, SEEING AS, AND SEEING THAT

In this section, I wish to defend Hanson’s account of intelligibility and his claim that observation of new, unfamiliar phenomena must be made intelligible through analogical seeing. It is thus shown that theory-laden observation and the intelligibility of theories are two sides of the same coin. I will also show that science’s intelligibility is cashed out in terms of the consequences of patterns of conceptual organization.

What is it to understand electromagnetism, quantum mechanics, or cosmology? What does it mean to understand anything at all? In a certain sense, all it means to understand the phenomena of science is to know what will happen under every conceivable set of circumstances. However, since it is impossible, for a number of reasons, to spell out what will happen in every circumstance, we are only able to make phenomena intelligible by constructing conceptual frameworks that capture as much of the phenomena’s structure and behavior as possible. Philosophers, and many scientists, have often singled out the inference network—i.e., the most general description of what will happen under what circumstances—as the essence of a scientific theory. Thus, philosophical study of science is often concerned with theoretical laws and definitions since these provide a shorthand expression of the inference network of an area of phenomena. Hanson, however, did not believe that the inference network alone, or even the sets of laws and definitions from which the inference structure could be abstracted, were sufficient for scientific understanding. For one thing, the laws and definitions had to come from somewhere, and some of the conditions on their intelligibility derive from the psychological construction preceding their abstraction. Additionally, due to our cognitive limitations and the way we are situated in the world, there are only certain limited ways in which systems can be psychologically encoded; more precisely, there are formally equivalent frameworks that differ greatly in their pragmatic aspects. Furthermore, as Hanson put it, “there is no substitute for old-fashioned familiarity.”

We often feel that we comprehend some subject due to long acquaintance with it. Nevertheless, while we can possess some intuitive comprehension about certain things, if we fail to also have some knowledge of the inference network in which such things figure, our “pancreatic sympathies” represent nothing but an illusory understanding. Science strives to create conceptual frameworks that encapsulate inferential networks within a context of familiarity.

Hanson offered an account of scientific intelligibility on a number of levels: first, he explained what it is that makes a particular scientific theory, law, or term intelligible; second, he rendered intelligible a number of the departments of methodology that other philosophers of his time were content to leave unanalyzed, such as discovery, model construction, and theory choice. Theories and laws are intelligible when we know how to use and evaluate them; methods and practices are intelligible when we understand their function and the constraints on their development, i.e., we understand their place in the logical structure of science. For Hanson, science is a quest for intelligibility:

Fundamental physics is primarily a search for intelligibility—it is philosophy of matter. Only secondarily is it a search for objects and facts (though the two endeavours are as hand and glove). Microphysicists seek new modes of conceptual organization. If that can be done the finding of new entities will follow.

When scientists encounter theoretically recalcitrant phenomena, they retreat to the already comprehensible realm of the familiar in order to make sense of them:

Explaining perplexities requires linking them to the normal cases—the un perplexing. The unusual becomes unsurprising only when inferentially hooked to the usual. Models suggest to us ranges of possible explanations—routes to the unsurprising.

Familiar objects are intelligible because we know how they behave in a wide variety of situations and through many sensory channels. The best way to simplify novel phenomena and introduce a working knowledge of causal expectation is to view such phenomena in analogy with a suitable familiar object or context. This process of analogical seeing, or seeing as, is the first step on the road to learning about anything; it is also through seeing as that new hypotheses and discoveries are produced. Analogical seeing produces a set of candidate inferences concerning the phenomena at hand; if the target representation (novel phenomena) is an isomorph of the source (familiar), then the target will behave like the source in heretofore
unobserved cases. If a propitious source representation has been selected, then many of the candidate inferences will be affirmed and, to the extent that they are, the phenomena have come to be understood. If a large number of candidate inferences are disconfirmed, one has either to supplement the original source representation with other analogical representations, start again from scratch, or simply note the places in which the analogical mapping breaks down. Hanson argued that analogical seeing (and analogical data interpretation generally) is a rational process of discovery since it directs inquiry down a path that is testable, self-corrective, and fruitful.

Once a strategy of analogical seeing achieves a degree of success, one’s model of the phenomena becomes deeply integrated in perception and cognition. The candidate inferences that were such a subject of conscious attention and novelty in the early stages of theory construction now become embedded in the experience and data themselves; they are, in fact, so deeply ingrained that a considerable mental effort is needed to recognize that any concept application or inferential process is occurring. Hanson refers to this epistemologically rich brand of seeing as “seeing that” or “epistemic seeing.” Once seeing is thus enriched with expectation, explanations appear with the immediacy and coherence of a newly appeared aspect in a Gestalt drawing:

The dawning of an aspect and the dawning of an explanation both suggest what to look for next. In both, the elements of inquiry coagulate into an intelligible pattern. The affinities between seeing the hidden man in a cluster of dots and seeing the Martian ellipse in a cluster of data are profound.

Our conceptual patterns, thus, allow us to see the world as intelligible, and, understood in its epistemic sense, “seeing is just what we might call appreciating the world as intelligible.” Hanson argued that we should not regard it as a mystery that our systems of ideas and our observations should so nicely correspond:

The intelligible world, the ideal of science, is the brute world as it comes through the sieve of language: it is (in part) the visible world as we see it, it is the world of objects, events, and situations as we can state facts about it. So perhaps after all there are really no insuperable philosophical or logical problems about how language and

the world ever got together. Nor is it any wonder that our statements so often fit the facts. They were made for each other.

While Wittgenstein and Jerome Bruner’s “New Look” psychology were certainly significant influences on Hanson’s philosophy, the contribution Peirce made to his thought was far from trivial, though it has not been appreciated. Hanson saw himself as reviving Peirce’s program of developing a logic of discovery and Hanson’s necessary conditions for concept possession are straightforwardly Peircean. Here is Hanson’s description of what it means to say that someone possesses a concept:

What is it to see boxes, staircases, birds, antelopes, bears, goblets, X-ray tubes? It is (at least) to have knowledge of certain sorts. . . . It is to see that, were certain things done to objects before our eyes, other things would result. . . . “Smith sees x” suggests that Smith could specify some things pertinent to x. To see an X-ray tube is at least to see that, were it dropped on stone, it would smash.

Hanson’s view here is reminiscent of Peirce’s more general account of conceptions:

Our idea of anything is our idea of its sensible effects . . . consider what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object.

Peirce wanted to clarify our ideas of things by reducing them down to items accessible to the mind. In a similar spirit of clarification, Hanson established a sort of operational definition of conceptual possession: by appealing to conditional sets as a means of delineating the minimal commitments involved in possessing a concept. While the emphasis surely differs between Peirce and Hanson, both of them are quite explicit in stressing the relation of theoretical commitment to empirical expectations. Thus, it is clear that for Hanson, to see that an item is of a specific type is, at a minimum, to have some capacity to forecast consequences concerning the item. Hanson’s notion of conceptual structure is very amenable to a rational account of scientific change: when candidate inferences are falsified, the associated conditional statements are purged from the concept.
new analogies are brought in, new sets of testable conditionals are incorporated into the concept.

Hanson’s distinction between *seeing as* and *seeing that* has often been misunderstood. *Seeing as* is clear enough: to see a collection of lines as a box is to see it as taking up space, as having certain properties (e.g., edges, inside, outside) and so on. *Seeing that*, conversely, has been the source of considerable confusion. What follows the “that” is a declarative sentence, according to Hanson, a sentence that the subject of the observation takes to be true in virtue of psychological inferences based on perception, many of which are unconscious. Hanson’s critics, like Kordig and Norris, argue that we should only append the “sees that” locution to declarative sentences that are in fact true. While we would certainly avoid some ambiguity by adopting such a proposal, we would also find that the locution “sees that” could no longer be used very often. In fact, “sees that” is perfectly applicable to historical cases of scientists observing through the lenses of obsolete theories:

In the east the 13th Century astronomer sees the sun about to begin its daily journey from horizon to horizon. Just as the birds fly from tree to tree, and the clouds float from hilltop to hilltop, so the sun here at dawn is beginning its journey to the western horizon like a fiery balloon. He sees that, were he transported to heaven, he could watch the sun, the moon, and all the planets and stars circling the relatively fixed earth, the center and fulfillment of the physical universe. Seeing the sun at dawn through the geocentric spectacles of the 13th Century astronomer would be to see it in something like this way.

Thus, we can see that the objection of Kordig and Norris misses the point of Hanson’s usage of the concept of *seeing that*. However, their objection misses the point in a far more significant way: the distinction between *seeing as* and *seeing that* is meant to mark a difference in the depth of perceptual and cognitive integration of concepts, as well as a greater degree of interlocking between one’s experience and language. When one *sees that*, one is no longer aware of the process of concept application; one simply reads factual statements off of one’s observation. *Seeing that* is so epistemologically important because it brings our knowledge to the front lines, so to speak, by integrating it effortlessly with our sensory experience.

The central insight for a proper understanding of Hanson’s conception of analysis. Analysis is a mode of interpretation, a way of taking things apart. It is not a thing unto itself, nor is it simply a description of things as they are. Hanson makes clear that his notions of *seeing as* and *seeing that* are the products of analysis: “Seeing as” and “seeing that,” then, are not psychological components of seeing. They are logically distinguishable elements in seeing-talk, in our concept of seeing.” This statement underscores the complex relations of different forms of analysis assumed by Hanson. In some sense, perception and thought just are: they are things we experience directly. However, when we analyze these activities for some purpose or other, we conceptualize them and break them down. For this reason, a psychological analysis of vision and a logical analysis of it need not be concerned with entirely alien and incommensurable subject matters. They are two different analyses of the same subject matter, not two analyses of different sets of data. It follows from this that logic and psychology are neither separate nor independent:

The exhaustive and exclusive dichotomy “Psychology or Logic?” may win debates occasionally, but it cannot win the guerdon of truth. Many features of the actual problem solving of ordinary people, and of ordinary scientists, require understanding of the criteria in virtue of which one can distinguish good reasons from bad reasons.

