N.R. Hanson on the Relation Between Philosophy and History of Science[α]

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Despite having put the concept of HPS on the institutional map, N.R. Hanson's distinctive account of the interdependence between history of science and philosophy of science has been mostly forgotten, and misinterpreted where it is remembered. It is argued that Hanson's account is worthy of renewed attention and extension since, through its special emphasis on a variety of different normative criteria, it provides the framework for a fruitful and transformative interaction between the two disciplines. This essay also examines two separate threads of Hanson's account of philosophy of science: his analysis of the conceptual dynamics of science and of the interrelation of the history and philosophy of science. While the two strands appear incongruent, and were perhaps inconsistent, a new interpretation of them is offered which is both consistent with Hanson's fundamental intuitions and defensible in its own right. It is demonstrated that Hanson's account compares favorably with those of Kuhn and Lakatos, and that it may provide a constructive means of scaling the barriers erected by fears of the genetic fallacy and 'whiggish' history.

I. Introduction

The relationship between philosophy of science and history of science has been vexed ever since it became commonplace to regard them as complementary disciplines. Ronald Giere voiced what has been the standard metaphor depicting their relationship as a "marriage of convenience" at

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[α] A note on the text: this article is a draft of Chapter 5 of *N.R. Hanson: Observation, Discovery, and Scientific Change* (Amherst, NY: Humanity Books, 2010). Scholars are encouraged to cite that source. (Noted added in January 2013, MDL).

best – each discipline has to do with science, and to that extent, they are bound together[1]. The presence and creation of interdisciplinary programs demonstrates the existence of a need, on the part of both philosophers and historians, for one another's insights, but there is no consensus on what the nature of their relationship should be, and most well-known formulations of the nature of their intercourse, like those of Giere (1973) and Kuhn (1977b), are fairly pessimistic.

Through a sympathetic interpretation of N.R. Hanson's approach to history and philosophy of science, this paper argues for a more vigorous type of interaction between the two fields. More specifically, the current vogue of the "deflationary" position, which holds that the history and philosophy of science are largely incommensurable, is shown to be a result of a confused conception of the normative and the descriptive as autonomous and independent forms of analysis. A satisfactory account of the interrelation of the history and philosophy of science is obtained by uniting two separate, and somewhat inconsistent, elements of Hanson's philosophy of science: his analysis of the conceptual dynamics of science and his explicit formulation of the relation between history and philosophy of science. It is argued that the essential elements of his view remain legitimate and that extension of his approach is more likely to overcome the contemporary rift between philosophers and historians of science than the historiographic approaches of Lakatos and Kuhn.

II. Hanson's Role in History and Philosophy of Science

Perhaps no one can claim to have done as much for the union of history and philosophy of science as Norwood Russell Hanson. In 1960, Hanson became the founding chair of the University of Indiana's Graduate Program in the History and Philosophy of Science, the first of

[1] It may no longer be proper to speak of a marriage, since another suitor has entered the fray: STS. Perhaps we might say we now have a love triangle, and one which, after the manner of love triangles, is divisive and far from convenient.

its kind, and soon after to become a department. However, from the very beginning, the program was a subject of dispute (to the extent that the Philosophy Department would not allow the word "philosophy" to appear in the fledgling program's title (cf. Grau 1999, S302 and Veatch 1997, p. 114 for additional information on naming of department) and Hanson's own connection with it was not to last very long. In addition to his historical and philosophical work, Hanson also devoted considerable effort to determining how the relation between history and philosophy of science is to be understood. However, for having been one of the brightest luminaries of the early days of HPS, interest in Hanson's philosophical and historical work over the past forty years has been confined to a few select areas like his analysis of observation, his arguments for a logic of discovery, and his defense of the Copenhagen Interpretation.

Hanson shared the positivist view that the function of philosophy of science is to examine and clarify the conceptual foundations of science, though he differed from the positivists in thinking that science – both historical and contemporary – provides guidance for philosophy. In a sense, Hanson can be seen not so much as criticizing logical positivism as extending the field of conceptual analysis to areas the positivists had considered off-limits, like the context of discovery and the conceptualization of perception. Most importantly for this discussion, Hanson also believed that philosophical accounts of science should not be concerned with the static frameworks of the "catalog sciences" or with the type of science done within such frameworks, but instead with the most dynamic and formative stages of scientific development.

While Hanson is now remembered as a philosopher – and he identified himself primarily as one – his contributions to history of science, both in terms of his own research and his role as an institution builder, are far from trivial. Hanson's two long works, *Patterns of Discovery* and *The Concept of the Positron*, are both heavily concerned with historical issues; *The Concept of* *the Positron*, in particular, incorporating as it did material based on interviews and correspondence with Dirac, Anderson, Blackett, and other principals involved, contains some very significant history of science.

On the institutional side, as has been mentioned, Hanson founded America's first HPS Department at the University of Indiana. Originally a member of Indiana's Philosophy Department, Hanson approached the newly hired historian Edward Grant "[w]ith mischief in his eyes" (Grau, p. S300) with a plan to create a *program* in History and Philosophy of Science. Due to his mistrust of the Philosophy Department, Hanson, along with Grant, eventually succeeded in proposing a new and independent *department* in HPS. The new department received instant acclaim, with no less an authority than Alexandre Koyré having said, "Indiana University has now become the center for studies in the history and logic of science in the world" (quoted on S303, Grau). Marie Boas Hall, one of the members of the department in the early 1960's, represents Hanson has having "for some reason longed to move to Yale (p. S82)" after recovering from injuries sustained in a plane crash. Upon Hanson's departure from Indiana, Hall found the environment there less appealing, and the original members of the department scattered; the department also lost its distinctive orientation as it lost its original members. Had Hanson remained at Indiana longer – indeed had he lived longer – perhaps his historical significance to HPS would be more highly appreciated.