*Seeing as* generates a set of expectations concerning nature experience. Perhaps we could also regard *seeing as* as just making the objects of perception look a certain way, a “certain way” that is not specifiable in any behavioristic or forecastable terms. Hanson uses *seeing as* in both of these senses: the first makes observation intelligible by placing it in chains of possible events; the second is a bit more mysterious. When one sees the Christ figure suddenly emerge from chaos or snap into place, one does not simply come to understand where the eyes and ears are; one is not just enabled to answer certain questions about the figure and specify the details. One now experiences the drawing as a unified whole, and the why it is experienced is qualitatively quite different than it was before.

While Hanson surely seemed open to the possibility that concepts affect, and perhaps determine, the phenomenology of perception, he did not seem to regard such a possibility as possessing
much logical significance. When he discusses the sense-data theory, he argues that we regard two observers as having the same visual experience when they produce essentially similar drawings of it. While I can surely experience phenomenological modifications when I switch aspects in *seeing as*, I am blocked from observing such a change in others. However, I am still able to discern the aspect under which some observer views some phenomenon by monitoring her behavior, descriptions, and knowledge claims. Once we start speaking of behavior, description, and knowledge, we are clearly dealing with perceptual cases that are epistemic; in these cases, “knowledge has threaded into seeing.”

Hanson was prepared to grant the existence of some “pure” seeing, unsullied by expectation or conceptualization, but he dismissed it as unimportant. Such cases of nonepistemic seeing are disconnected from the background of theory and the train of expectations that constitute our epistemological interaction with the world. Useful seeing always involves the hooking of the phenomenal onto the structure of a theory, into a matrix of entities, relations, and forecasts.

**Objectivity**

On Hanson’s view, any proper analysis of objectivity is concerned not with the phenomenal but with the behavioral and logical consequences of an experience. Thus, in some sense, the question of whether “pure seeing” is objective or not is a nonstarter—we have no way of testing the claim. In fact, we may not even be able to conceive of what such a claim would mean absent some reference to consequences. On the other hand, when perception is sufficiently loaded by concepts, further sets of perceptual and sentential consequences are implied, and many of these are testable. Thus, it is only with regard to conceptually integrated experience that objectivity claims can be appraised.

Psychological treatment of observation is not irrelevant to the logic of observation. Hanson is often called to task for dismissing psychology, for instance by Hans Radder, but I disagree that Hanson actually dismisses it. Hanson’s approach to observation could never have been motivated without consideration of cases from Gestalt psychology. Those cases press us to acknowledge the imposition of concepts on the visual field. More specifically, the psychological contemplation of Gestalt drawings discloses the logical character of such perception—the way we appreciate ambiguous or detail-impoverished drawings crucially involves the integration of knowledge and expectation in our seeing. Hanson turns away from psychology only when he starts inquiring into the meaning of the concepts of seeing. When he asks what it is to see differently, to see the same thing, to *see as* and to *see that*, he is after a conceptual analysis, an analysis of meaning. Such an analysis certainly dovetails with the psychological, but psychology is not equipped to give answers to such questions.

There are indefinitely many ways in which a constellation of lines, shapes, patches, may be seen. Why a visual pattern is seen differently is a question for psychology, but that it may be seen differently is important in any examination of the concepts of seeing and observation. Here, as Wittgenstein might have said, the psychological is a symbol of the logical.

The capacity to *see that* differently is logically significant because doing so allows us to draw different inferences from the “same” experience, depending on the concepts we bring to bear on the experience. Under what conditions will a person be able to *see that* a certain object is an X-ray tube? This is a question for psychology to answer, though many philosophers, Kordig especially, have felt compelled to approach it solely from the perspective of philosophy of language. What it means to assert that such and such an object is an X-ray tube tells us little about the psychological origin of such an assertion. We can of course ask, “What does it mean to *see that* an item is an X-ray tube?” This question is best answered by appealing to the formal structure of language—it is what Hanson called a logical question. Taking a closer look at the forms of representation theories facilitate will give us an even deeper insight into ways they make our experience intelligible.

Hanson distinguishes between two different forms of representation for theories. On the one hand, we have the notion of an inference structure; generally, inference structures are somewhat idealized and at some remove from actual experience. It takes some time to learn to see phenomena in terms of an inference structure. On the other hand, we have the psychological or embedded representation of a theory. Again, the inferential network and the
embedded network are not to be thought of as entirely disjoint representations; they are the products of different activities of analysis, but refer to the same underlying phenomenon. Hanson criticized Hempel's model of explanation because the capacity to predict, which is only concerned with the inferential structure, is not the capacity to explain—the fullest explanation that can be given of a phenomenon involves representing it both inferentially and psychologically, and this representation is facilitated by situating the phenomenon within a conceptual pattern.

So, on the other hand we have the abstract theoretical representation of a theory, and on the other we have its concrete form, embedded in cognitive, social, and practical forms. Growth is caused by making more of the abstract theoretical representation explicit and embedded; in the other direction, the abstract theoretical representation is altered when its consequences are unrealized.

It is fitting with Hanson's notion of conceptual structure presented earlier to consider the inferential structure of a theory to be an indefinitely large set of counterfactual or hypothetical conditionals. In this way, it would be possible to mark out the implication content of the theory. When Tycho and Kepler looked to the east at dawn, their observations differed primarily in terms of their implication content, even though the two men lacked the technology to put the implications to the test. In fact, many *Gedankenexperimente* are concerned to discover what would happen even in nonobservable circumstances. Often the impetus for theoretical innovation is the consideration of what might happen under some unthought-of or unrealizable set of circumstances; Einstein's thought experiment as a young man concerning what he would see if he were riding on a beam of light is an example of such a thought experiment. However, Hanson notes that contemplation of inferential structure is important on even the most practical level:

The very comprehension of everyday processes and laboratory events depends on conceptual extrapolations to what would obtain with "pure designata" released-in-thought from the "imperfections" of their empirical embodiment.37

If it were possible simply to enumerate all the consequences of a theory and then test them one by one, science would be an infinitely more successful enterprise, hardly calling for philosophical attention; however, science is so conceptually interesting because a number of complex factors block us from extracting all of a theory's consequences. While such blockages have often been understood as instances of irrationality in science, these limitations are not irrational but are merely evidence of a mismatch between the inferential representation of a theory and its embedded form.

Psychological structures are necessary because they encapsulate a great deal of latent inference and are sufficiently vague to be extended in different directions when changes in theory are called for. Psychological representations are an important source of insight, discovery, structure, and semantics for scientific theory.38 As a psychological scheme enables its user to make progress in dealing with the world, it also becomes possible to specify a rather detailed set of entailments of the scheme. In such a case, we might say that the psychological representation is mature enough that it can be associated with an inference structure. Since the inference structure is defined in terms of intersubjectively meaningful conditional statements, there is no room for subjectivity about it. There can, however, be considerable doubt about whether a given inference structure patterns the data in a more useful fashion than a rival structure. However, Hanson believed that explanatory unification considerations were what ultimately settled such doubts.

The facts of psychology will be most relevant to those aspects of scientific methodology that most intimately involve the use of embedded structures, whereas those aspects that involve inferential structures will be relatively independent of psychology. Objectivity is a value that does not depend closely on the details of cognitive structure—as long as commitment to a theory can be somewhat cashed out in terms of intersubjective and intertheoretical consequences, the particular concrete niceties of the theory are not critical. Alternatively expressed, since objectivity is best understood as a procedurally acquired value, there will exist multiple avenues through which objectivity can be established; because there are such alternatives, it is likely that establishing objectivity would not depend on a theory of conceptual structure.

In discovery, the situation is somewhat reversed. The scientist is casting about for a propitious analogy according to which it is possible to *see as* in a fruitful new way. The scientist here deals almost entirely with rather inchoate, vague, and subjective Gestalts, and tries
to locate a pattern that will offer some intelligibility and new predictions. Thus, within the period of discovery, the scientist searches for a strategy of seeing as that is rich enough to provide the basis for seeing that. The constraints on how we see as are clearly psychological, so the path to discovery is much more dependent upon our psychological makeup than are the processes of justification.

Since objectivity ultimately depends on how well a theory predicts and explains the observable, and such predictions and explanations must make substantial contact with intersubjectively expressible statements about observables, objectivity questions are adjudicated at a level removed from the embedded representation. So, on the one hand, one tends to traffic mainly in terms of inference structures, in terms of sets of conditional statements, many of which are intersubjectively decidable.

Controversy, the H-D Method, and Explanation

The tortuous process of selecting source representations, drawing out implications, testing the implications, and revising the representations accounts for the travail of scientific discovery. The psychological perplexities of the conceptual loading of observation provide an explanation for the deep, and at times strident, controversy that often results between apparently quite rational people. For our knowledge to thread into seeing, considerable perceptual penetration by concepts is required. However, despite the high degree to which perception is theory-laden in epistemic seeing, Hanson believed that Scheffler’s “paradox of common observation” could be resolved by analyzing test implications; he also thought it possible at least partially to see that through rival frameworks.

Hanson’s intention in his discussion of scientific controversy was precisely the opposite of the one ordinarily ascribed to him. Hanson was not arguing, as a standard interpretation insists, that controversies are irresolvable since theories insulate themselves from resolution by penetrating down to even the most elementary components of scientists’ cognitive and perceptual systems. Instead, Hanson tried to make his account of scientific intelligibility square with the historical ubiquity of controversy. Controversy derives from the cognitive obstacles that impede one’s capacity to see that according to a rival framework. However, just because it is difficult to learn to see that in a different way, does not entail that it is impossible. Scientific disputes are ultimately settled by appeal to consequences, though in a somewhat roundabout way through the provision of explanations, ontologies, and tests. Generally, the appraisal of intertheoretically accepted sees that sentences leads to the resolution of controversy. Occasionally, it is necessary to retreat all the way back to a sense-datum description, but we do this only because we lack the capacity to apply knowledge to our experience in the usual way. To say that there are perspectival differences is not in any way to say that they are absolutely insuperable; in other words, controversy hardly entails incommensurability. In fact, we might even say that the presence of legitimate controversy assumes the existence of some standard of resolution.

“Intelligibility” and “objectivity” are slippery words—since they connote success, they are used in an affirmative, though often nebulous, way. However, when we attempt to tie down their meanings in a specific context, it is possible to attach something like a definite meaning to each. I have shown that on Hanson’s account, for a theory, framework, or conceptual Gestalt to be intelligible it must be capable of expressing what kinds of things we should expect to encounter under what circumstances.

In ordinary contexts, to be objective means to be impartial, not to be swayed by mere subjective interests when pronouncing judgment on some situation. When we say one has objective grounds for believing something, e.g., that Caligula was the cruellest of the Caesars, we mean that others would be moved by these grounds to the same belief. Thus, objective belief, in the everyday sense, means belief that is built on the relevant information and not influenced by that which is false, immaterial, or merely personal. Whereas logical objectivity involves reasoning in a straight line from the certain to the certain, everyday objectivity involves selecting out likely claims from a sea of conflict and contradiction, subjecting them to test, and making decisions as to what to keep and what to discard and whether to continue investigating or to leave off. Everyday objectivity is achieved by adhering to a fallible method, not by deduction.

In science, much as in everyday contexts, opposed claims to the truth are resolved not through deduction from the given, but by determining the degree to which each claim squares with everything
else we either know or could test. Since absolute certainty at each step is impossible, the best one can do is to find a method that is
fruitful and self-correcting. The form of inquiry that leads to the
greatest growth of knowledge is the one to be adopted and Peirce
argued that the scientific method is this form of inquiry. For Hanson,
the sense of objectivity most appropriate to science is the objectivity
of inquiry: we shall call that pattern of inquiry that directs us down
the path of the most discoveries and explanations “objective.”

“Objectivity” is not a word that Hanson used, though he
argued that considerations of explanatory unification are what lead
us to prefer one theory over another:

If that to which I refer when accounting for events needs more
explaining than that to which you refer, then your explanation
is better than mine. Kepler’s astronomy needed less supplementary
explanation than Tycho’s, and so was better. Galileo’s cosmology
required less explanation than did that of his Ptolemaic adversary,
Simplicius; therefore Galileo’s was better. Because Aristotle’s account
of the natural motion of bodies required more ad hoc explanation
than the account in the first law of motion, Newton’s was better.

Hanson subscribed to the position that explanation involves
reduction to the familiar: “An event is explained when it is traced to
other events which require less explanation; when it is shown to be
a part of an intelligible pattern of events.” “What then is the
familiar?” we might ask. On the one hand, he says it is that which is
too commonplace to excite our curiosity—we know it when we see
it. On the more interesting side, being familiar with a conceptual
pattern is to know what happens under what circumstances; one is
capable of tracing in thought the future behavior of the system. Pheno-
mena are rendered intelligible when we are capable of seeing them
in analogy with the familiar. Psychologically, this entails a feeling of
familiarity and comfort with the phenomena and a comprehension of
how the phenomena “hang together”; logically, it implies some
capacity to make hypothetical forecasts.

Intelligibility is clearly not as closely tied to the actual state of
affairs in the world as objectivity is. One can conceive of a perfectly
intelligible story that one nonetheless recognizes as false and nonob-
jective—for a theory to be intelligible, it need not be objective; how-
ever, in order to be objective a theory must be intelligible. An intel-
ligible pattern of concepts or explanation tells us what we could
expect to observe under some precisely specified circumstances. For
instance, Dirac’s speculation that there existed a subatomic particle,
differing only from: the electron with respect to the polarity of its
charge, was therefore intelligible because it forecasted what would be
observed in future experiments.

There is a sense in which explanations, models, or theories either
are intelligible or they are not. However, this strict binary rendering
of intelligibility is seldom of any real interest. Instead we are inter-
ested in determining which conceptual organizations are most intel-
ligible, and after that: we concern ourselves with the question of
which model is most objective. While it seems reasonable, and
tempting, to say that the most intelligible model is the one with the
largest set of hypothetical empirical claims, there is something artifi-
cial, if not false, about speaking of numbers of claims. However, we
can appeal to the notion of explanatory unification and say that the
most intelligible model or theory is the one with the greatest
capacity for explanatory unification.

A frequently overlooked feature of Hanson’s philosophy of sci-
ence is the distinction he draws between pattern statements and
detail statements. Pattern statements make assertions about
the conceptual ordering of an area of phenomena, while detail state-
ments make empirical claims in light of the scheme according to
which the phenomena are organized. Both types of statements are
empirical since they answer to observation, but they are tested and
evaluated in different ways. Organizing patterns or Gestals tell us
how to appreciate the details of the pattern, i.e., they provide condi-
tions for determining the truth values of detail statements. Pattern
statements answer to observation in a very different, much more
complicated, way: rival patterns are appraised on the basis of their
capacity and track record for making the world intelligible, for
explaining the phenomena. The hypothetico-deductive method (H-
D method) provides an adequate account of the logic of detail state-
ments, whereas considerations of explanatory unification provide the
logical framework for decisions about pattern statements.

To those accustomed to thinking of Hanson as a critic of the H-D
method, such statements as the following will be perplexing: “Most
features of the anatomy of theories receive a clear elucidation within
hypothetical-deductive accounts.” Hanson thought that the H-D
method provided adequate explication of the process through which mature theories—those possessing a clear inference structure—are to be tested and appraised. However, he thought that the method was deficient since it only describes the anatomy of a theory, and says nothing about a theory's generation, growth, and development.

Hanson found fault with the H-D method only because of what it lacked, namely, a normative account of theory construction. He pointed out that there is no inconsistency between the H-D method and either inductivism or abductionism. H-D theorists just obstinately denied the possibility of any logical analysis of theory construction, but there was nothing intrinsic to the H-D model that barred the normative analysis of discovery. Moreover, Hanson found the notion that scientists deal only with fully-formed, closed logical systems unbelievable. While it is true that abstract inference structures are utilized in the testing of theories, the occurrence of anomalies, i.e., observational implications that are not realized, forces the scientist to retreat back to the psychological plane in order to refine the theory. At this time, the scientist either alters the conceptual framework by adding new elements to it or by replacing items in the old framework; it may even seem necessary to create an altogether new framework. The provision of an inference structure capable of being tested is something that only occurs after much conceptual travail; further, testing is a procedure that is intimately connected to revision, extension, and tweaking of theories, and it struck Hanson as misleading to pretend that testing is a purely logical or formal affair.

Why does it matter that Hanson subscribed to the H-D method? In this context, it shows that analysis of the inference structures of theories was possible and further that the H-D method gives us a method of appraising mature frameworks. Furthermore, it shows that he did not subscribe to a global incommensurability thesis, and therefore believed that justification and comparison of mature theories is possible.

The Case of N-Rays: Theory-Vitiated Observation or Delusion?

It has been argued that theory-informed observation does not endanger scientific objectivity provided that the results of such observation can be expressed in terms of intersubjectively testable consequences. It would seem that if theory-laden observation really were the threat to objectivity it is usually represented as being, we should find several clear historical cases where overly theoretically loaded observations led to epistemic disaster. In what follows, I offer what I regard as a typical historical episode in which theories seem to have temporarily compromised objectivity through their penetration of observational processes; I show, however, that theoretical influence on observation is made harmless by consideration of practical test implications and that this result is largely consistent with a Hanson-inspired philosophy of science. Thus, the case supports the conclusion that the theoretical loading of observation is not problematic when a theory is associated with an inference structure that is practically testable.

René Blondlot's purported discovery of N-rays in 1903 is a colorful example of the excesses to be wrought in science by a personal stubbornness and nationalistic pride bordering on delusion. In the course of investigating X-rays, Blondlot thought himself to have stumbled upon a completely novel form of radiation, the N-ray. Blondlot noticed that the sparks produced in his spark gap detectors, which were being used to determine whether X-rays exhibited polarization effects and were therefore composed of waves, were made brighter by the introduction of incident X-ray radiation. However, he also noted that the radiation could be bent by a quartz prism, a type of behavior that had been conclusively ruled out for X-rays by earlier experiments. As a result of these observations, Blondlot concluded that he had discovered yet another new form of radiation.

After the discovery, Blondlot and a legion of other French scientists began identifying N-ray sources (both natural and artificial) and catalogued the many remarkable properties of N-rays, which included the capacity to make one's vision more acute. This apparent triumph of the French intellect quickly turned into a disgrace when the American physicist R. W. Wood visited Blondlot's lab after having failed to replicate the phenomena in his own lab. The skeptical Wood quickly noticed that the sparks produced in his spark gap detectors, which were being used to determine whether X-rays exhibited polarization effects and were therefore composed of waves, were made brighter by the introduction of incident X-ray radiation. However, he also noted that the radiation could be bent by a quartz prism, a type of behavior that had been conclusively ruled out for X-rays by earlier experiments. As a result of these observations, Blondlot concluded that he had discovered yet another new form of radiation.

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blocking them. All of Blondlot's experiments were conducted in dark rooms, in order to minimize the interference of visual light sources, and this circumstance allowed Wood, the famous showman and skeptic, ample opportunity to vary parameters of the experiments without the observers' knowledge. Wood engaged in a scientific game of "rock, paper, scissors" with Blondlot's group by having them attempt to detect when he blocked the N-ray beam in a dark room. Wood noticed that there was no correlation between the position of his hand and the alleged presence of the N-rays, as evidenced by the intensity of the sparks of the detectors. Eventually, Wood kept his hand out of the beam altogether, but this did not stop Blondlot's group from observing changes in intensity.

Another observation of N-rays was performed by exposing photographic plates with the spark gap detectors. As the theory had it, the plates would be more exposed when the N-rays were incident on the detector. What is more, the plates that were exposed to the N-ray-enhanced sparks were indeed more highly exposed than those that were not. Wood conjectured that the experimenters were, perhaps subconsciously, holding the plates under the detector a tiny bit longer when they knew the N-ray beam to be on. In order to ensure a test that was impregnable to theoretical bias, Wood suggested producing two sets of outwardly indiscernible photographic plates, one set with a hidden N-ray filter, the other without. Blondlot, however, declined to undertake this crucial experiment.