The orientation of the Indiana HPS Department was expressed characteristically by Hanson in a grant application to the Office of Aerospace Research in 1962: "Each of us has been concerned with the structure of scientific argument, whether those of late-medieval mathematics or astronomers, or of the giants of the Scientific Revolution, or of the fertile 18th and 19th centuries, or of early-20th-century physics, or contemporary microphysics, mathematics, and the social sciences. It is the ideas, concepts, propositions – the structure of arguments which is our concern" (quoted in Grau, p. S306). This statement, which presumably was acknowledged to be accurate by the Department's historians (Edward Grant, A. Rupert Hall, and Marie Boas Hall), marks out the principal region of shared concern for philosophers and historians of science: historically situated scientific arguments.

Hanson advocated *institutional* integration of HPS: history of science and philosophy of science should retain their disciplinary identities, but need to stand in a close, collaborative relation to one another if they wish to stem their respective vices of blindness and emptiness. While many philosophers of science and historians of science today would assent to this view, there is little consensus concerning the reasons why such an interrelation is necessary; I shall, thus, present Hanson's reasoning behind the view and analyze the extent to which Hanson's account could be extended into contemporary contexts.

III. Hanson on Conceptual Dynamics

Hanson's philosophical orientation was focused on making sense of both the logical and interpretive aspects of science. Hanson's forays into experimental psychology, Oxford analysis, and the study of Peirce, were all motivated by a desire to make sense of the rationality of science, in all of its forms, and it was this emphasis on the normative aspects of science that kept his philosophical theory together.

a. Patterns of Discovery

Hanson's optimism regarding commerce between history of science and philosophy of science is a central theme of his first and still most influential long work, *Patterns of Discovery*.

Utilizing insights from ordinary language philosophy, history of science, and psychology, Hanson made the case that, as a matter of fact, scientific thinking and observation are always laden with conceptual and theoretical elements. More importantly – and this is the aspect of Hanson's argument that many philosophers pass over – science would not be the rich and versatile instrument it is were it not so loaded with theory and expectation. Thus, Hanson argued that science's deep conceptual interconnectedness is what accounts for its epistemic power, for its normative character. Hanson marshals a collection of descriptive claims in *Patterns of Discovery* in the interest of clarifying the normative aspects of science.

Hanson's goal was to elucidate the 'open' structure of scientific frameworks, as opposed to the rigid, definitional networks of geometry, formal logic, and pure mathematics. Formalist philosophy of science, epitomized by the logical positivists, sought to impose the clean definitional structure of formal systems onto the subject matter of science. Such a procrustean enterprise not only fails to aid us in our understanding of science, but it generates useless linguistic paradoxes that divert our attention from actual science.

While the received thrust of *Patterns of Discovery* was an attack on logical positivist accounts of observation, Hanson's explicit intent was to illuminate the processes through which new conceptual frameworks are constructed and thereby to render the analysis of scientific discovery rationally appraisable:

> In *Patterns of Discovery* … 'explaining x' is represented as 'setting x into a conceptual framework'. *Discovery* is thus characterized as 'the dawning of an aspect of x' such that x is at last seen as part of a more comprehensive and comprehensible pattern; earlier, x might have been anomalous in seeming not to fit any intelligible organization of ideas. (Hanson, Notes Toward a Logic of Discovery, p. 48)

For Hanson's project to succeed, it was necessary to recast the central epistemological concepts of science in a manner that is true to their use in science, instead of relying on unenlightening formulaic constructions. Hanson's description of the proper relation between psychological and epistemological analyses, a relation that parallels the one he forges between history and philosophy of science, is illustrative of his general approach to conceptual foundations:

> [T]he factual details of discovery constitute a subject matter for psychology – wherein words like 'intuition', 'insight', 'hunch', 'in a flash', etc., are descriptively associated with the phenomenon to be investigated. But *that* such spectacular reorganizations of concepts do occur is a matter of profound epistemological importance. [*Patterns of Discovery*] traced some philosophical implications of such sudden *coagula* in the data of scientific perception. (Hanson, ibid, p. 48)

Hanson's discussion of observation was intended to provide a methodological gestalt for the proper understanding of apparently opposed concepts in scientific epistemology. It is carefully presented in what follows since it makes clear, by extension, Hanson's model of the interrelation of forms of analysis generally.

Hanson's critique of the logical positivists' philosophy of language, as well as to their commitment to phenomenalism, illustrates his general approach to categorical divisions. The positivists conceived of observation as involving the clamping of an intellectual interpretation onto a purely given sensation. Hanson asks whether such a picture of perception is capable of conflicting with conceivable states of affairs and concludes that it cannot be. Therefore, the positivist account of observation's central components is unfalsifiable and fails to qualify as empirical. It could, of course, be argued that the positivist account is still acceptable if its linguistic constructions were simply representations of our most natural habits of speaking about observation. Hanson points out, however, that the sense-datum theorists, and their forebears, did violence to ordinary speech in their attempt utterly to sunder terms like 'interpretation' and ‘perception’. The positivist is forced, by an ineradicable fondness for an *a priori* theory of observation, to consider aspect shifts as cases of ‘instantaneous interpretation’. Hanson has two main objections to this way of speaking.

The first objection is that in ordinary discourse we understand interpretation to be something we can either engage in or refrain from performing – to call all acts of perception acts of interpretation is to render the term ‘interpretation’ relatively meaningless, since it applies everywhere. Furthermore, if it makes sense to speak of something as being interpreted, we ought to have some conception of what that something is prior to being interpreted. For instance, it is perfectly clear that an interpretation of *Moby Dick* is something distinct from the book itself, and something the book could easily get along fine without. Thus, to speak meaningfully about interpretations in perception, we should at least be able to conceive of a completely uninterpreted perception; but this we cannot do.

Hanson’s second objection concerns some natural language assumptions concerning the use of the word ‘interpretation’. First of all, interpretation is an activity, and it takes time to occur – it makes sense to say that one is halfway through interpreting something, as when one translates a manuscript from a foreign language. If all the interpreting we speak of in ordinary language is understood to involve the passage of time, we do violence to our language, and understanding, when we speak of ‘instantaneous interpretations’. Additionally, when one interprets something in an ordinary context, one is aware that one is engaging in interpretation; cases of scientific observation typically are not characterized by such explicit awareness, so it would be misleading to speak of them as being products of interpretation.

Hanson objects to the view that observation and interpretation are entirely separate, non-interdependent items – like “peaches and cream” (Hanson 1964, p. 1) – since such a view blocks our ability to investigate the nature of interpretation empirically. Hanson believed that once we divest ourselves of misleading presuppositions, it is clear that observation and interpretation are intimately bound together:

> [S]cientific observation and scientific interpretation need neither be joined, nor separated. They are never apart – so they need not be joined. They cannot, in principle, be separated – and it is conceptually idle to make the attempt. Observation and interpretation are related symbiotically such that each conceptually sustains the other, while separation kills both. This will not be news to any practicing scientist – but it may seem heretical indeed to philosophers of science for whom *Analysis* has become indistinguishable from *Division.* (Hanson, 1964, p. 9)

This statement brings us to an interesting general question: if analysis of a concept like observation does not lead to the walling off all of the concept's subordinate elements into strictly separate compartments, might we not expect that, when properly analyzed, logic and psychology, the descriptive and the normative, and philosophy of science and history of science are not, after all, distinct, mutually exclusive pairs? Hanson, of course, gives an affirmative reply, and expressed, for instance, the following claim concerning the relation of history and philosophy of science: "Let no man completely sunder disciplines that are intimately connected through their common concern with ideas, concepts, reasoning, and the argumentation of scientists." (Hanson, 1962, p. 581) According to Hanson, such conceptual pairs are to be understood through careful analysis of their use. How precisely such conceptual pairs are intermingled, interrelated, and complement one another can only be appraised through a study of their functioning within particular cases, though he argued that general pattern of interrelation is very like that of observation and interpretation: neither is ever found purely on its own – they stand, if at all, together. In the next section, Hanson's account of Galileo's discovery of the law for free falling bodies is presented to demonstrate the way in which the normative and descriptive are intermingled in cases of discovery.

b. Galileo's Discovery of Free Fall

Hanson was concerned to make normative sense of the processes through which scientists on the frontier, like Galileo, were able to produce their conceptual innovations. Hanson uses the case of Galileo's discovery of the law for free-falling bodies[2] to show that while facts inexpressible in a given notation are not impossible to grasp, the practical obstacle such a process involves is very conceptually important for understanding the growth of science:

> [T]hinking new thoughts in a conceptual framework not designed to express them requires unprecedented physical insights. In the history of physics few could sense the importance of things not yet expressible in current idioms. The task of the few has been to find means of saying what is for others unsayable. (Hanson 1958, p. 46)

The point Hanson wishes to make in his discussion of Galileo is of a Wittgensteinian flavor: it is possible to form "a concept 'x' in a language in which x is not easily expressed." (Hanson 1958, p. 185) Given the extant conceptual, mathematical, and practical experimental context, it was most difficult to frame the law of free fall correctly. Galileo was able to give the correct proportionality for spaces traversed in free fall, but then claimed that the velocity of a falling body is proportional to *distance* fallen. Thus, he passed from the correct form of the law to a very different, incorrect form, but which nonetheless struck him as equivalent since the framework he initially worked in provided no good means of measuring time durations, especially on the tight scale required for falling.

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[2] Hanson's account of Galileo's discovery of the law of free-fall relies crucially on Alexandre Koyré's discussion, as does much of Kuhn's thinking. Both Hanson and Kuhn are of course at odds with Stillman Drake's account, based on a more recent analysis of Galileo's fragments and notes. Drake indicates, contrary to Hanson, that Galileo was conscious of the correctness of the square-times law in 1604 (Drake 1989, p. 49). As I.B. Cohen remarks (Cohen, Birth of a New Physics, p. 204 – check this), it would appear odd that Galileo would have established such a result through experiment so early, and yet would not have presented it.

Though all the requisite data, or ‘facts’, were known from the beginning to Galileo, Beeckman, and Descartes, the correct law of free-fall was only grasped after a long period of confusion. All three persisted in thinking of velocity as a direct proportion to the space traversed, rather than, as is correct, a direct proportion of the times. Hanson attributes this apparent obtuseness among geniuses to the geometric notation with which such problems were then treated, which left no room for the expression of a time axis:

> The thinking of scientists in this period ran along geometrical rails; it was constituted of ideas of spatial relations. A ‘time co-ordinate’ would have had little significance for these natural philosophers, as little as would a ‘fragrance’ or a ‘beauty’ co-ordinate. (Hanson 1958, pp. 39-40)

While it was already known, in a sense, that velocity is directly proportional to time, the notation of the period induced scientists to organize the phenomena according to spatial reasoning. Also, since pile-drivers were the paradigm case of free-fall, distance, both in terms of the initial height of the weight and the depth to which the weight sunk (which gave a measurement of velocity), was the most obvious parameter of consideration. Spatial properties were more easily measured and represented than temporal ones, especially since the times involved were so short, and it took the penetrating mind of Galileo to see past this theory-laden factual representation to the correct solution. Here we see something like a paradigm at work, particularly insofar as the scientists were ‘blinded’ to certain aspects of the phenomena, but Hanson’s emphasis is on how the successful conceptual framework was rationally constructed – Hanson’s disbelief in flashes of inspiration separates him from Kuhn and it follows directly from his commitment to a normative framework.

In order for Galileo to find a proper mathematical characterization of free fall, he needed to fashion a conception of velocity as change of position with respect to time in the place of the confused Aristotelian and impetus notions of speed, which both tended to confuse average and instantaneous velocities. (See Kuhn 1977a). How did he acquire such a conception? First, Galileo needed to devise various means of measuring time accurately on the scale appropriate for free fall. Hence his water clocks and his fretted inclined plane, both of which were only fully illuminated in the 1970s by Stillman Drake[3]. Galileo also needed to 'dilute' the speed of falling objects to measurable levels by using the inclined plane. However, before any of thse improved measurement techniques could be of real use, Galileo needed to craft the conceptual framework within which free fall would be expressible as a systematic mathematical relation, and this required the creation of a time axis, which was necessary for framing concepts like 'moment' and 'acceleration'. Galileo's act of creation was not instantaneous, but was extended over the course of thirty four years, during which time he slowly shifted from viewing the temporal aspect of falling as a negligible and redundant detail to an essential component of the concept of velocity.