We need not pursue the other tests of the N-ray theory, since it is clear the theory had spawned an inference structure amenable to intersubjective testing. That Blondlot and his associates were so deluded that they observed what they wanted to observe provided no barrier to the ultimate refutation of the theory. While Blondlot's own reluctance to concede defeat is indicative of his pride, his refusal to pursue the crucial examination of his theory was not based upon a commitment to the incorrigibility of his own observations. Blondlot's refusal to subject the N-ray hypothesis to the crucial test procedure proposed in 1906 by the *Revue Scientifique* indicates that his grounds for belief in N-rays were no longer based on a strategy of seeing that.

Permit me to decline totally your proposal to cooperate in this over-simple experiment; the phenomena are much too delicate for that. Let each one form his personal opinion about N-rays, either from his own experiments or from those of others in whom he has confidence. Blondlot still ordered his perceptions with the N-ray pattern, though he recognized, perhaps, that this pattern was too weak to underwrite intersubjective testing.

Given the delicate nature of N-rays, and the crudity of the available measuring instruments, it would appear to be a case in which theoretical biases were capable of decisively cleaving the believers from the unbelievers. Thus, it looks exactly like the type of case that would worry those concerned that the theoretical loading of observation threatens scientific objectivity. However, as we have seen, because the N-ray theory was sufficiently developed to be associated with an inference structure, H-D testing was sufficient to discredit the hypothesis. Even though Blondlot stubbornly persisted in believing in N-rays till the end of his life, his reasons for having done so were clearly irrational—far from sheltering scientists like Blondlot from criticism, Hanson's full account of theory-laden observation provides criteria for distinguishing rational and irrational strategies of observation and theoretical commitment.

**CONCLUSIONS**

Hanson's epistemology provides a suggestive account of how new theories are constructed and how surprising and intangible phenomena are rendered intelligible. The principal critical reaction to Hanson's work has been committed to false assumptions about his understanding of intelligibility, conceptual structure, explanation, and theoretical testing. Given that his account provides the resources to support scientific objectivity, analysis of the thesis of theory-laden observation should focus on elaborating the logical status of discovery and the relation of psychology and logic in methodology; these elaborations are the subject of the next chapter.

In the decades since Hanson wrote, much progress has been made in the study of the ways humans actually categorize items and the cognitive structure of concepts, analogy, and metaphor. Empirical investigation has thus provided important insight into descriptive epistemology; however, it is still uncertain whether cognitive science can illuminate normative epistemology. Can the relation of psychology and
logic be clarified to the extent that psychology would be able to fill in
the logical gaps in scientific methodology that Hanson was concerned
about? This important theme is the subject of chapter 7.

Notes
1. Hanson, "Observation and Interpretation," Voice of America
Forum Lectures: Philosophy of Science Series 9 (Washington, DC: US Information
Agency, 1964), p. 1. This essay is the transcript of Hanson's Forum Lecture
that was broadcast in 1963. The lecture was intended to be self
contained, and also attempts, though in an oblique way characteristic
of Hanson, to answer objections to his view. Also, the version of the theory
laidness thesis he put forward there is bolder and less tentative than the one
presented in chapter 1 of Patterns of Discovery (Cambridge: Cambridge University
Press, 1958). Perhaps Hanson was goaded by the following critical comment by Feyerabend on his Wittgensteinian overqualifications: "I do not
like the completely useless qualifications which occur . . . throughout [Pat
terns of Discovery]; results are not stated in a straightforward way but are pre
fixed with such phrases as 'there is a sense in which' and the like. Such restric
tions, which are rather common among Wittgensteinians and which are sup
posed to show that the author is conscious of the complexity of a problem,
are of no help at all unless it is specified what the senses are in which the state
ment made is not correct. It is as if somebody in a zoo were to say, not 'This
is an elephant,' or 'This is a giraffe,' but rather 'In a certain sense this is an
elephant' and 'In a certain sense this is a giraffe.'" Paul Feyerabend, review of
Patterns of Discovery, by N. R. Hanson, Philosophical Review 69 (1960): 248.
2. Ibid.
3. Carl Korth, The Justification of Scientific Change (Dordrecht: D.
Reidel, 1971).
4. Abner Shimony, "Is Observation Theory-Laden? A Problem in
Naturalistic Epistemology," in Search for a Naturalistic World View (Cam
bridge: Cambridge University Press, 1993). The argument in the present
chapter shows that Hanson's account of theory-laden observation had the
resources to account for objectivity.
5. Israel Scheffler, Science and Subjectivity (Indianapolis: Hackett,
6. Harold Brown, "A Theory-Laden Observation Can Test the
Theory," British Journal for the Philosophy of Science 44 (1993): 555-59, and
"Circular Justification," Proceedings of the Philosophy of Science Association 1
7. See Franklin et al., "Can a Theory-Laden Observation Test the
8. See Alan Chalmers, Science and Its Fabrication (Minneapolis: Univer
sity of Minnesota Press, 1990) and "The Theory-Dependence of the Use
9. Hanson, Observation and Explanation (New York: Harper & Row,
10. Ibid., p. 43.
11. Ibid., p. 44.
12. Hanson, Patterns of Discovery, pp. 18-19.
13. Hanson, Observation and Explanation, p. 78.
14. I am here using terminology that Hanson did not use; I do so both
because of its clarity as well as its linkage to the cognitive scientific theories
of analogy presented in chapter 7. In Hesse's terminology, candidate infer
ences are "neutral analogies." Keynes would have called untested candidate infer
ences "unknown analogies." See John Maynard Keynes, Treatise on
15. Even if the candidate inferences are not affirmed, one could still say
that the target representation is intelligible since it gives an account of what
would occur in what circumstances; it just happens that the account is false.
16. Hanson, Patterns of Discovery, p. 86. This early remark of Hanson's
demonstrates how intimately he considered theory-laden observation and
the process of discovery to be related.
17. Hanson, Perception and Discovery (San Francisco: Freeman,
18. Ibid., p. 185.
19. Hanson, Patterns of Discovery, pp. 20-21.
C. Hartshorne, P. Weiss, and A. W. Burks (Cambridge, MA: Harvard Uni
versity Press, 1931-58), CP 5.401-402.
21. That is, the conditional statements are purged as long as their
removal does not destroy the coherent structure of the concept.
23. Christopher Norris, "The Expert, the Neophyte, and the X-Ray
24. Hanson, Perception and Discovery, p. 119. Italics were added to the
quotation to make it consistent with the standard adopted regarding the
use/mention distinction.
25. Hanson, Patterns of Discovery, p. 21.
26. Hanson, Observation and Explanation, p. 64.
27. Hanson, Patterns of Discovery, p. 14.
28. To be sure, though, the capacity to answer such questions provides
others with a means of measuring one's knowledge.
29. Hanson, Patterns of Discovery, p. 16.
30. Ibid., p. 22.
When discussing aspect shifts, Hanson asserted that nothing phenomenological changes during such transformations: "When attention shifts from the cup to the faces does one's visual picture change? What is it that changes? What could change? Nothing optical or sensational is modified. Yet one observes different things. The organization of what one observes has changed." Hanson, "Observation and Interpretation," p. 5, italics added. It appears reasonable that any change in the phenomena in the H-D model is a necessary condition for the "optical" or "sensational"; thus, is unaffected by theoretical commitments, and, even though he did not take the thesis to be of much epistemic significance.


53. Norris draws the opposite conclusion to Radder: "The whole drift of Hanson's argument—like Wittgenstein's before him—is to assimilate scientific enquiry to that sub-branch of behavioral science which examines the processes of visual perception from a Gestalt-psychological perspective." Norris, "The Expert, the Neophyte, and the X-Ray Tube," pp. 131-32.

54. Hanson, Patterns of Discovery, p. 17.

55. These notions figure especially prominently throughout Observation and Explanation.

56. We might nowadays wish to broaden the notion of the embedded representation to include the details concerning a theory's social, historical, and practical embodiment. Hanson gestured at the significance of social psychological factors in his conclusion to The Concept of the Postulate (Cambridge: Cambridge University Press, 1963): "Perhaps [social psychological] factors affect the growth of science as strongly as does any strictly conceptual or theoretical development" (p. 165). While this sedate conclusion indicates that Hanson was taking social factors more seriously in 1963, he clearly regarded the sociological as extraneous to scientific representations.

57. Hanson, Observation and Explanation, p. 68.

58. In chapter 6, the idea of the "implicit structure" of a theory will be discussed, a notion that is closely related to psychological structure.

59. Hanson's account viewed past scientific disputes as having been waged by partisans of rival frameworks, frameworks that could not, in any straightforward sense, be evaluated by simply being compared with the facts. His account seems to fit rather well with the history of science. It is worth noting that the critical reactions to Hanson's work almost never concern his historiography or the interpretations his account gives of historical episodes—the criticisms are nearly always concerned with matters of pure philosophy—logic, philosophy of language, objectivity, rationality, and ontology.

60. This is closely related to the point made by Shapere that we only need retreat to more phenomenal "sees that" statements when we have reason to doubt the reliability of statements more heavily laden with theory.

61. Such an account of intelligibility has been recently put forward by Henk De Regt and Dennis Dicks in "A Contextual Approach to Scientific Understanding," Synthese 144 (2005): 137-70. De Regt and Dicks argue that scientific understanding involves recognition of the qualitative behavior a system will display under a variety of conditions; though they offer up a considerably more detailed account than Hanson and are more concerned with context, the fundamental elements of the views are similar and Hanson's account seems acceptable to the same degree as is the account of De Regt and Dicks.

62. That is, he did not use it in the sense in which it is most often used by contemporary philosophers. Hanson used the term to describe physical systems whose properties can be ascertained without the measuring apparatus contributing anything. See Hanson, Concept of the Postulate (Cambridge: Cambridge University Press, 1963), p. 80.

63. Hanson, Patterns of discovery, p. 95.

64. Ibid., p. 94.

65. As will be seen in chapter 6, this was so even though Dirac himself was initially more embarrassed at the apparent necessity of this speculation than he was enthusiastic about its chances of being experimentally verified. As Hanson put it, Dirac "made every conceivable effort from 1928 until 1931 to 'cook away' this awkward theoretical sleight within his otherwise spectacularly successful electron theory." Hanson, "An Anatomy of Discovery," Journal of Philosophy 64, no. 11 (1967): 337.

66. See Hanson, Patterns of Discovery, pp. 87-88.

67. Hanson, Observation and Explanation, p. 63.

68. Hanson even asserted that the possession of an inference network of the kind associated with the H-D model is a necessary condition for something to count as a scientific theory. See Observation and Explanation, p. 47.


70. It may still be the case that science is subjective with respect to theory choice, but if observational consequences can be spelled out, then such subjectivity derives from something other than observation.


72. The procedure recommended was the one put forward by Wood.

73. Quoted in Klotz, Diamond Dealers and Feather Merchants, p. 63.
Chapter 4

Logic of Discovery

Do not throw the whole problem of discovery too quickly into those large, dark receptacles called “intuition,” “inspiration,” “guesswork” and “hunch”—or even “paradigm.”
—N. R. Hanson, “Commentary,” in Scientific Change, ed. A. C. Crombie, p. 461

INTRODUCTION

Since the middle of the nineteenth century, the view that the discovery of scientific theories lies outside the pale of rational or logical analysis has enjoyed a nearly unprecedented status of orthodoxy among philosophers. On this orthodox view, logic can only lay hands upon scientific inquiry after a theory or hypothesis has been produced—until then, all bets are off, as far as logic is concerned. Despite this belief in discovery’s rational intractability, the history of science has been centered on the great discoverers and, indeed, has revered them as paragons of rationality.

The coexistence of the beliefs that discovery processes are not rationally appraisable and that the rationality of the great discoverers is worthy of veneration and emulation ought to surprise us—it certainly surprised Hanson and motivated him to develop his notion of the logic of discovery. Hanson forcefully defended the thesis that discovery has a rational aspect:

The original suggestion of a hypothesis is often a reasonable affair. It is not as dependent on intuition, hunches, and other impoerables as historians and philosophers suppose when they make it the province of genius but not logic. . . . Perhaps only Kepler, Galileo.
analitical reasons for proposing H, just after 1609 were good ones. But, logically, they would not have been good reasons for asserting the truth of H—something which could be done confidently only years later. 18

In retroductive inference, we reason from the conclusion back toward some initially unknown premises; to use Hanson's expression, “one reasons from the bottom of the page to the top.” 9 In the H-D method, one reasons from the top to the bottom. In terms of logical form, one could be dealing with the same argument in either case; however, in the case of retroductive inference, one is supplying missing premises, while in the H-D case, one derives the conclusion. In these two situations, the moves from what we know to what we do not know are very different; thus they have different logical characteristics. Hanson showed how Newton's inference to the inverse square law of universal gravitation proceeded according to the retroductive schema.

To Halley's query, in 1680, as to what path a planet moving under the attractive force of an inverse square form would have, Newton directly answered that it would be elliptical. Had the query been understood from a purely geometrical perspective, as would have been appropriate considering Newton's status as the foremost geometer of his day as well as his predilection for the Euclidean mode of exposition, one would have expected Newton to have given the correct general geometric answer, namely, that such an orbit would be a conic section. Instead, Newton asserted that a particular conic section, the ellipse, was the correct form of the path. Why did Newton reason so directly when strict adherence to geometrical demonstration demanded a more generic answer? An obvious factor was that Newton already knew the orbits to be elliptical. He reasoned from this conclusion to a set of premises from which it followed. Thus, he reasoned from the bottom of the page to the top. More interestingly, though, Hanson showed that Newton reasoned his way to the top of the page using explicitly physical thinking by combining Kepler's third law and Huyghens' law. Here is Hanson's reconstruction of the reasoning process:

Given an egg-shell, elliptical in section (rather than oviform), imagine a marble moving inside that shell (with velocity sufficient to keep it in the maximum elliptical orbit.) What force must the egg-shell exert on the marble to keep the latter in this path?

Huyghens' weights, when whirled on strings, required a force in the string, and in Huyghens' arm, of \( F_{(K)} = r/T' \) (where \( r \) signifies distance, \( T \) time and \( k \) is a constant of proportionality). This restraining force was needed to keep the weights from flying away like catapulted projectiles, and something like this force would thus be expected in the egg-shell. But from Kepler's third law we know that \( T' = r \). Hence \( F_{(K)} = r/k = 1/r' \). The force the shell exerts on the marble varies inversely as the square of the distance of the marble from that focus in which the sun rests. This follows by retroductive reasoning. 11

As Hanson noted, Newton had indeed engaged, very early on, in a thought experiment very like that described above concerning the inverse square nature of the force required to keep the moon in its orbit:

And in the same year [1665, twenty years before the Principia] I began to think of gravity extending to ye orb of the Moon, and (having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere), from Kepler's rule . . . I deduced that the forces which keep the planets in their Orbs must be reciprocally as the squares of their distances from the centres about which they revolve. 12

In this case, the retroductive analysis gives insight into the history of science while clarifying the logical character of Newton's discovery process. It tells us how Newton acquired his hypothesis, not merely what features the hypothesis had once acquired. We might conclude from this case that Hanson thought that the logic of discovery might tell us how to go about discovering new theories, as "providing rules to guide the perplexed towards making discoveries." However, Hanson, as shall be shown presently, resisted this conclusion.

In his excellent discussion of "The Logic of Discovery," Donald Schon, in a manner reminiscent of Margaret Masterman's critique of Kuhn's terminological liberty in Structure of Scientific Revolutions, pointed out that Hanson had conflated a variety of different definitions of the logic of discovery. According to Schon, Hanson was not clear as to which of the following items the logic of discovery was to apply:
reasoning behind the original suggestion of certain hypotheses [p. 1074];
reasons for suggesting H originally, or even for formulating H [p. 1074];
reasons for formulating and entertaining hypotheses [p. 1075];
reasons for entertaining a hypothesis [p. 1077];
reasoning which often conditions the discovery of laws [p. 1082];
[how] scientific discovery actually proceeds [p. 1080];
an account of the way in which hypotheses in science are actually discovered [p. 1084].

Schon’s guess at Hanson’s conception of the logic of discovery was explicitly endorsed by Hanson in his response:

At times, Mr. Hanson treats the logic of discovery as though it were nothing more nor less than the logic of good reasons for proposing a scientific hypothesis, a sort of logic of preliminary evaluation as opposed to the logic of secondary evaluation which is the logic of proof. It is treated as a logic of processes of evaluating what has already come to mind.

Schon noted Hanson’s disdain for the idea of a discovery manual and wondered on what logical grounds Hanson excluded such an idea from the logic of discovery. Why, he asked, would the logic not apply to the actual processes of discovery? Schon argued that since “scientists can and do, in a relevant sense, bethink themselves of ideas which suit their purpose,” the process of formulating hypotheses might be something the logic of discovery should deal with. Furthermore, he argued that works like those of Hadamard and Poincaré on the nature of discovery, works that Hanson dismissed as being concerned with only psychological and sociological matters, do advance normative claims and, hence, ought to be considered as belonging to the “methodology” of discovery, if not the logic of discovery.

In his response, Hanson rebuked Schon for “melting together” “the processes genetically responsible for the suggestion of H” and “the justification for suggesting H in the first place.” Hanson then made it absolutely clear that he did not view the process of hypothesis generation to be the business of a logic of discovery: “[Schon argued] that something logical underlies the processes which lead to the initial suggestion of an H. I doubt this.” To Schon’s suggestion that the genesis of a hypothesis can be rational, Hanson noted that even an unjustified hypothesis has a genesis, as though such an observation ruled out altogether the question whether logical criteria can be applied to the processes through which a hypothesis is generated. This, again, seems like more of an assumption or Hanson’s part than an argument.

In response to Schon’s criticism, Hanson weakened his conception of the logic of discovery to being essentially a “logic of plausibility.” He retreated to the view that there can be good reasons to suppose that a hypothesis type is plausible before testing. Basically, Hanson’s modifications made it so that a number of his different formulations of good reasons, which were pointed out as being inconsistent by Schon, became equivalent. However, it seems that the notion that psychology and logic can both provide analysis of a thinking process makes it possible for the logic of discovery to apply to the actual processes of hypothesis generation, even in particular cases. Furthermore, if the psychological and the logical are dealing with the same phenomenon, even the generation of a theory might be treated from a logical perspective, as Hanson often asserted (though not entirely consistently) was possible.

Hanson’s goal was to point out the incompleteness of both the H-D and the inductivist methods: neither of them gives an adequate account of the logic used in suggesting hypotheses. H-D theorists simply deny there is any logic there at all, while inductivists offer up a factually and logically false account of the discovery process. However, since Hanson was unable to bring his logic of discovery program to fruition and often waffled on extremely critical points in his arguments, his inquiry into the nature of the logic of discovery has been principally remembered as a critique of existing canonical views and not as a free-standing line of inquiry to be taken up by subsequent philosophers. One of the principal objections made by philosophers has been that Hanson’s logic of discovery fails to be a logic at all, and to this issue we now turn.
Is It a Logic?

Much of Hanson's burden in arguing for a logic of discovery was to convince his readers that we are really dealing with logic when new hypotheses are generated and entertained. Hanson's view is founded upon the assertion that where there are good reasons for $x$, there is a logic of $x$:

If the statement "A is a good reason for $H$" is true, it is logically true—even when the reasons are inductive. If I say "Jones has good reason for $H$," that is contingently true, if true at all. Jones could have had other reasons for $H$. But this statement is logically different from "A is a good reason for $H$." What are and are not good reasons is a logical matter.

Thus, Hanson made it clear that all claims about good reasons are logical truths, even if one's reasons are not deductive. One of Hanson's assumptions in his discussions of the logic of discovery is that informal logic is a branch of logic. While he was as aware as anyone of the formidable obstacles to a satisfactory justification for informal logic, he was not concerned to justify the different varieties of informal argument. Thus, the discussion, as he saw it, is concerned with determination of whether inferences used in discovery fall under the already accepted definitions of logic, and not explicitly with justification of novel discovery logics.