For Hanson, the free fall case demonstrates that a good deal of conceptual labor is often necessary in order to make certain 'facts' expressible. More broadly, such a case has the capacity to illustrate how new conceptual structures are built. In particular, we need not assume that new conceptual structures are conceived instantaneously, or that their creation is not rationally driven. Galileo was able to create his mature framework by developing the appropriate conceptual and experimental base.[4]

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[3] While Hanson did not emphasize Galileo's experimental innovations as strongly as contemporary historians and philosophers of science do, the insights gained by the close study of experimentation's role in the creation of meaning, particularly in the work of Ian Hacking, David Gooding, and Nancy Nersessian, would only add force to, rather than weaken, Hanson's account.

[4] Galileo was not especially keen on laying out the sources of his inspiration, and his works reflect only those things about which he was confident. In this respect, he was quite different than his contemporary, Kepler, who seemed to revel in the contemplation of his own thought processes as much as in his theories. We can only piece together an image of his mental transformations by analyzing his books, letters, notes, and the writings of his contemporaries.

More generally for Hanson, and here I am applying his general account to the free fall case in a way that he explicitly does not, it was necessary for Galileo to see unconstrained free fall, pendulum motion, and falling on inclined planes as phenomena of basically the same type. Traditionally, these types of motion were treated as belonging to different fundamental categories. For instance, on Aristotle's account, pendulum motion is violent, whereas the other two are natural. It was necessary for Galileo to see these phenomena as of the same type, and his conception of nature's laws as expressions of simple mathematical relations facilitated this unification. It also appears reasonable that his focus on mathematical relations (or, his search for geometric ratios between measurables) allowed him to distinguish between otherwise similar-looking formulations that were in fact incompatible. Furthermore, his quest for mathematical relations allowed him to detect the implicit contradictions in the "natural" conception of free fall (i.e. the view that the speed of a free falling body is proportional to the distance fallen). In short, Galileo's new strategies of *seeing as* and *seeing that* enabled him to see past traditional conceptual frameworks toward new sets of expectations.

Kuhn's attitude concerning Galileo's discovery of the law for pendulum oscillations is characteristic of his treatment of extraordinary research, and provides a telling contrast to Hanson's presentation of Galileo's discovery of free fall. Kuhn attributes Galileo's capacity to see the pendulum as a special item to be characterized in terms of period, length, etc. as resulting from his having internalized the paradigm of the impetus theory: "what seems to have been involved was the exploitation by genius of perceptual possibilities made available by a medieval paradigm shift." (Kuhn 1962, p. 119) While it is no doubt true that the impetus theory provided a necessary conceptual bridge from Aristotelian to classical mechanics, its presence alone is not sufficient to explain Galileo's production of a novel conceptual framework. For that, it was not only necessary for Galileo to learn to see differently from Aristotle but from the impetus theorists as well. How exactly did he accomplish this? Was his new paradigm the result of an extended process or was it a spontaneous creation, an imaginative posit "invented in one piece for application to nature"? (Kuhn, 1970c, p. 12) Hanson's discussion of free fall makes a plausible case that Galileo's process of creation was rational and not paradigm-directed; if anything, Hanson's presentation is a study in the gradual fabrication of a paradigm.

While Hanson was unable to fill in all the gaps in the conceptual history of Galileo's development of the law for free fall, his framework seems capable of rationally patterning the historical facts, such as we have them; it also allows us to infer how the history was likely to have unfolded where respectable historical evidence is lacking.

IV. The "Irrelevance" of History of Science to Philosophy of Science

Philosophers of science are familiar with the following maxim: 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind'. The maxim is generally attributed to Imre Lakatos (Lakatos 1978, p. 102), and sometimes to Herbert Feigl (Feigl 1970, p. 4), and both Lakatos and Feigl give credit to Kant as the inspiration for their nearly identical maxims. However, the maxim was originally expressed by Hanson, who credits John Maynard Keynes as its inspiration, in 1962 in his introduction to Keynes's *Treatise on Probability*:

> *Without logical analysis history of science is blind. Without attention to the arguments of past scientists, philosophy of science is empty.* And without Keynes, the rational connection between enquiries within these two disciplines might be extremely difficult to perceive. (Hanson 1962a, pp. x-xi)

Not only has Hanson's clever slogan been purloined, but his distinctive account of the relation of philosophy and history of science has been mostly ignored. Hanson's attitude toward history of science has been taken – not without some warrant considering his flamboyant prose – to be somewhat deprecatory[5]. However, Hanson was not, despite the impression one might gather from his slogans, anti-historical. Instead, he believed that historians of science ought to address the issue of justification within historical context in order to produce quality history of science. Histories that abstract entirely away from all normative considerations are rightly viewed as being incomplete, since history, like many other disciplines, cannot just shirk its obligation to utilize its data and methodology to address issues that interpenetrate it. Far from arguing for a diminishment of history's role, Hanson favors its enlargement: historians should consciously attend to questions of justification in context (a context they are in a privileged position to comprehend), in addition to performing all their other necessary analysis. At the same time, historians need to listen to, and interact with, philosophers concerning models of justification. If anything, Hanson's proposal is a clarification of the respective occupational duties of history of science and philosophy of science, as well as a plea for their increased interaction.

In his principal article concerning the relation of philosophy and history of science (Hanson 1962b)[6], Hanson argued that the history of science is logically irrelevant to the philosophy of science – a surprising position for the founder of the first HPS program in the United States to hold. However, what Hanson was worried about in this discussion was the genetic fallacy. He thought that if the truth of philosophical claims were dependent on historical factors, all demonstrations of philosophical claims would commit the genetic fallacy. To the question, "can a philosopher utilize historical facts without collapsing into the "genetic fallacy"?" (Hanson 1962b, p. 574) Hanson provides an affirmative answer (perhaps somewhat surprisingly in light of the article's provocative and misleading title). However, the historical

[5] Jutta Schickore, for instance, characterizes Hanson as a "fervent anti-historian" due to his pronouncements in his 1962b. (Schickore 2006, p.59)
[6] The Kantian maxim discussed above provides the organizing motif of the article.

facts cannot be used as a justification for some philosophical thesis; instead, the historical facts can be used as particular premises, as instances of statement forms, within a valid formal argument. The particular premises, if true, render a particular substitution instance of the general argument sound.