For Hanson, the logic of discovery is not necessarily some novel logic, unlike anything we have ever seen before. All of the logics of discovery he outlines are garden-variety informal arguments. His purpose was to show that these arguments are different in kind from H-D justifications and testing of hypotheses. Hanson affirmed the H-D account of justification since he believed that hypotheses could be established inductively; however, since informal discovery arguments could not themselves establish hypotheses (though they may give us some reason for accepting, not simply suggesting, them), Hanson argued they had to be of a distinct logical type:

Analogical, symmetry, and authoritative arguments could never by themselves establish an $H$. Inductive arguments can, by themselves, do this. So they must be different in type. Though any of these could make it reasonable to propose an $H$, only the inductive argument can by itself establish hypotheses.

Classically speaking, logic is just the study of good reasoning, and it is concerned to distinguish good patterns of reasoning from bad ones wherever possible. Formal logic has developed an imposing battery of methods, definitions, and exemplary demonstrations. Because the machinery of formal logic has produced such extraordinary results, philosophers have come to accept that anything else that aspires to be a logic should have such machinery as well.

Because Hanson was objecting to the H-D method, he attempted to demonstrate that the logical nature of the processes leading to discovery are distinct from the logic of justification; he even argued that while it is possible for the logic of discovery and the logic of justification to be concerned with one and the same argument, the two logics are nonetheless distinct. Discovery inferences might be justified by the principle of induction, but that fact alone does not mean that they are best described as inductive inferences. This is so because their inductive features may not fully express what makes the inferences persuasive.

A typical objection to Hanson's logic of discovery program is that it is not rigorous enough. This certainly was a criticism to which Hanson was sensitive, since he attempted some rigorous defenses; however, all of his moves toward increasing rigor were moves away from historical accuracy—a not uncommon trend in philosophy of science. In addition, he often purchased more rigor at the expense of making the logic of discovery less precise and less constrained. For instance, Hanson argued that a retroductive inference, i.e., an inference in which a set of premises are discovered from which a conclusion can be deduced, is conceptually quite different from an identical argument construed as a simple deductive inference. Hanson provided a modal argument for the conceptual distinctness of these two processes of inference that arrive at an identical result. When deducing conclusions from a consistent set of premises, one is constrained by the law of noncontradiction; all of the possible conclusions derivable from a consistent set of premises are themselves consistent. Conversely, when one reasons from a conclusion to a set of premises from which the conclusion may be deduced, there will exist many candidate sets of premises that will work, and these sets can contradict one another: "All conclusion-sets derivable from consistent premises are themselves compatible. But not all premise-sets from which a given conclusion-set is derivable will themselves be
compatibility." So, all the possible statements to be inferred in deduction must be consistent with one another, whereas in retroduction there is no such constraint. Thus, retroduction, as a process of reasoning, is considerably less constrained than deduction. Therefore, Hanson concluded, the two processes of reasoning are conceptually distinct.

Notice that Hanson was only able to distinguish the retroductive argument by showing what features of the deductive argument it lacked—his account offers no insight into how retroduction actually proceeds. Many of his examples, particularly Newton's thought experiment concerning the inverse square law, are impressive precisely because they illuminate the steps leading to the top of the page, i.e., to the hypothesis. It was not simply the hankering after rigor that made Hanson's project become progressively less appealing. Instead, it was simply that he went after the rigor of deductive and modal logic rather than seeking the maximal degree of rigor available within informal logic. In chapter 7, it is shown that innovations in the study of analogy and the psychological structure of concepts provide rich enough structures of representation to enable a rigorous, though informal, logical analysis of discovery.

Another reason philosophers are reluctant to concede that discovery is rationally appraisable has to do with the imperfection of the relevant historical evidence. It is possible that some discoveries are, for practical reasons, beyond the grasp of descriptive analysis; however, this does not mean that such discoveries are outside of the bounds of rationality. For instance, let us consider a case in which a scientist discovered a theory long ago without having left behind any evidence from which a later description could be produced. In such a case, the mere shortage of evidence is no reason for us to suppose that the discovery was either irrational or beyond the scope of logical appraisal. We may have reasons to doubt that the theory was in fact rationally devised, but the simple paucity of evidence does not affect claims about how things actually happened, but only our knowledge of what happened. Of course, the imperfection of historical evidence is a serious problem in the sense that it threatens to make the logic of discovery, if it exists, a program that cannot be especially illuminating about the history of science.

Most philosophers and scientists today would not object to the claim that there can be good reason for initially proposing a hypo-

thesis. However, if one then goes on to say that there is, therefore, a logic of discovery, since logic is after all concerned to distinguish good reasons from bad ones, disagreement and impatience result. It is protested that a logic is much more substantial than a mere bifurcation of reasons into the good and the bad. If one has a logic, one has methods of proof, perhaps even effective methods, and one has a justification for these methods. Additionally, a logic must contain a syntax and a precise delineation of which kinds of syntactical structures are well formed and which ones can be inferred from others. The very demand that the proponents of a logic of discovery ought just to spell out their logic makes clear how deep the chasm is separating the believers and the skeptics.

The critics go on to say that logic is not simply concerned to distinguish good from bad reasons; it must also provide an account of why a particular reason or inference is either good or bad, i.e., a logic must provide an account of justification. Put alternatively, a list of good reasons does not qualify as a logic; we also require a principle that explains why certain reasons qualify as good reasons. Deductive logic is so exemplary because it contains such a clear and rigorous principle of argumentative goodness in the notion of truth preservation. If the argumentative strategies Hanson alleges to be logics of discovery are to have any hope of being accepted, their conditions of justification would have to be spelled out. In chapter 7, I will attempt to provide a full justification for arguments from analogy.

**THE DISCOVERY-JUSTIFICATION DISTINCTION**

A standard objection to the idea of a logic of discovery is that the existence of such a logic clearly violates the discovery-justification distinction, a positivist dogma that has fared much better than positivism itself. Most analytic philosophers associate an epistemic distinction between discovery and justification with Hans Reichenbach. However, the idea that theories are to be accepted only on the basis of their conformity with the evidence and that the actual processes of discovery are epistemically irrelevant began enjoying its orthodox status for more than one hundred years before Reichenbach offered his conception of the two contexts. Unfortunately, Reichenbach's distinction is sometimes misunderstood as differentiating two sepa-
rate temporal situations. Originally, Reichenbach took the context of discovery to be the actual historical situation, broadly construed, within which a theory arises and develops. The context of justification, on the other hand, is an analysis of the logical structure of the theory, and as such is nontemporal; if the empirical claims inferred from the theory are true then the theory is (at least incrementally) confirmed. Following Frege's stricture that logic is concerned merely with the formal structure of correct reasoning and not with the historical/psychological processes of reasoning, Reichenbach had to say that the context of discovery, as he had defined it, lay outside the scope of logical analysis. However, Reichenbach actually held that the process of theory creation could be rationally reconstructed and thereby analyzed in the context of justification. In fact, as Nickles points out, Reichenbach's straight rule of induction "constitutes the basis of a logic of discovery in the strong sense of a logic of hypothesis generation." 27 Reichenbach himself makes the following avowal of his belief in the rationality of discovery:

Men of scientific research are not always of so clear an insight into philosophical problems as logical analysis would require: they have filled up the world of research work with mystic concepts; they talk of "instinctive presentiments," or "natural hypotheses," and one of the best among them told me once that he found his great theories because he was convinced of the harmony of nature. If we were to analyze the discoveries of these men, we would find that their way of proceeding corresponds in a surprisingly high degree to the rules of the principle of induction, applied however to a domain of facts where average minds did not see their traces... The mysticism of scientific discovery is nothing but a superstructure of images and wishes; the supporting structure below is determined by the inductive principle. 28

The current received view concerning the rationality of scientific discovery and justification is the result of the unholy union of the nineteenth-century sentiment that theories be appraised only on the basis of their fit with the facts and the dogma, associated with Reichenbach, that the contexts of justification and discovery are logically incommensurate. This fusion has led to the view that the process of discovery is not rationally reconstructible—a view to which neither Reichenbach nor many nineteenth-century scientists would have subscribed. Most likely, the standard contemporary reading of the context distinction, especially as regards the logic of discovery, is due less to Reichenbach than to Popper, who of course eschewed all talk of induction and rational methods of theory generation. 29

To conclude this section, the received context distinction is very different from the original from which it is derived. The deviations from the original obtain their main force from their capacity to effect disciplinary demarcation, not from any independent justification. Since the received view functions as a disciplinary marker, it has not been subjected to strong criticisms within philosophical circles. As historically problematic and protean as this context distinction is, it seems hardly capable of providing any sensible constraints for the analysis of discovery.

THE RECEIVED VIEW OF THE LOGIC OF DISCOVERY

In an important article, 30 Larry Laudan charts the historical development of the idea of a logic of discovery. He divides those thinkers who take science to be an a posteriori, empirical undertaking into two camps: the consequentialists and the generationists. The consequentialists believed that theories are to be justified by assessing their claims about observable consequences. Generationists held that theories are justified if they are inferred from observational data by the logic of discovery. In contradistinction to the received dichotomy between the contexts of discovery and justification, which supposes discovery (and any logic it may have) to lie outside the scope of justificatory measures, the generationists viewed the logic of discovery as providing the justification for theories. Simply put, if a theory is derived in the appropriate way (i.e., in accord with the logic of discovery), we are justified in accepting it.

The generationists were ascendant in the field of scientific methodology up until the beginning of the nineteenth century. At that time, according to Laudan, science ceased to be viewed as an infallible mode of inquiry. When science was seen as infallible, the consequentialists had little hope of winning much sympathy since their view appeared to commit the fallacy of affirming the consequent. All the true consequences in the world were insufficient to
guarantee the truth of the antecedents; thus, as a method of justification, consequentialism was predicated on an invalid form of inference and, as such, was a failure. However, once science came to be widely acknowledged as a fallible undertaking, the fortunes of the two schools of methodological thinking were instantly reversed. Since following the logic of discovery would not lead to an infallible theory, there was no longer a sense in which there was a single logic of discovery. If there were a multiplicity of distinct logics of discovery, all leading to different fallible theories, there would no longer be any place for theory justification in generation. Conversely, consequentialists were now reasoning in the correct direction, from the supposed antecedent (the theory) to the consequent (observational data). Since theories were taken to be fallible, the best one could do in justifying them would be to see whether the consequences were realized.

In Laudan’s view, the logic of discovery program died out because its rival offered an acceptable justificatory scheme where it did not. If there remain reasons for philosophers to investigate discovery, they will be discontinuous with the traditional motivations. Thus, Laudan concludes that, on our current understanding of the epistemology of science, hypothesis generation has no special philosophical significance; our view of science may change, but until proven otherwise hoth-cad generadoia is of includes that, on our current understanding of the epistemology of science, hypothesis generation has no special philosophical significance; he discontinuous with the traditional motivations. Thus, Laudan concludes that, on our current understanding of the epistemology of science, hypothesis generation has no special philosophical significance; our view of science may change, but until proven otherwise hypothesis generation is of no philosophical interest. For all his interesting historical insights, Laudan caves in to an utterly traditional view on the philosophical value of studying discovery:

Before one concludes that the logic of discovery still has a philosophical rationale, one must ask what is specifically philosophical about studying the genesis of theories. Simply put, a theory is an artifact, fashioned perhaps by certain tools (e.g., implicit rules of “search”). The investigation of the mode of manufacture of artifacts (whether clay pots, surgical scalpels, or vitamin pills) is not normally viewed as a philosophical activity.

In response to Laudan, we can acknowledge that theories are created artifacts, but they are artifacts of a very special type, since they structure our epistemological systems—to say that their mode of creation has no interesting relation to our system of knowledge is to assume a great deal. Laudan’s dismissal is predicated on his view of theories as artifacts, and this seems to be a rather spurious identi-

fication. We can grant that theories are epistemic artifacts, in the same way that language is an epistemic artifact; surely, philosophers are not so pure minded as to avoid analysis of language on account of its artifactual character. Thus, it seems appropriate to dismiss Laudan’s argument that theory generation is epistemically worthless simply because theories are artifacts. However, Laudan’s more general concern about whether analysis of discovery is epistemically valuable or not needs to be addressed.

Aristotle, Peirce, and Hanson have been taken to task for having actually produced not methods of theory generation, but of theory evaluation. Of course, such a criticism presupposes that evaluation and generation are distinct activities (either in the temporal, logical, or combined sense) and it is not clear that this is the case. In fact, outside of the rare “flash of inspiration” types of theory discovery, theory generation would seem to contain a very prominent evaluative component. In historical cases of theory generation, the steps in the process of construction are generally presented by the historians—or sometimes by the scientists themselves—in a roughly chronological pattern where each step is explained by reference to a set of goals, constraints, and new empirical data. Such a scheme of explanation, and presumably of motivation, is explicitly evaluative. This discussion points out the pernicious ambiguity in the distinctions underlying our talk of theory creation and evaluation, and highlights the urgency of the need for recasting the significant logical relations and temporal stages characterizing the life of theories. However, I believe that the confusion surrounding the relation of creativity to evaluation is not merely the issue of an unfortunate terminology. Instead, it seems the tendency to extract all that is articulate and comprehensible in a creative act as its factual context, in order to seal off in some indivisible germ of mystery the creative energy, a quasi-divine force, is a remnant of the romantic viewpoint.

As we have already discussed, Reichenbach saw the context of discovery as historical and the context of justification as logical. While these are indeed two distinct contexts, this does not entail that the contents composing them are disjoint. Just as a history of religion in medieval Europe would treat some of the same events as a history of economy for the same period, albeit from a different standpoint, so too can the historical unfolding of discovery be analyzed in the context of justification. In fact, Reichenbach asserted...
that there must be a correspondence between the two contexts in order for logical analysis of science to count as justification:

In spite of the justificatory task's being performed on a fictive construction, we must retain the notion of the descriptive task of epistemology. The construction to be given is not arbitrary; it is bound to actual thinking by the postulate of correspondence.

Laudan attempts to rectify the ravages wrought by an overly simplistic and generally confused notion of the logical and temporal interrelations of the stages in the theoretical life cycle. He conceives of the contexts of discovery and justification as marking out historical periods. In addition, he interposes an additional context, that of pursuit. He too claims that Peirce and Hanson were off base when they presented abduction as a logic of discovery, but this time it is a logic of pursuit, if a logic at all.

These three contexts mark the temporal, if not the logical, history of a concept. It is first discovered; if found worthy of pursuit, it is entertained; if further evaluation shows it to be worthy of belief, it is accepted.

While it may be useful to have this threefold division, it seems only to promote greater obfuscation to retain Reichenbach's terminology. Instead, we should perhaps speak of three periods: discovery, pursuit, and justification. Doing so will help us avoid a trap one can quite naturally fall into, namely, that of assuming that the existence of distinct stages implies the existence of distinct and constitutive logics, or patterns of inference, for the different stages. Instead, the truly interesting question regarding discovery—a question that cannot be framed without great danger of misunderstanding on all the available classificatory schemes—is whether there exist canons of inference in the process of discovery separate from those in justification and pursuit. To speak of all appraisal of inferential moves as justification, as Reichenbach does, is to lump what could be widely heterogeneous forms of reasoning under the same rubric. For instance, while the steps in a discovery process can themselves be justified, such justification is of an entirely different order than the justification of the theory said to be discovered.

If we adopt the Laudan-inspired conception of three periods in the history of a scientific theory, we can then investigate the types of reasoning process associated with each. I shall understand the term logic of discovery as the set of good reasoning routines that operate in the period of discovery; it is of course an open question as to whether there are any such sets of routines at all, and if so, whether they are unique to discovery.

Recent skepticism about the idea of a logic of discovery has been guided more by preconception than by explicit argument. When all of these preconceptions are spelled out, we have a conception of the logic of discovery that is obviously defective; however, it remains to be seen whether dispelling a collection of these preconceptions will leave us with a notion of logic of discovery worth having. What follows are a number of the most significant properties spoken of as characteristic of the logic of discovery:

(a) **Infallibility**: The logic of discovery is infallible in two senses: (i) it leads to true theories and (ii) employment of it inevitably leads to such theories. In other words, its results are infallible and it is infallible as methodology.
(b) **Uniqueness**: The logic is robust enough to arrive at a unique and fully determined solution. Thus, the data plus the logic combine to produce a single solution.
(c) **a priori** as a logic, it is taken to be independent of experience.
(d) **Content independence**: the logic is general enough to work for all branches of science.
(e) **Context independence**: the logic is general enough to work in all research, historical, social, psychological, and goal-oriented contexts.
(f) **Rule-governed, or algorithmic**: there exist sets of rules or finite sequences of steps leading to theories from data.
(g) **Logic of propositions**.
(h) **Totality**; the logic of discovery gives an exhaustive account of how all theories are generated.
(i) **Autonomy**; the logic of discovery is distinct from all other logics.

While these features of a logic of discovery were seldom made explicit, a significant collection of them was involved in all the cri-
tiques of the logic of discovery, (a), (b), and (c) are clearly unreasonable requirements for a logic of discovery in the sense understood after the fall of generationism. Hanson, for instance, explicitly denied (a) and (b) in his account of logic of discovery, and probably (c) as well. The weaker independence requirements (d) and (e) are also very doubtful, though we might justly expect to find regimes offering relative content and context independence. All investigations into discovery processes generally subscribe to (f), though they generalize from a logic of propositions only (g) to one that takes more complex structures as arguments.

While almost no post-eighteenth-century thinker would have acceded to the totality requirement (h), examples of chance, serendipitous, or counterintuitive theory generation are often offered up as proof that there is no logic of discovery. However, one can infer true conclusions from invalid arguments, but this fact in no way impugns the adequacy of deductive logic. That there are discoveries that are produced without reasoned processes hardly entails that all of them lie beyond the grasp of reason or that they all contain an "irrational element."45

Finally, (i) expresses the peculiar presupposition that in order for the logic of discovery to qualify as a bona fide logic, it must be distinct from other logics, the logic of justification in particular. Such an assumption is dubious, not only because it makes the historically and conceptually problematic context distinction central to a logical investigation of discovery, but because it assumes that where there are different contexts, there are (to be) different logics. Strangely, most of the criticism explicitly directed at Hanson’s account has focused on the nonautonomous character of the logic of discovery, even though Hanson acknowledged the logic of discovery’s lack of autonomy from his first article on the subject on.

**Revised Logic of Discovery**

As we have seen, most of the preconceptions traditionally associated with the notion of a logic of discovery are unsupportable. It remains to be seen whether the rejection or modification of these requirements will still leave us with something useful and rigorous enough to qualify as a logic of discovery.

Clearly, the infallibility requirement, in both of its senses, must be rejected (as indeed, it was by Hanson). Since science itself is fallible, its method of discovery must be as well. The assumption that logical inferences must be infallible has been responsible for our not having a deep understanding of the reasoning processes actually utilized by scientists. Thus, a nonformal conception of logic, for instance that of the early Peirce, is more likely to be able to make sense of discovery, as well as other a posteriori forms of scientific reasoning. According to this conception, logical inquiry is concerned to locate those habits of thought that are most likely to lead us from doubt to belief.

The normative weight pulled by the notion of truth or inevitability of success in inquiry must be replaced by a normative standard better suited to the domain of discovery. I submit that fecundity is the feature of discovery processes of most normative import—if the descriptive epistemological task discloses methods of inquiry whose fruitfulness distinguishes them from their peers, we are then justified in adopting such methods in the future. Of course the notion of fruitfulness is itself in need of definition. Here there are two important senses that parallel the two senses of infallibility: (i) a strategy is fruitful insofar as it leads to theories and models that gain acceptance, and (ii) a strategy is fruitful insofar as it structures inquiry to produce a steady increase of empirical content.

We shall also need to reject (b), the uniqueness condition, if we wish to arrive at an adequate notion of a logic of discovery. A proper logic of discovery must be comprehensive enough to account for the fact that there are “many paths to salvation” in scientific inquiry; it must also, however, be specific enough to give a clear illustration of what those several paths may look like in a particular case.