Hanson distinguished between three modes of interpenetration between history of science and philosophy of science. The first mode concerns the overarching philosophical framework, or *Weltphilosophie*, that informs the historian's selection and interpretation of data[7]. These 'cosmic commitments' (Hanson 1962b, p. 574) can be misleading if they are uncritical or unacknowledged, though enlightened philosophical criticism has the capacity to dismantle and analyze these large-scale elements of mental architecture.

Perhaps as a result of philosophical interference, historians of science from the late nineteenth century onward became concerned with the second type of interrelation of history and philosophy of science: the development of the 'conceptual bricks and beams' that compose larger-scale intellectual edifices. Historians of science of this cast of mind (Ernst Mach, Pierre Duhem, Alexandre Koyré, and Edward Rosen) attempted to chart the development of philosophical conceptions like law, demonstration, observation, verification, etc., through analysis of historical cases. Much of Hanson's own work, including his discussions of observation and Galileo presented previously, falls into this category.

The first two modes of interpenetration, the grand philosophical architecture and the conceptual bricks and beams, though conspicuous in Hanson's day, did not typically lead to the mutual enlivening of the two fields. Hanson counsels historians and philosophers instead to concentrate on the third mode of interpenetration: the arguments of science, the "engineering

[7] This uncontroversial scheme of data selection is similar to what Koertge calls a 'conservative reconstruction'. When producing a conservative reconstruction "the historian relies on minimal attributions of rationality to the actors in the past – he assumes their actions were appropriate to their problem situations (p. 360).

connections" that support the edifice of science. "[i]t is in the detailed analysis of the detailed arguments of scientists *and* historians where philosophy can most help, and *be helped."* (Hanson 1962b, p. 576)

Expanding upon suggestions of Keynes, Hanson argues that philosophy of science and history of science share a concern with the arguments used by historical scientists. It is relevant to both fields whether a given theory or statement was well supported by the evidence, as it was understood, at the time in question. According to Hanson, Keynes's argument "that no scientific statement is ever *probable* in itself, but probable *only* on the assumption of given evidence," (Hanson 1962b, p. 576) opens the door to an objective analysis of the relative reasonableness of competing historical theories. Hanson conceives the appraisal of the connection between a theory and its evidence to be deductive – whichever theory at a given time that has the highest probability relating its assumptions and initial conditions to its consequences is the best theory for that time. Hanson explains the manner in which adoption of this approach would transform the activity of philosophers of science:

> Assuming an advanced familiarity with a scientific subject matter, then, the logician of science should be capable of assessing the formal cogency of arguments of, e.g., "steady-state" cosmologists as against "big-bang" theorists: he should be able (in principle) to determine which claims of reasoning are the "best made," which conclusions are most likely *on the evidence given,* which assumptions *en route* are most and least vulnerable. (Hanson 1962b, p. 577)

Note that this analysis yields not *the best possible theory,* as a pure formalist would desire, but the best theory historically available at *t*[8] – both history and philosophy are essential to this type of analysis. The 'formal cogency' of scientific argument thus provides us with a means of assessing the quality of conclusions as well as assumptions. Hanson's commitment to this mode

[8] We can see that Kuhn's critical remark regarding the aims of philosophers does not apply to Hanson's account: the philosopher's "goal is to discovery and state what is true at all times and places rather than to impart understanding of what occurred at a particular time and place." (Kuhn 1977b, p. 5)

of proceeding is yet another sign of his adherence to the thesis of what has since come to be called explanatory unification. It also demonstrates Hanson's belief that the evidence in favor of a theory can be objectively appraised – this thesis is obviously in conflict with Kuhnian incommensurability, since it assumes that what qualifies as evidence is not importantly affected by one's theoretical commitments. Though Hanson is invariably careful when discussing the degree to which 'theories square with the facts' (Cf. Hanson 1965, p. 58, fn 23), he clearly believed that the theoretical loading of factual language presented a merely practical, and surmountable, barrier to theory appraisal[9].

Of course, Hanson recognizes the impracticability of producing numerical probability figures for theories past. Nonetheless, he does believe that informed inquirers are capable of recognizing clear cases of probable and wildly improbable theories, and we can increase our certainly by appealing to ever more inclusive sets of initial conditions and historical detail.

Let us look more closely at what Hanson's view entails for the intersection of history and philosophy of science, and then note what it means for each discipline separately. Hanson depicts their intersection through a colorful metaphor:

> [The analysis of important historical arguments] is the "hot" junction box which connects the conceptual circuitry in history of science with that of philosophy of science. Professionally, the logician and the historian will often be concerned exclusively with the rational wiring within that box – the scientific argument itself – and not just with the intricate intellectual geometry leading to it and away from it, nor with the lights that may go on in the world of science, and the illumination afforded by historians of science, as a consequence of that circuitry and that junction box being designed as they are. The historian of science and the logician are both concerned with the structure of scientific ideas. These concerns fuse into

[9] There are many striking parallels between Hanson's view and Kitcher's (1981) account of explanatory unification. Both Kitcher and Hanson argue that explanatory unification considerations ought to make sense of the history of science. Also, on Kitcher's account, rival theories can only be compared relative to a more or less stable and theoretically neutral body of facts, which each theory strives to unify. Finally, both Hanson and Kitcher conceive of *arguments* as the entities that unify the facts in an intelligible way.

> one when the scientific *argumentation* of the past takes the spotlight. (Hanson 1962b, pp. 579-580).

History of science without philosophy of science is blind because without the normative concerns of epistemology, the assorted data of history are nothing but a chaotic jumble, whose only principle of order is their temporal arrangement. If an historical narrative is to be coherent, it must not only refer to the way in which a scientist's *Weltphilosophie* and understanding of key philosophical concepts of science figured in the course of research, but it most go deeper and assess the strength of the arguments offered in support of the theory. Attention to these philosophical elements of scientific activity will keep history of science from getting lost – the fear of which haunts the blind – by curbing it from losing itself in irrelevant historical information and dulling "the scalpels of philosophy by burying them in the historical gravel." (Hanson 1962b, p. 580)

Philosophy, according to Hanson, is primarily concerned with assessing the adequacy of arguments; hence, his occasional use of 'logic of science' as a synonym for 'philosophy of science." In his 1962b, Hanson seems most keenly interested in defending historically inclined philosophers like himself from the charge that they commit the genetic fallacy. One is guilty of the genetic fallacy when one holds that the validity of an argument depends on its source. Hanson claims that the *validity* of an argument can in no wise depend on historical facts:

> The logical relevance of history of science to philosophy of science is nil. Staring at novel facts has never made old arguments invalid, new arguments valid (or vice versa) (Hanson 1962b, p. 585)

However, an argument's soundness will depend on historical facts, if the premises refer to historical episodes. The arguments studied and advanced by philosophers may be perfectly valid or cogent, yet nonetheless have no connection to history or everyday reality. Thus, Hanson characterizes much of positivist philosophy of science as being guilty of the "fallacy of misplaced abstraction" (Hanson 1962b, p. 582); while such analyses ordinarily possess many formal virtues, they have the defect of not really being about anything – one might say that they qualify only as philosophy – since they have no subject matter – not philosophy *of science*. Hanson illustrates the interrelation between philosophical and historical analysis of science with an appropriately aeronautical conceit:

> For a work in philosophy of science to be shot down by philosophers, it must at least get off the ground. This is done only via a runway of facts concerning the history and present state of the science with which the investigator is concerned. Such facts are not germane to the sophisticated professional appraisal of the intellectual flight and logical maneuvers demonstrated thereafter. But the philosopher of science who does not know intimately the history of the scientific problem with which he is exercised is not even airborne. His analytical skill may be admirable, but it does not take us anywhere. (Hanson 1962b, p. 586)

Hanson clearly believed that great works in history and philosophy of science embodied the type of interpenetration he highlighted, and, presumably, understood his own work on Leverrier, Newton's theory of Fits, and the discovery of the positron as being in the same vein. His frustration with the relation of the two fields must have stemmed from the degree to which they were bedeviled by misleading conceptual assumptions.

The Keynesian formula can, of course, be generalized to cover ranges of time. Thus, it can be employed to adjudicate on the reasonableness of a theory over time, e.g., undulatory or corpuscularian theories of light in the 19th century, or theories of special creation in the early 19th century. This extension of the formula, however, does not go beyond anything Hanson advocated, representing, as it does, integration of the formula over a range of time.

Philosophers have long been keen on proffering methods; history of science has also been populated with a multitude of methodological pronouncements. Perhaps one could legitimately use history to determine which method was most likely to lead to discovery in some field over some range of time. In fact, it would seem as though, if philosophy of science is to possess any prescriptive force whatsoever, it would need to derive such force from a Keynesian analysis of methodological efficacy. Another shortcoming of the Keynesian formula is that it leaves out of account notions like promise, simplicity, consistency, consilience, fecundity, and elegance. Such notions have been instrumental in the historical development of scientific theories, and their role seems to be straightforwardly normative. In the final section of this paper, I will argue that generalizing the Keynesian formula such that it ranges over facts of human cognition and historical patterns of argument will provide a framework in which the history and philosophy of science can be usefully interrelated.

V. Comparison to Rival Accounts of Historiography: Kuhn and Lakatos

Arguably, the two most influential historiographies on philosophers of science have been those of Kuhn and Lakatos. While there are some obscure and unsatisfactory features of Hanson's account of the relationship of philosophy and history of science, it has some advantages over the historiographies of Kuhn and Lakatos. Hanson argued that Kuhn's model of science is guilty of conceptual circularity, and therefore non-empirical; had he lived long enough to see Lakatos's program reach its peak, he surely would have offered up the same criticism.

It is common to think of Kuhn's *Structure of Scientific Revolutions* as deriving a descriptive model of science from a study of its history. Lakatos exemplified this reading in his (1978) and argued that Kuhn's account is defective due to its having shut out all normative analysis. Since Kuhn's historiography is commonly thought to be too heavily inclined toward merely descriptive analysis, and since discussion of that topic is so well known, I will not discuss criticisms of that type, but instead will present Hanson's very own original critique of Kuhn's account of method.

Hanson was one of the earliest readers of *Structure of Scientific Revolutions*, since he refereed it for University of Chicago Press[10]. Hanson argued that Kuhn's model of science was not only something more than description, but that its central concepts were defined in terms of one another (Cf. Hanson 1965 as well). If every revolution involves paradigm replacement, and every paradigm replacement is by necessity a revolution, then Kuhn's view, because of its circularity, is insulated against falsification. Hanson argues that the model of science Kuhn presents lacks the normative characteristics of an empirical theory of science: "As a genuine historical thesis, Kuhn's must be like all others – factually true (for the most part) but vulnerable-in-principle to possible counter-evidence." (Hanson 1965, p. 371) That Hanson criticized Kuhn on these grounds indicates that he held his own accounts of science and history of science as possessing the capacity to conflict with the facts.

Hanson challenged Kuhn to explain two historical cases in which paradigm replacement and revolution did not go hand-in-hand – a challenge Kuhn never took up or even addressed. In the end, Hanson seemed critical of models of science that aspire to cover all possible cases, and asserted that legitimate history concerns itself with generalizations that usually admit of exceptions (Hanson 1965, p. 373).

Kuhn's perceptive article (1977) on the relation of the history and philosophy of science does little to free him of the charges that his theory is either entirely descriptive (Lakatos) or that it is semantically impoverished (Hanson). Kuhn asserted, like Hanson, that history and philosophy of science should retain their disciplinary identities, but should rely on one another

[10] I owe Eric Schliesser a debt of gratitude for having provided me with a copy of the referee report (Hanson 1961). I also thank Jordi Cat for having called the whole matter to my attention.

when needed. However, he represents philosophers as needing history far more than historians philosophy. He argues, based mainly on his teaching experience, that historians and philosophers bring characteristic intellectual 'sets' to their study of the history of science, and he characterizes the perspectival divide between history and philosophy as being like that encountered in an aspect shift.

Kuhn identified himself as a working historian, as one engaged in putting scattered elements of data into a sensible arrangement. He didn't view historians as dealing with arguments, but rather as constructing narratives based on a primitive notion of similarity, like that which guides a child through the putting together of a jigsaw puzzle. It is rather odd that he did not explicitly regard history of science as being governed by paradigms, particularly since history is a discipline that has undergone dramatic periodic shifts in the methods of argument, data selection, and narrative construction. Importantly, on Kuhn's view, history has little need of the insights of other disciplines, since its guiding methodological principle is primitive and, presumably, not subject to change. Clearly, the view that history is a methodologically self-contained discipline does much to stifle interaction with philosophy, as well as with other fields.

The notion of the "rational reconstruction", so critical to the historiography of the positivists and Popperians, does not figure prominently in Hanson's work. Needless to say, the radicalized version of the rational reconstruction championed by Lakatos, with all of its fascinating and troubling perplexities, is also not a part of Hanson's historiography.

If histories are to be reconstructed along the lines of a philosophical theory of rationality, in what way does that theory of rationality get critically appraised? Of course, if the theory of rationality is purely formal and *a priori* justified, then its critical evaluation will be unproblematic[11]. Lakatos's model of "sophisticated falsificationism", by contrast, does not seem capable of any history- or experience-independent justification – if anything, it seems merely calculated to align with our historically-informed intuitions concerning the historical workings of science. As such, the methodology of competing research programs appears to be incorrigible by more searching historical study or by future experience with science. The foundations of Lakatos's methodology, thus, seem to suffer from difficulties very similar to those plaguing inductivism, since such foundations cannot be derived either from pure reason or from experience. While Lakatos's model certainly seems "agreeable to reason", it appears to be incapable of falsification or rationally driven revision.

When philosophy of science is strongly identified with normative analysis, as it is in Lakatos's account, it departs from the simple world of facts and history. Lakatos claimed, for instance, that his methodology expresses a set of truths in Popper's third world (cf. Lakatos and Musgrave 1970, pp. 179-180, esp. fn. 1 on 179), and thus is not subject to falsification by the facts. Kuhn's reaction to Lakatos's historiography is typical of historians, and illustrates why Lakatos's historiography has found little sympathy among pure historians. While Kuhn admits that all case studies are rational reconstructions (Kuhn 1970b, p. 256), he asserts that a historian of science could never in good conscience present historically false claims as though they were true, as Lakatos advises. As we have already seen, Hanson was also critical of such moves, and argued that models of science, if they are to be anything more than word games, must be capable of coming into conflict with historical facts.

Hanson outlines the terms for a fruitful interplay between history of science and philosophy of science. What is distinctive about Hanson's view is the primacy of normative

[11] Popper's approach to scientific method attempted to embody the ideal of formal justification, though it famously failed since it had to introduce non- *a priori* elements in order to save itself from problems introduced by holism of testing and its poor fit with actual history of science.

criteria, both within the disciplines themselves and in terms of their interaction; the flipside of this is that normative constraints must themselves be capable of conflicting with the facts. The historian has a set of normative criteria that govern the selection of materials, and the construction of narratives – such normative criteria set the ground rules for the production of historical descriptions. In addition to those normative elements that are necessary for producing historical descriptions are those that are concerned with the rationality of certain historically situated beliefs and epistemic practices; it is here where philosophy of science and history of science have their most substantial overlap.

VI. Conclusions and Criticism

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. Of course, the hidden assumption in such an approach is that philosophical models of science must be *a priori*, an assumption that few philosophers today would accept without qualification, though the idea is still central to philosophy of science as a discipline. Historians, on the other hand, abstain from offering normative judgments for fear of engaging in 'whiggish' interpretation (Cf. Laudan (1990) and Nickles (1995)). Each of these positions is committed to a hopeless epistemology, and while Hanson seemed to share the philosopher's occupational scruples concerning the genetic fallacy, in many areas he clearly violated, with valuable results, these two disciplinary constraints. Furthermore, it seems that Hanson's own philosophical account, which suffers from some conflicts and tensions, if not inconsistencies, can be ameliorated by drawing together his account of conceptual dynamics and the appraisal of historical arguments. More importantly, such an improved interpretation of Hanson's position is capable of navigating a *via media* between the Scylla of deductive philosophy of science and the Charybdis of anti-whiggism[12].

Hanson's treatments of observation and Galileo's discovery of the law of free fall rely on historical and empirical facts; nonetheless, from such facts, normative lessons can be gathered. Clearly these discussions are concerned with *a posteriori* normative criteria. Philosophers of science are still concerned to analyze the foundations of our normative pronouncements, even those that are non-deductive. If one wishes to appraise the adequacy of a method of discovery, or evaluate the promise of a new theory, one then needs to assess the connections lying within a body of empirical data; i.e. one will be appealing to *a posteriori* normative criteria, just as Hanson implicitly referred to in his account of Galileo. In what follows, I will argue that broadening the Keynesian formula to range over facts about human cognition and the inference patterns used in discovery provides a powerful tool for the normative appraisal of science, since it would then cover both *a priori* and *a posteriori* normative criteria.

With respect to the Keynesian formula, Hanson did his utmost to craft an *a priori* justified instrument for the normative appraisal of the history of science. However, we might wish to inquire more closely into the type of normativity offered by Hanson's Keynesian formula for analyzing the well-groundedness of a theory at some point in history. The Keynesian formula specifies the probability for a given theory to be true at time *t*, and thus gives a measure of what one ought to believe at *t*. It grounds normativity of belief, but not normativity of method: it does not tell us how we ought to theorize if we wish to discover some new, or comparatively superior, theory. We might have expected, especially from Hanson considering his enduring interest in

[12] This is the metaphor Hanson uses for his middle course between formalism and sensationalism with respect to perception (Hanson 1971, p. 1)

discovery, a normative analysis, based upon historical data points, of reasonable methods to generate new theories. In fact, to confine our interest in the annals of history to the search for answers to the question, "What was the most reasonable belief to have about phenomenon *x* at time *t*?" is to leave out a great deal. We should also wish to know what the best way would have been to proceed at *t* in order to arrive at a theory capable of explaining *x*. It appears likely that a normative analysis of method would have to be *a posteriori*, since the idea that we could offer directives about how to conduct science optimally with no experience of the world seems absurd.

It is reasonable to include our knowledge of human cognition in our reckoning of all the facts as we know them. Doing so would give us greater capacity to appraise methods of theorizing; surely the facts concerning human cognition must figure in an account of how best to proceed in enlarging our knowledge, if not even our calculations of what would be the most rational thing to believe at *t*. The kinds of things we can know, and the kinds of inferences we are able effectively to produce, are relevant to how we should proceed in our theorizing. After all, everyday problem solving strategies are normatively loaded, as Hanson mentions, "Many features of the actual problem solving of ordinary people, and of ordinary scientists, require understanding the *criteria* in virtue of which one can distinguish *good* reasons from *bad* reasons."[13] (Hanson 1971, p. 64). Leaving these strategies out of our epistemology of science, simply because their logical character is non-deductive, leads to an impoverished epistemology.

Complete generalization of the Keynesian formula could lead to a full logical analysis of the conceptual dynamics of science. Not only would we be able to say which theories are most highly supported at *t*, but we could also determine which strategies – formal or informal – are most likely to lead to success during some historical period. Hanson's writings on the logic of discovery, particularly the early articles, were concerned with exactly such a project. It is useful

[13] Italics in original.

to distinguish between *a priori* and *a posteriori* forms of normativity, and it is a mistake to regard the *a posteriori* forms as so historically tainted that they are irrelevant to philosophers, just as it is an error to suppose that the historical objectivity of narratives will be inevitably contorted by considerations of rationality. Instead of absolutely separating the normative and the descriptive, philosophers and historians of science should remain aware of the ways in which normative judgments depend upon, or are corrigible by, empirical facts; conversely, the empirical facts must be seen as being filtered by over-arching, though revisable, principles of interpretation and selection.

References

Feigl, Herbert (1970), "Beyond Peaceful Coexistence", in *Minnesota Studies in the Philosophy of Science, Vol. V: Historical and Philosophical Perspectives of Science,* Roger H. Stuewer (ed.), pp. 3-11.

Giere, Ronald N. (1973), "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?", *British Journal for the Philosophy of Science,* 24 (3): 282-297.

Grau, Kevin T. (1999), "Force and Nature: The Department of History and Philosophy of Science at Indiana University, 1960-1998," *Isis,* 90: S295-S318.

Hall, Marie Boas (1999), "Recollections of a History of Science Guinea Pig," *Isis,* 90: S68-S83.

Hanson, Norwood Russell (1958a), "The Logic of Discovery," *Journal of Philosophy*, 55 (25): 1073-1089.

---------- (1958b), *Patterns of Discovery*. Cambridge: Cambridge University Press.

---------- (1961), "Report on Dr. T. Kuhn's *The Structure of Scientific Revolutions*" (Referee Report for University of Chicago Press).

---------- (1962a), 'Introduction' to Keynes's *Treatise on Probability,* pp. v-xi.

---------- (1962b), "The Irrelevance of History of Science to Philosophy of Science" *Journal of Philosophy*, 59 (21), pp. 570-586.

---------- (1964), "Observation and Interpretation" *Voice of America Forum Lectures: Philosophy of Science Series,* 9.

---------- (1965), "Notes Toward a Logic of Discovery", in *Perspectives on Peirce*, R.J. Bernstein (ed.), pp. 42-65. New Haven:

---------- (1971), *Observation and Explanation: A Guide to the Philosophy of Science.* New York: Harper & Row.

Keynes, John Maynard (1962), *A Treatise on Probability.* New York: Harper and Row. First edition 1921.

Koertge, Noretta (1976), "Rational Reconstructions" in *Essays in Memory of Imre Lakatos,* pp. 359-369. Dordrecht: Reidel.

Koyré, Alexandre (1978), *Galileo Studies.* John Mepham (trans.). Atlantic Highlands: Humanities Press.

Kuhn, Thomas S. (1970a), *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press. Expanded edition 1970. First edition 1962.

---------- (1970b), "Reflections on my Critics", in *Criticism and the Growth of Knowledge,* pp. 231-278.

---------- (1970c), "Logic of Discovery or Psychology of Research", in *Criticism and the Growth of Knowledge,* pp. 1-23.

---------- (1977a), "A Function for Thought Experiments", in *The Essential Tension,* pp. 240-265. Chicago: University of Chicago Press. Originally published in 1964.

---------- (1977b), "The Relations Between the History and the Philosophy of Science", in *The Essential Tension,* pp. 3-20. Chicago: University of Chicago Press.

Lakatos, Imre and Alan Musgrave (eds.) (1970), *Criticism and the Growth of Knowledge.* Cambridge: Cambridge University Press.

Lakatos, Imre (1978), *Philosophical Papers I: The Methodology of Scientific Research Programmes*, J. Worrall and G. Currie (eds.). Cambridge: Cambridge University Press.

Laudan, Larry (1990), "The History of Science and the Philosophy of Science" in *The Companion to the History of Modern Science,* R.C. Olby et. al. (eds.), pp. 47-59. New York: Routledge.

Nickles, Thomas (1995), "Philosophy of Science and History of Science", *Osiris,* 10: pp. 139-163.

Shickore, Jutta (2006), "A Forerunner? – Perhaps, but not to the Context Distinction. William Whewell's Germano-Cantabrigian History of the Fundamental Ideas", in *Revisiting Discovery and Justification: Historical and Philosophical Perspectives on the Context Distinction,* Jutta Schickore and Friedrich Steinle (eds.). Dordrecht: Springer.

Veatch, Henry (1997), *Towards a History of the Indiana University Philosophy Department in Bloomington: The Years 1929 – 65: A Personal Memoir.* Bloomington: Department of Philosophy, Indiana Univ.