An acceptable logic of discovery should proceed in a stepwise fashion wherein each step is guided by a set of rules or principles, and the whole process is governed by a number of higher-level rules. This is of course merely nothing more than the retention of requirement (f) from above. However, it must be made manifest that by “rules” and “principles” here I do not mean general statements or directions that do not admit of exceptions. Rather, higher-level directives and purposes will largely dictate the level of commitment granted to lower-level rules. To illustrate, I could offer up the following as a rule for chess playing: whenever possible, the sacrifice of a bishop for an
opponent's knight should be performed. It is perfectly sensible to speak of this as a strategic principle or a rule, even though it is understood that higher-level purposes may trump it on many occasions and that it can even be neglected sometimes through mere caprice. It may be objected that what is meant here is more of a heuristic or rule of thumb, i.e., a useful practice of broad application that is not used invariably and whose conditions of application are not well defined. That a bishop can only move in diagonal paths, or that one cannot move into check mate are rules of chess; but that one generally would trade a bishop for a knight is merely a rule of thumb. To this objection I only answer that there can be a high level of inferential interconnection between a state of affairs and its relevant rules of thumb and that mastery of these not strictly logical interconnections is what constitutes expertise in a field. However, since a rule of thumb is a type of rule, and both ordinary usage and historical considerations sanction the use of “rule” to denote a guide to behavior, and not only a strictly inviolable logical principle, we could also, in good faith, retain the notion that discovery is a rule-governed process.

Finally, rather than merely taking propositions as arguments and confining ourselves to the highly specialized rules of deductive logic as the means of getting through the steps of discovery, an adequate logic of discovery should incorporate both richer structures of representation and more complex processing routines to operate on those structures. There are two related motivations for such a move toward greater complexity. First of all, much of the creative thinking of historical scientists relies on forms of representation and reasoning whose complexity far exceeds the expressive capacity of deductive logic. There have been many attempts to render scientific theories as sets of propositions. While some may still argue that such propositional representations are adequate rational reconstructions of finished theories, their highly restrictive notion of what qualifies as a legitimate inference has rendered them totally inappropriate as a means of rationally reconstructing the processes of theory construction; propositional logic is too representationally and inferentially weak to provide a rational reconstruction of the periods of generation and pursuit. Second, the researches of cognitive science into learning, categorization, and problem solving—both for everyday and specialized contexts—have had to utilize richer, and muddier, structures of representation than propositions. In conclusion, to be true to the history of science and consistent with the successful program of cognitive science, a modern-day logic of discovery should take complex structures as arguments. Rather than dealing only with propositions, the logic should be motivated by problems, sensitive to constraints, and capable of both generating and reasoning from complex structures like models and analogies.

I have argued that a pattern of inquiry with the following features qualifies as a logic of discovery:

(a') The method is explicitly fallible.

(b') It is capable of producing multiple solutions to a given problem.

(f') The method consists in a series of inferentially related steps, each of which is populated by a different representation.

(g') The method utilizes the complex forms of representation and inference found in actual scientific thinking and in the researches of cognitive science.

(j) The criterion of fruitfulness is what ranks the various paths of inquiry; as such, fruitfulness provides the most salient normative component to the account.

This formulation, of course, leaves open the possibility that there may be many distinct logics of discovery, depending on how precisely criteria (f') and (g') are filled. I will show in chapter 7 that the models of analogy developed by Gentner, and Holyoak and Thagard, meet the above conditions, and therefore qualify as logics of discovery.

In a deductive proof, each justified step is fully justified and as such could itself be considered the conclusion; put alternatively, each justified step is a conclusion. With analogical reasoning, each step is far from justified, though there are normative considerations involved. Similarly, while the whole train of an analogical reasoning process cannot be justified in the strict sense—it can be appraised in terms of its fruitfulness, its ability to solve the problems that motivated it, and its performance relative to alternative approaches.
CONCLUSION

Hanson’s critics were partially on the mark, and partially off. They were wrong in claiming that there is no logic of discovery, since what they took it to be was very different from Hanson’s definition. However, they were right in saying that Hanson’s conception was not really worth a whole lot. If there is a logic of discovery, we should expect to be able to see what such a logic looks like and how it is applied. More interestingly, such a logic should make sense of certain historical episodes as well as offer up normative directives to science. In chapter 7, the revised desiderata on a logic of discovery are fully met and a new logic of discovery is presented, in the spirit of Hanson and Peirce’s conceptions, which is capable of making sense of hypothesis generation and elaboration within the history of science. Historical case studies involving the theorizing of Ampère and Kepler demonstrate the power of this logic of discovery. Finally, I will mention a related point that draws together material from the last three chapters. Initially, Hanson’s discussions of theory-laden observation and the logic of discovery were very closely related; however, as he came less and less to view the logic of discovery to be concerned with hypothesis generation, and more with the appraisal of the plausibility of hypotheses, these two elements of his thought became somewhat estranged. I shall argue in chapter 7 that this was a mistake on Hanson’s part, and that his original insight can be strengthened into an informative theory of theory generation by utilizing advances in philosophy, cognitive science, and AI.

NOTES

3. He listed enumerative induction as well, but he did not have much to say about its role in discovery.
4. Hanson’s “An Anatomy of Discovery,” Journal of Philosophy 64, no. 11 (1967): 321–52, was mailed to Journal of Philosophy on April 17, 1967, the day before his fatal crash. Additionally, Hanson had long planned to write a book on the logic of discovery. In a list of his publications, apparently from 1963, Hanson includes a contracted book project with Cambridge University Press, entitled “Towards a Logic of Discovery.” He estimated at that time that the book would appear in 1965. Cambridge University Press does not keep records of unfilled contracts going back that far, so it cannot be said how much Hanson advanced on the project. See “Norwood Russell Hanson: Publications,” Department of History and Logic of Science departmental files, IU Archive.
5. That is, the conception first hinted at in Patterns of Discovery (Cambridge: Cambridge University Press, 1958) (especially in chapter 4) and explicitly developed in “The Logic of Discovery,” Journal of Philosophy 55, no. 25 (1958): 1073–89. Both of these works appeared in 1958.
8. Ibid., p. 1077.
10. Throughout the article, the expression “Huyghen’s Law” is erroneously used.
12. Quoted by Hanson, ibid., p. 1088. Newton’s original draft, from which Hanson quotes, is part of the Lord Portsmouth collection in the Cambridge University Library. See Hanson for the exact citation.
15. Donald Schon, “Comment on Mr. Hanson’s ‘The Logic of Discovery,’” Journal of Philosophy 56, no. 11 (1959): 500. The page numbers refer to Hanson’s article.
18. Ibid., p. 503.
20. Ibid.
21. Ibid.
22. See Hanson, Patterns of Discovery, chapters 2 and 4 in particular.
As Reichenbach noted: "it seems to be a psychological law that discoveries need a kind of mythology" (Reichenbach, Prediction and Experience, p. 402). This theme is explored at length in the first chapter of David Lamb, Discovery, Creativity and Problem-Solving (Aldershot, UK: Avebury, 1991).

36. Reichenbach, Experience and Prediction, p. 5. To highlight the general misreading of Reichenbach, the actual historical procedures of theory testing would be inhabitants of the context of discovery, and therefore, according to the received view, lie outside the purview of logical analysis. Thus, if we ignore Reichenbach's correspondence principle, we end up in the undesirable predicament of having all of historically situated science outside the pale of philosophical scrutiny.

37. The ideas described in this paragraph were put forward most fully in Laudan's Progress and Its Problems (Berkeley: University of California Press, 1977).

38. Laudan, "Why Was the Logic of Discovery Abandoned?" p. 174.

39. William Whewell: "Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin; some fortunate cast of intellect rising above all limits. No maxims can be given which inevitably lead to discovery." Whewell, Philosophy of the Inductive Sciences, Founded Upon Their History, vol. 2 (New York: Johnson Reprint Corp., 1966), pp. 20–21; quoted in Laudan, "Why Was the Logic of Discovery Abandoned?" p. 181.


41. Hempel's account of Kekulé's discovery of the structure of benzene is a well-known instance of the expectation of totality. Hempel, Philosophy of Natural Science, pp. 15–16.

42. Often philosophers speak of a "recipe book" for discovery as encompassing all of these properties. For instance, Hansed claimed that Aristotle and Peirce "did not think themselves to be writing manuals to help scientists make discoveries. There could be no such manual." He also approvingly quoted Mill: "There is no science which will enable a man to bethink himself of that which will suit his purpose." Hanson, "Is There a Logic of Discovery?" p. 21.

44. Nickles counters the philosophers who claim that instances of irrational discovery prove that there is no method of discovery with the following: “If you are struggling with a problem, these philosophers should (on their view) tell you, citing the cases of Kekulé, etc., that as good a way as any to solve it is to doze off before a fire, board one tram after another, start pecking randomly at the typewriter, sit under an apple tree...” Nickles, “Scientific Discovery and the Future of Philosophy of Science,” p. 29.


46. It is not the understanding of the strictly logical rules of chess, which any child can comprehend, that qualifies one as a master, but rather it is the possession of an encompassing and ready acquaintance with the rules of thumb of chess together with an ability to tease out their entailments. Thus, I am perfectly content to adopt the term rule of thumb, though doing so results in ugly expressions like “scientific discovery is a rule of thumb-governed process.”

Chapter 5

PHILOSOPHY AND HISTORY OF SCIENCE

INTRODUCTION

Philosophy and history of science each have a default position that guards against illicit incursions by the other. Philosophers discuss the Genetic Fallacy, or at least they used to, and argue that the revision of philosophical positions in light of historical evidence is to be avoided. If philosophers no longer steer clear of history out of dread for the Genetic Fallacy, they are still content to confine their attention to conceptual analysis. Historians, on the other hand, abstain from offering normative judgments for fear of engaging in Whiggish interpretation. Each of these positions is widely regarded as being committed to a hopeless epistemology, though there is no strong consensus as to why. In what follows, I will present Hanson’s various attempts at finding an epistemologically sound characterization of the relationship between history and philosophy of science. While he never was able to articulate a completely satisfactory or consistent account, a position that synthesizes his best intuitions on the topic is presented.

Hanson devoted a great deal of effort to the creation of a proper model to characterize the field of interpenetration between history and philosophy of science. Although he seemed to share the philosopher’s occupational scruples concerning the Genetic Fallacy, in many areas he challenged and tested received disciplinary constraints, often with valuable results. Here, as in many other areas of his thought, Hanson thought of himself as navigating a via media between two undesirable alternatives; he characterized the alternative positions as follows: