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Norwood Russell Hanson

Matthew D. Lund *Editor*

Perception and Discovery

An Introduction to Scientific Inquiry

Second Edition

Springer

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and Philosophy of Science

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Norwood Russell Hanson

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Editor

Perception and Discovery

An Introduction to Scientific Inquiry

Second Edition

First Edition Edited by
Willard C. Humphreys

Springer

Norwood Russell Hanson (deceased)

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For Trevor and Leslie

Preface for the First Edition

Norwood Russell Hanson did more in his life than three good men. It is most fortunate that this substantial part of his unpublished work has been made available through the exceptionally devoted work of his friend and former student, W. C. Humphreys. As editor of this volume, he has put in far more of his time and of himself than the reader could infer from his modest comments. But the work itself bears Hanson's stamp on every page—forceful, informative, original, charming, witty, and colorful.

In a general sense, Hanson continues the application of the Wittgensteinian approach to the philosophy of science, as Waissman and Toulmin have also done. But he goes much further than them, exploring questions about perception and discovery in more detail and—perhaps his greatest strength—tying in the history of science for exemplification and for its own benefit. Hanson was one of the rare thinkers in the tradition of Whewell—a man he much admired—who could really benefit from and yield benefits for both the history and philosophy of science. He founded the only combined department in this country, and he was a paragon of its principles. There is a certain tension between the demands of exact historical scholarship and the more free-ranging interests of the overviewer—the philosopher of science in one of his roles. Hanson released this tension by simply working harder, so that he became an expert on such topics as the mathematics of epicycles, the development of twentieth-century physics and on inductive logic, etc. Philosophy of science is an immensely demanding field in itself since any comprehensive approach requires considerable scientific knowledge (*not* great research performances) as well as philosophical expertise, and it is immeasurably helped by a really good knowledge of the history of science. Throughout his work, Hanson is calling on all these skills—but he had many more, from musical to engineering. He always felt that the wider a man's scope of understanding is, the better his performance would be in *each* field of his practice, despite the time he would be taking away from specializing. So he was a true generalist, and the rewards of achieving this impossible goal are clearly exemplified in this book, making us more acutely aware of the loss involved in his early death.

But there is something paradoxical about regretting the fate of dynamite. Its nature can be fulfilled only in a way that carries with it the risk or fact of destruction. Hanson's great red Harley, the vintage Jaguar, and the ravenous Bearcat were the kinesthetic counterparts of the intellectual daring that distinguished him. Where angels feared to tread Russ would drive tanks in tandem, laughing. And half the time he'd get away with it.

The measure of the merit of a man is what he did where others failed, and in this book there are many such successes. It is quite uninteresting that there are some less-than-successes. Safety is not the name of success. The angels who fear to tread are sissies, Hanson would have said, and the price they pay for survival is insignificant; only gamblers get rich. It's a tough line, but I like it, even if it won't work with mountains. I wish there *were* angels: by now Russ would probably have them organized into a motorcycle marching band in the mornings and mounting the heads of pins for a population count in the afternoon.

Instead of Russ, we have only a good book—but it is the best kind of book to leave behind, his first textbook.

Berkeley, CA, USA Michael Scriven
1969

Editor's Prologue for the First Edition

When Norwood Russell Hanson died in a tragic plane crash in April 1967, he left a number of projects unfinished. One of these was the writing of an elementary textbook in the philosophy of science based on lectures he had given in recent years at Cambridge University, Indiana University (where he founded the present Department of History and Philosophy of Science), and Yale University. The basic material of this text comes from his original Cambridge lecture notes, modified and amplified and suitably updated. In addition, a number of new chapters were added. Of the latter, three plus a fragment of a fourth had been written in rough draft form by the time of his death.[1] The others—dealing with experimental laws, the role of notation, and methods of representation in science—were never done.

The present book has been constructed from the materials which were in completed or nearly completed form. It aims to be an introduction, suitable for use in the first year or second of a college or university student's work in science. It presupposes a wide acquaintance with neither philosophy nor science, only an interest in understanding science more fully. At the same time, it is a genuinely philosophical study which professional philosophers and scientists will find interesting and absorbing.

A number of changes have been made in putting the lectures into book form. Some of the English figures of speech and allusions have been Americanized or internationalized in order to make them intelligible to a broader audience. Some redundancies and "lecturisms" suitable for the classroom but not necessarily the written page have been omitted or altered. Footnotes have been provided (Hanson's original references were for the most part missing or incomplete); titles for chapters, for the main sections, and, indeed, for the book itself have been added; and at the beginning of each of the four main sections, a reading list of books—mostly books Hanson himself used to recommend to his students—has been inserted. Several illustrations and the calculations in the appendix to Chap. 12 have been supplied by the editor, either from his own recollections of Hanson's lectures at Indiana University or from other sources duly noted. In all of this, the aim was to preserve

[1] The three complete chapters form Part I of this book.

as far as possible the lively and exciting style which characterized Russell Hanson's work in the classroom and all of his writings.

Since the book was essentially incomplete as it stood, an editor's epilogue has been added to tie together some of the loose ends and provide students with a summary of some of the main points covered in each section. For any misinterpretations therein, the editor assumes full responsibility.

In terms of subject matter, one of the most unique features of the book is its extended treatment of the nature of scientific observation. Here Hanson gives, in a fuller version than has ever before appeared in print, a defense and exposition of the Wittgensteinian, ordinary language theory of perception and its ramifications for scientific observation. Opposed points of view (phenomenalism and the ocular-neural causal theory) are given fair treatment, but essentially Hanson is concerned to bring to bear on scientific practice the lessons which the ordinary language movement has to teach.

Since the ordinary language theory of perception has many points of contact with Gestalt experimental psychology, there is a good deal of discussion in Part II about Gestalt theory. Had Hanson lived, he doubtless would have wished to bring the experimental references there fully up to date. The editor has not done so chiefly because Hanson's argument is not substantially altered by recent findings of experimental psychology; it is a philosophical argument about perception anyway, not one which can be refuted or confirmed by new experimental findings (which is not to say, however, that they are altogether irrelevant).

The editor is indebted to Professor Stephen Toulmin of Brandeis University, who has served unofficially as literary executor of Hanson's estate, for his assistance and permission in bringing the manuscript to publication. The editor should also like to thank Mrs. Margaret Freeman for her estimable help in copy editing; Mrs. Sally Rahi of New College, Sarasota, and the editor's brothers, Dr. James Humphreys of the Institute for Advanced Study, Princeton, and Professor Lester Humphreys of the University of Massachusetts Boston were of great help in hunting up missing references and illustrations. Lastly, the editor wants to thank Mrs. Fay Hanson for her cooperation in making this book possible.

Readers who are interested in pursuing other writings of Hanson's should see Volume III of *Boston Studies in the Philosophy of Science* (Cohen and Wartofsky 1967), where a complete bibliography may be found.[2] The same source contains much biographical material in the form of memorial notes from philosophers of science, historians of science, and scientists the world over. Hanson's own favorite among articles written about him is a piece called "The Bearcat Professor," by James Gilbert, associate editor of *Flying Magazine* (March, 1966).

New College
Sarasota, FL, USA
1969

W.C. Humphreys

[2] Readers will find an updated bibliography of Hanson's works in Lund (2010, 229–236). –*MDL*.

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Contents

Introduction

Perception and Discovery is the pedagogical counterpart to Norwood Russell Hanson's celebrated 1958 book *Patterns of Discovery.* Both books were mainly written in the middle 1950s, when Hanson was teaching at the University of Cambridge, though *Perception and Discovery* only appeared posthumously in 1969.

The charismatic and original Norwood Russell Hanson is very much alive in the pages of *Perception and Discovery.* Not only was Hanson a gifted philosopher, he also excelled in pursuits as varied as boxing, music, shot putting, drawing, and aviation. Sadly, Hanson's love of flying led to his early death at the age of 42. Beyond all of Hanson's many talents, it was his forceful personal presence that made the greatest impact. Michael Scriven's Preface provides a compelling and accurate portrait of the man who was Norwood Russell Hanson. Scriven's view is not the mere preferential perspective of a friend. Nearly all who knew Hanson – even those who didn't like him – were awed by his tremendous energy, fertility, and polymathic talents. Both his students and his colleagues were wowed by this imposing and fearless dynamo.

Perception and Discovery offers an intimate access to Hanson's famously engaging lectures. The director, actor, physician, and public intellectual Jonathan Miller vividly recalls Hanson and his potent influence: "[Miller] found Norwood Russell Hanson's tutorials truly life-changing, for this philosopher of science introduced him to the notion that supposedly objective systems of classification might, in fact, be subjective. That may sound arid or abstract, but Hanson was a hugely vivacious American, an ace trumpeter-turned-pilot-turned-Fulbright scholar. He had a dramatic mane of hair and rubber-soled, springy shoes. In his tiny office, up rickety stairs in the Whipple Museum of the History of Science, Hanson would literally bounce with excitement when demonstrating the principles of thought, and Miller emphasizes that his world was turned upside down by this intoxicating man's ideas." (Bassett 2012, 66)[1]

One of Hanson's most attractive characteristics was his capacity to see science – and philosophy of science – from the vantage point of students of science. Rather

[1] Bassett's book is largely composed from interviews with Miller.

than pompously invoke philosophy's storied history or its reputation for clear and logical thought, Hanson preferred to start right where the science student experiences the first traces of bewilderment and discomfort – with what more romantic and even classical ages called "the sense of wonder". Wonder, like all truly valuable things, cannot be manufactured. Hanson also possessed a rare capacity to get scientists to see the important contributions philosophy could make to their field. He did this primarily by showing that confusion in science frequently derives from those philosophical presuppositions that scientists are taught to ignore in their science classes.

Perception and Discovery is Hanson's most thoroughly Wittgensteinian book. Hanson was at Cambridge during the apex of Wittgenstein's influence, and Hanson's adroit application of Wittgenstein's philosophical approach to science reveals science to be a fascinating and creative enterprise. Hanson's approach was, first, to study science as it was and, second, to analyze science philosophically. If the philosophy of science were somehow produced in complete isolation from science, how could we ever hope for such a philosophy to apply to science? From Hanson's perspective, most philosophers were so concerned with preserving a cherished epistemic image of science that they left real science out of the equation. Hanson cautions against premature acceptance of monolithic definitions; he asks the reader to reflect on all the different, and even opposed, uses to which scientific terms are routinely put. Only once we understand each of these usages, and their roles within their specific contexts, will we be able to appreciate the meanings of particular utterances and assertions.

Hanson is sometimes characterized as a brash and sloppy philosopher; insightful and creative, but lacking in patience. Many who knew him best even described him in this way. Peter Achinstein typifies this species of criticism: "Hanson was a Van Gogh rather than a Vermeer among philosophers. He lacked patience for fine detail. His penchant was for rough brush strokes that would suggest the important features and that might stimulate others to pursue details." (1972, 241) While there is certainly truth in these remarks, I believe they somewhat miss the point, particularly of Hanson's pedagogical writings. In the Wittgensteinian tradition, Hanson was often content to expose the conceptual dogmatism at the root of received positions. He found shifts in perspective exhilarating, and communicated his enthusiasm to his students. Hanson was not interested in just shaking things up momentarily, only to immediately slip in a new replacement position. Instead, he fostered an appreciation for the fascinating philosophical terrain that lay quietly submerged beneath the apparently calm surfaces of our most securely held scientific theories. Only perturbations can reveal what lies below. No matter how much we love stability and simplicity, we should love the truth more, even if the truth should turn out to be aesthetically disappointing.

Perception and Discovery hails from an era in which one could unapologetically refer to *the* philosophy of science. People are much shyer about employing the definite article nowadays. In fact, contemporary philosophy of science appears to be in something of an identity crisis. While knowledge of the history of science and contemporary scientific theory is highly esteemed, there is little consensus concerning philosophy's contributions to science. In fact, there is even considerable disagreement concerning which areas of science are "philosophical". This contemporary predicament is in contrast to the environment in which Hanson worked – an environment in which the scientists, the philosophers, and the historians believed they were working under a shared vision.

Hanson saw philosophical thinking as a component of all good scientific inquiry, and his Cambridge colleagues in the sciences largely agreed. The inclusion of history and philosophy of science (HPS) in the Natural Sciences Tripos was not pushed forward by historians and philosophers, as we might now expect. The impetus came from the scientists themselves, with physicist J.A. Ratcliffe as the principal champion. Hanson's approach mirrored that of the scientists: he generally eschewed philosophical treatments that either contemplated the moral consequences of science or traded in meaningless "word painting" that ignored the weighty contributions of science to our contemporary view of the world. Hanson found philosophy for philosophy's sake distasteful – he was always determined to show philosophy's relevance, if not necessity, to other fields. Also, at Cambridge, Hanson was not merely teaching philosophy to young science students. He played an important role in educating them as scientists: in his final years at Cambridge, Hanson was the Head of Examiners for the Natural Sciences Tripos.

Due to its high level of interdisciplinarity, philosophy of science now suffers from obscure territorial boundaries. Interestingly, Hanson is often ranked alongside Thomas Kuhn and Paul Feyerabend as one of the architects of this new and diffused brand of philosophy of science. This routine and – truth be told, often thoughtless – characterization is most unfortunate, for it leaves out all the features of Hanson's thought that made him such an excellent bridge between disciplines. Hanson, in fact, stuck fairly closely to the everyday language school of philosophy, and also bought into many of the articles of faith of logical empiricism and falsificationism. For instance, Hanson believed that conceptual analysis is a purely philosophical activity, that mathematics and science are distinguished on the basis of the analytic-synthetic distinction, and that worthy hypotheses must be verifiable and falsifiable.[2] However, he is remembered best for his departures from the received logical empiricist views: he argued that observation is theory-laden, that scientific discovery is a rationally appraisable process, that knowledge of history of science is essential to philosophy of science, and that history of science is "blind" without philosophy of science.

Hanson had a keen sensitivity for disciplinary boundaries, and this sensitivity no doubt enabled him to traverse these boundaries, deftly and profitably, in a way that few have been able to mimic. Hanson's argument that observation is theory-laden nicely demonstrates this point. Kuhn's appropriation of the theory-laden observation thesis emphasized that a paradigm is a prerequisite to observing the world; without some preexisting framework, scientific observation is not possible. In a certain sense, Hanson would agree. However, Hanson makes the further conceptual argument that things could not be otherwise. Language, notation, observation, and

[2] See Chap. 13 of this volume for Hanson's views on these matters.

prediction – all being such different kinds of things on the conceptual plane – must somehow be fused together in order for us to make sense of the world. It is our theories that act as a great conceptual glue[3] – the better our theories are, the better they cement together the varied conceptual elements comprising the epistemology of science. Hanson did not argue that his thesis is a consequence of an unprejudiced study of the history of science (as Kuhn did). Instead, Hanson saw the theory-laden observation thesis as a general epistemic thesis derivable from everyday experience, but made more poignant and obviously necessary through contemplation of science. Hanson was roundly contemptuous of the positivist tendency either to ignore or obscure the complexity of the conceptual landscape by formulas and intellectual legislation. He troubled himself little with denunciation of specific positivist systems, preferring to forge ahead with his own Wittgensteinian analysis. After completing his ordinary language demonstration that thought and perception must thread into one another, Hanson turned to the burgeoning experimental psychology of the 1940s and 50s to fill in some of the empirical details. Anticipating the views Quine would later espouse asserting that epistemology can be considered a chapter in psychology, Hanson vigorously argued that psychology, being a factual discipline, can never answer philosophy's logical and normative questions. Thus, in Hanson's account of observation, he shows how the various disciplines must cross paths, and even spill over into one another, but he does not deny the independent adequacy of each of the disciplines.

Hanson's first major published work, *Patterns of Discovery,* significantly altered the face of philosophy of science. The book was generally well-received even by those the book was later understood to have criticized. Hanson believed that the study of HPS would free young scientists from the shoddy philosophical dogmas implicit both in their scientific studies as well as in their laboratory and professional engagements (Hanson recognized that students take their biases and preconceptions more heavily from the latter source). Hanson believed, no doubt as a result of his association with the brightest scientific minds of the day at Cambridge in the 1950s, that the best scientists are those with the broadest historical outlook and whose creativity was not fettered by conventional philosophical formulas. To emulate Heisenberg, Dirac, or Bragg, one had to uncover the philosophical presuppositions that covertly arrested new development, and then erect a new set of presuppositions capable of fostering new theoretical growth. HPS, Hanson thought, could help scientists develop the clarity of vision and the audacity of invention to produce landmark discoveries and theories.

Many of the lessons taught in *Perception and Discovery* were taught to the still young Hanson at Cambridge. Hanson attended the physics lectures and laboratories along with his students, and he and historian A. Rupert Hall made sure that the history and philosophy of science they were studying in their individual sections matched up with the physics curriculum week by week.[4] One must bear in mind that

[3] Hanson's metaphor. See Hanson (1955, 7).

[4] Hanson also learned much of his own science through this process: Hanson had only a slight undergraduate education in the sciences.

subjects like history and philosophy of science were not standard university subjects in the early 1950s. The critical, if not antagonistic, relations these specializations were later to develop with science had not yet materialized. Much about Hanson's thought and academic career is made intelligible by this observation. Hanson was no *enfant terrible*, mischievously poking epistemological holes in the fabric of science. Instead, Hanson was trying to *deepen* the students' comprehension of science. In many ways, Hanson's determination to communicate the relevance of philosophy to undergraduate science students was one of the most decisive and enduring aspects of his academic career. It is no exaggeration to assert that Hanson's very image of himself as an academic is most completely encapsulated by his preoccupation with the philosophical education of scientists.

According to Hall, the scientists at Cambridge introduced the teaching of HPS to promote the "broad notion of the literate scientific culture." Hall asserted that this exploration of history and philosophy in science was likely thought to be little different from how non-scientific disciplines used history and philosophy: "History and philosophy of science were elements in the culture of science, their special significance being their enrichment of a scientist's vision of what his study, occupation, and passion might be, just as (I imagine) knowledge of the works of Bach and Mozart is supposed to enrich the musical experience of a young musician." (1984, 25)

Before the name Thomas Kuhn was well-known, Hanson was inveighing against the evils of the textbook approach to science. The textbooks, in Hanson's view, sought to simplify out of existence all the elements of scientific thought that enabled the development of science in the first place; through their brevity and focus on narrow performative results, textbooks not only obscure the true face of science, they also stifle the students' curiosity. Hanson abhorred the textbook program of arranging all the content of a field of science into a dainty catalogue of formulas, the mastery of which would qualify one as a scientist. Instead, Hanson saw science as a great human drama, possessing the fullness and imperfections of our all-too-human natures.

What kind of textbook, then, would we expect from someone who hated the very idea of textbooks? One that is probing, provocative, light on didactics and veneration of authorities; one that challenges its readers to think for themselves about those exact issues that standard textbooks pass over either in embarrassed silence or with a profusion of high-sounding, but ultimately meaningless, phrases. In terms of its audience, *Perception and Discovery* is a textbook; in terms of its goals, it strives to create the perspective of the philosophically astute scientific inquirer.

In advertising the success of Cambridge's new approach to educating scientists, Hanson said that philosophical discussion of problems arising in the physics curriculum destroys "the schoolboy conviction that physics is a great shelf of thicker, more unreadable textbooks and directories, all containing the right answers in the back pages. If science were just such a shelf then there would indeed be an unbridgeable gap between it and other disciplines. No student with initiative or imagination would dream of undertaking such a study." (1955, 7) Thus, philosophy illuminates the true conceptual topology of a scientific field, and once one has come to see science in this more mature and comprehensive way, one is no longer seduced by silly oversimplified accounts of science.

Hanson's originality is on full display in *Perception and Discovery*. By the middle 1960s, philosophy of science texts were starting to proliferate, such as those of Ernest Nagel, Carl Hempel, Karl Popper, and Arthur Pap. Most philosophy of science textbooks had a broadly logical empiricist orientation, which involved starting from the problems that initiated the movement and terminating in the favored solutions to those problems.[5] In such books, the philosophy was certainly in the driver's seat, and the proffered solutions delivered were of a decidedly philosophical character and not calculated to affect the way fledgling scientists would approach their profession. Hanson makes little direct reference to that august tradition, and where he obliquely mentions it, his intention is usually to display its defects. Hanson is not after a set of "philosophical chestnuts" he can cull from science and then crack; instead, Hanson is interested in providing a philosophical analysis of science, particularly the thorny parts that either get finessed or over-simplified in the textbooks. Hanson's goal is not to delineate an alternative orthodoxy of philosophical positions – he is satisfied with the more modest goal of getting science students to engage philosophically with science.

Hanson's most enduring contribution to the philosophy of science is his position that observation is theory-laden. In *Perception and Discovery*, one finds Hanson's most complete and clear treatment of observation. While the celebrated first chapter of Hanson's *Patterns of Discovery* is the *locus classicus* for the theory-laden observation idea, the much more exhaustive treatment in the present volume offers a more definitive and satisfactory presentation of theory-ladenness. Notably, *Perception and Discovery* presents Hanson's refined thoughts on the topic following the publication and critical response to *Patterns of Discovery*. Hanson was not one to embrace criticism openly, but in this work he is clearly attempting to answer the many objections that had been leveled at his account. The subtle alterations and elaborations of his original views on observation in the present work typify Hanson's elusive manner of incorporating criticism. It is rather unfortunate that philosophers have for so long criticized the concise treatment of theory-laden observation from *Patterns of Discovery* without having taken Hanson's more elaborate and mature position into account.

Hanson rightly abuses traditional philosophical approaches to perception by pointing out that they define the conceptual landscape explicitly to vindicate some pet epistemological theory. Hanson was bothered by the philosophical tendency to venerate ideal systems to the scorn of reality. Hanson was no builder of systems or a dabbler into intellectual games playing – his philosophical curiosity always inclined toward illuminating areas that other philosophers sought to define out of existence, like observation and discovery. By defining observation as being incorrigible, as many philosophers in the analytic and logical empiricist schools did, the capacity to say anything meaningful about observation was forfeited.

[5] A notable exception to this trend is Stephen Toulmin's *An Introduction to the Philosophy of Science.* Toulmin was a close friend and mentor of Hanson, and he organized the publication of Hanson's posthumous works.

The general position of traditional philosophers is that we cannot err concerning what we see, since seeing is either the stimulation of our sensory receptors or the reception of sense data, the latter being purely phenomenal objects only spoken of by philosophers. Once the raw data of perception arrive, they are then processed, in a second and discrete step, by interpretation; interpretation being a thoroughly mental and self-conscious activity, always subject to the will of the observer. Against this orthodox "two-phase account"[6] of observation, Hanson counters that scientific observation is already imbued with organization and interpretation. It is only in the rarest and most extraordinary circumstances that we see things without the benefits of interpretation. In such circumstances, we crave intelligibility, and do not feel comfortable until we hit upon a theoretical framing of our perception that allows mere sensation to coalesce into observation. If we look at actual scientific observation, we find that it is always closely tied to theory and expectation – those very connections, instead of being merely accidental as the orthodox view would have had it, are of the very essence of observation on Hanson's view.

Hanson argued that there is a sense in which the 13th and 20th century astronomers saw the same thing at dawn and different things. He pointed out that most philosophers sought to emphasize the sense in which the two saw the same thing, and they did this to protect cherished notions of objectivity. Against the grain of all his contemporaries, Hanson argued that the sense in which the two see differently is the one of interest to philosophers. How they see differently reveals the different theories that each one holds to make the world intelligible – we should focus on the profound differences between these theories, and what they predict about future experience, instead of contenting ourselves with trivial similarities in sense data reception or in visual physiology.

It is customary for philosophers to credit Quine with having ushered in the naturalistic turn, this despite the fact that few philosophers have found Quine's particular conception of psychology (behaviorism) attractive or revealing. Hanson, by contrast, made a serious study of the experimental psychology of vision, and carefully showed how such factual inquiries could fit alongside his analysis of concepts like seeing, observing, and knowing. Hanson's masterful presentation of the empirical background for observation is truly the highlight of *Perception and Discovery*, and one wonders whether his approach to naturalizing epistemology would not have been greatly influential had he not died before being able to develop it further.

One salient difference between *Perception and Discovery* and *Patterns of Discovery* is the very scant treatment Hanson gives of scientific discovery. Hanson had argued that there is a logic of discovery, and that our capacity to render ambiguous figures intelligible through application of familiar concepts is a model of rational discovery.[7] However, as Hanson's career progressed, his optimism about producing a logic of discovery cooled, and he moved in the direction of providing a

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[6] See page 62 of this volume.

[7] On page 141 of this book, Hanson claims that discovery occurs in the "strip" that lies in the vague area between the noticed and the unnoticed. That discussion is very much in the spirit of his earlier views concerning the rationality of discovery.

general taxonomy, or "anatomy" of discovery. Despite this apparent retreat though, Hanson believed to the end that discovery was a rationally appraisable activity. The absence of a sustained treatment of discovery in this book is more likely a result of Hanson's pedagogical aims than of his having soured on the idea of a logic of discovery. The fresh-faced student in introductory science courses will have no first-hand experience with what historians, philosophers, and scientists call "discovery", and precious little understanding of the second-hand accounts of discovery from the history of science. Much science and history will need to be learned before it can even be clear what it is that is so significant about the notion of discovery. As an introduction to the philosophy of science, this book is concerned with the less spectacular form of discovery experienced by the student learning to see the world as a scientist.

Finally, some appreciation for W.C. Humphreys is in order. Humphreys was a friend and student of Hanson, and, like Hanson, was a man of many talents; in music, especially, he was very accomplished. To Humphreys fell the unenviable task of editing both the present volume and *Constellations and Conjectures*. Unfortunately, Hanson's original notes and manuscripts for both works have not survived; from the variable texture of both books, it can readily be surmised that a tremendous amount of labor was required of Humphreys to put them into organized publishable shape. Beyond Humphreys's brief comments in the Editor's Prologue, we have little to go on concerning the conditions under which *Perception and Discovery* was written. Due to the frequent references to local features of Cambridge, Indiana, and Yale – the three universities at which Hanson had academic posts – one can surmise that Hanson had been working up the materials for *Perception and Discovery* throughout his academic career. As a posthumous book, *Perception and Discovery* certainly suffers from not having been scrupulously edited and reflected upon by its author, and there is some natural doubt concerning nearly every part of the text. However, it is fortunate that Humphreys did such a thorough job of editing, and his Editor's Epilogue is one of the most perceptive short accounts of Hanson's philosophy available. Thanks to Humphreys's hard work, we get to experience once more Hanson's lively writing and his distinctive personality, not to mention the most mature expression of his philosophical views.

Rowan University Matthew D. Lund,
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Part I
Provocations and Restraints

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Chapter 1
On Philosophizing—and Some Logical Distinctions

Historians of science are more than mere chroniclers. They are not content only to construct a master record of what happened and when—of discoveries, inventions, and scientific personalities, of birthdays and family connections. True, many books on the history of some science read as if the author were designing a kind of periodic table or a calendar or a genealogical tree of the events which have made the science what it is. But this is to history of science at its best as bird watching is to genetic theory.

History of science is concerned with *ideas*—with the thinking of scientists. And this is also what the philosopher of science is interested in, except in a radically different way.

Once it is admitted that doing science does require thinking, it is clear that these two related studies are immediately important to an appreciation of that thinking. Thinking evolves, and it has an internal structure. The historian explores the evolution of scientific thinking and ideas. The philosopher explores the internal structure of scientific thinking and ideas. This internal structure is only generalized from what obtains every day when any scientist is said to have an adequate grasp of a certain concept. Would we ever say this of a man who lacked *all* knowledge of the development of an idea and all knowledge of its internal structure—its logic? Hardly!

So the historian of science is not a Royal Society bookkeeper or an A.A.A.S. librarian, kept just to settle future claims as to the priority of inventions and discoveries. He is an explorer. He seeks those factors in the intellectual environment of a given period which led to the initial formation of a certain pattern of thought. He wishes to disclose new dimensions in old concepts such as *acceleration*, *force, mass*, *charge, field, point,* etc. He does this by revealing factors which inclined men of different scientific periods to fashion these concepts one way rather than another, this way rather than that. Just as we can understand a man's career better when we know something of him—how he has behaved on similar occasions and why, what his views are on the matter which led to his action, etc.—so we shall have a better

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grasp of a scientific concept, e.g., H_2SO_4, when we know something of what led chemists to express themselves in this way with respect to this substance.

It has been remarked that the formula H_2SO_4 contains the whole history of mankind. As in most exaggerations, there is a kernel of truth here.

Few would deny that the sciences *have* a history, that the history of science *exists.* Philosophy of science, however, is not always granted even that minimal claim. Since this book is an exercise in this black art, we had better proceed to do in detail for philosophy of science what has been done cursorily for history of science. As before, I will begin by saying what philosophy of science is *not,* or (at least) what it need not be.

If history of science is not chronicle, then philosophy of science is not a secular religion for conscience-stricken laboratory researchers. In this decade the question "Whither science?" has been posed *ad nauseam.* Divines, demagogues, and despondent dramatists have viewed science—microphysics and biochemistry, rocketry and genetics—as the instrument of gleeful Frankensteins bent on creating the uncontrollable. And so they are led to "philosophize" about the future of our civilization under titles like "Religion and Science," "Science and Future Civilizations," "Are Scientists Human?" Doubtless, in an age of bigger and better bombs, such questions are worth discussing—they are even worth discussing carefully, which is too rarely done. But no matter how carefully they are discussed, these questions are not issues of internal importance to the teaching of science. They are concerns of a different order. They affect scientists no more than they affect other members of the community. They are matters affecting the scientist as a citizen, not as a scientist.

If there is a real case for the introduction of history and philosophy of science into undergraduate courses, it must consist of the possibility that, in some derivative sense at least, men may become better scientists as a result. It is this stronger claim that we should consider. But, however that discussion fares, the speculative, deep-purple variety of "philosophizing" to which I have alluded finds no place in philosophy of science as it will be dealt with in this book.

Philosophy of science cannot, of course, increase manual dexterity. It is not wholly unrelated, however, to the business of sharpening one's wits—the business, that is, of carefully considering the character of one's experimental problems, the logical structure of arguments and proofs, and the general nature of a science's subject matter. The details of all this will be set out. But let us first allude to another thing that philosophy of science is not, or need not be. For scientists often recoil at the sound "philosophy of science" for yet another reason.

They rightly dislike the idea of academic philosophers and collegiate historians telling them, and the world, what science is all about. If physics were beset with all the problems that professional philosophers and historians manage to find in it, then doubtless they would be handy chaps to have around the laboratory, and around every school and university concerned with the teaching of science. But here the scientist will ask, "How can book-scholars who are unlikely ever to have seen the insides of a modern physics laboratory—who have never muddled and groped through the perplexities of a research task of their own, or felt that profound unsettlement which attends every decision at the frontiers of scientific inquiry—be relied upon to know what are the conceptual problems of physics?" Well, they cannot be relied upon for that, not unless they themselves have been scientists. Indeed, to have been a scientist is an indispensable requirement for anyone concerned with writing and teaching these subjects. Unfortunately, it is not met by enough individuals who expound on the history and philosophy of science.

This revealing question gains force when one sees how unrecognizable to laboratory researchers are some of the problems which "pure" philosophers have about the natural sciences. E.g.: "How can one 'construct' concepts of electrons out of visual impressions of pointer readings?" "How can one justify the use of inductive procedures in natural science?"

"Is science possible?"

If you think the answer to this last gem is an obvious "Yes!" just because your school is crawling with science, you have not read some of the more "profound" and arresting judgments on the matter. Since the time of Kant, experimentally innocent philosophers have been industriously digging up the *presuppositions* that got buried beneath the superstructure of modern science. (Kant, incidentally, was not experimentally innocent, but neither was he grossly guilty. Besides, he had other problems.)

The search, then, is for the logical guarantee that science is built upon a rock, and not on a bog. Or, alternatively, what can science *really* help us to know? Can we ever determine matters of fact with the surety which characterizes mathematics? (Don't just sneer "Yes"; and don't just sneer "No," either.)

Suppose a Yale professor were to pride himself so much on his sobriety and rationality that he wrote an essay on sobriety. Naturally, in that work he would lament and deprecate the sentimental, gushy impressionism of some of his colleagues. But the sentimentalists will surely smile and whisper, "Isn't 'the proof' sentimental about sobriety and rationality?" Metaphysicians (2nd class) have for generations actually earned their livings by whispering (in stage whispers), "Aren't scientists unscientific about their presuppositions—about their faith in induction and their dogmatism regarding what are and are not meaningful questions, about their acceptance of principles like 'All molar physical magnitudes are linked to continuous functions,' and 'Repeat the cause of X and X will occur repeatedly'?"

Mature scientists pay little attention to these academic worries. Would a lawyer worry on being accused of uncritically presupposing the principle that important evidence may be produced by cross-questioning witnesses? He *does* presuppose it, but so what?

Thus the unabashed metaphysician can often give his argument away by confessing in his question that he knows not whereof he speaks. Such is the case also with a certain kind of epistemologist (one who theorizes about the nature of knowledge). This particular species of epistemologist, of which but few living specimens are now extant, managed to baffle himself about the data of science. Impressed by the fact that we are sometimes mistaken in our descriptions of how the world is furnished, these philosophers fancied that if science were really to succeed, the "stuff" of observations (objects, events, situations) ought to be analyzed and segregated into those components which are *strictly* supported by our sensations and those which are not—these latter being but inferred, or constructed out of what we really do have as genuine physical experiences.

"That is a galvanometer," we say—but it *might* be a wireless set, or a mousetrap. True enough.

"Ah, there is the diffraction pattern," we say. But perhaps the Christmas cheer was a little too strong.

I might declare a band of light to be almost monochromatic. But perhaps my oculist knows something about me that he isn't telling.

In short, *we could be wrong.* That, indeed, is the logic of factual statements. Nonetheless the epistemologist may point out that concerning some things we cannot be wrong; it is certain that something galvanometerish dominates my visual field. No one outside my skull can deny that diffraction-like patterns appear when I open my eyes; who should know better than I what impinges on my retina or at least of what visual imagery I am aware? That band of light may not, in fact, have wave lengths of 5890 Å and 5896 Å, but that I am entertaining a sodium-yellow patch is indubitable. And so it goes. All experience is experience *of,* and the incoming signals are all we really have to go on. If only scientists would come to recognize the *priority* of exclamations like "red now," "pointer-image oscillating," and "buzz, buzz"—if only they would compound these sense-experiences in a truly logical manner to "form" the material objects of the laboratory and our world—then and only then, these epistemologists suggest, science will not be the "wobbly, illogical heap of half fictions" that appear so regularly in the pages of *Nature, The Scientific American,* and *The Review of Modern Physics.*

In terms of these epistemological criteria science surely is shaky. And so is everything else. It will not surprise you therefore to learn that these criteria are seldom invoked by non-philosophers.

Later on we shall explore some of the epistemological matters much further, especially as they bear upon our notions of *fact, observation, causality, theory*, and *hypothesis.* But for now we shall simply declare with delicate dogmatism that as a general approach to our studies in philosophy of science the posture just depicted is wholly unsatisfactory.

The third "improper" question to be considered, while just as easy to puncture as the other two, is more difficult to deflate, for it is not all hot air. Let us parody it thus: How can an experimental scientist, in the reports of his research, most closely approximate to the manner of exposition of the pure mathematician or the formal logician? Can he do this at all?

Together let us answer "No!"—perhaps even "Thank heavens, no!" This verdict is written in every bit of laboratory guesswork, in every crude set of apparatus, and in every persistent perplexity which refuses to disappear simply through more deduction. Indeed the whole tradition of natural science at places like Cambridge, Göttingen, Harvard, and Moscow is expressed in the phrase "sealing wax and string." These commodities are only slightly less useful today than they were in the glorious past. Still, concerning the dispensability of axiomatization in science there have been judgments to the contrary, and passed by some very able logicians and mathematicians.

One of the latter might argue:

> It is not my intention to suggest that the ideal for laboratory research be that it might one day be carried on by chromium-plated, self-correcting, algebraically-programed automatons. Scientific discovery will always be to some extent a groping, stumbling affair, ever requiring great ingenuity, insight, and imagination. This is because it is a step into the dark, into the uncharted unknown. And there is no way of lessening the risk incurred in taking that step. The mathematical-logical philosopher, however, is not concerned with the actual things an experimenter does, says, thinks, or feels—his inner mental life, his I.Q., or his digestion. He is concerned, rather, with the logical relations between, for example, the general statements which stand at the head (or alongside) of a given theory and the myriad specific statements which follow inferentially from them—or between an hypothesis and the evidence in support of it. It is the formal, logical structure of bodies of scientific knowledge, and not the behavior habits of any or all scientists, that interests those of us whose philosophy of science is studded with symbols, deductions, and entailments.

Now this kind of philosophizing about science is not to be despised, (Why, some of our best friends are "logical reconstructionists"!) Who will deny that many important advances in modern science were of a distinctly logical cast? I should argue strongly that this was so, that the history of scientific progress is *not* a history of increasingly refined laboratory technique but a history of changing *conceptions.* Something was looked at in a new way, the priority of some principle of nature was challenged, a set of deductions or inferences was compared with another set, ultimately to conflate the two, or to mark out differences, or even contradictions, between them. The names of those who have made such advances are familiar enough. Philosophers and logicians have rightly interested themselves in exciting systematic advances like these, and concerned themselves with the formal connections and interconnections between aspects of certain scientific theories. It is both enjoyable and intellectually profitable so to concern oneself with the sciences.

What *is* objectionable is this: The philosophy of science is often identified exclusively with just this sort of activity. Most of the important logical and philosophical aspects of the sciences can be examined without a prerequisite study of the theory of deductive systems—without even assuming any great facility in symbol manipulation, though this is, of course, a distinct advantage.

Hence, this third interpretation of "philosophy of science" is somewhat inadequate, I think, not because philosophy of science in this sense is not worth doing, or because it is incapable of interesting experimental scientists. It is inadequate because it is but a small chapter in a very large volume, a chapter too often presented as if it were all that had to be said. The danger of distortion is therefore great with the philosophers of science who spend all their time writing and rewriting this one chapter.

Apparently, then, the subject will be developed here in a different way. The questions "After science, then what?" (the consequences of science), "Is science possible?" (the assumptions of science), and "Can natural science be made into a formal discipline?" (the axiomatization of science) will not figure dominantly in our discussions. You may well ask, "Then what will?"

Let it be said once and for all that there is no *subject* to be called "philosophy of science"—not if by a "subject" is meant a subject matter, i.e., a collection of unique facts, plus a set of specially designed theories and specific rules for interpreting those theories in terms of facts, or vice versa. There is nothing to memorize, no formulae or tables to be taught. But there are lots of questions to be asked.

Now these questions are of a logical type different from those to which you may be accustomed. Here are some questions about the game of chess to illustrate differences of logical type: "How many pawns does white have?" "Why is it that the bishop cannot move along the edge of the board?" "Did Fischer make the best possible move at 15?" "Why do you speak so highly of Capablanca's game of 1925?" Note how very differently we assess the meanings of these questions. And note the different kinds of inquiry involved in giving an answer to them, and the different kinds of criteria appropriate to assessing the status of each of these answers: E.g., I can tell you how many white pawns are on the board by looking and counting them. But looking and counting are not involved in referring you to the *rule* that bishops must move diagonally. And reference to Bobby Fischer's move as the *best possible one* in the circumstances involves a subtle mixture of considerations involving matters of tactics, issues of strategy, the history of the game, and even some assessment of the abilities of Fischer and his particular opponent. Finally, thinking well of a move or a game involves many further things, some of them bordering on the aesthetic. It is in some such way as this that philosophical questions *about* and *within* science are of a logical type different from those to which you may be accustomed, as, e.g., "What is Avogadro's number?", "How does gastrulation proceed in the coelenterates—by invagination, immigration, delamination?", "What is the half-life of oxygen 17?" Questions like these will not arise here directly, though questions about these questions certainly will.

It cannot now be said precisely what it is that characterizes philosophical questions like: Are Protozoa one-celled or non-celled organisms? What are the meanings of "principle" in the expressions "principle of least action," "principle of the rectilinear propagation of light," or "principle of natural selection"? And what are the meanings of "law" in "law of nature" (e.g., Snell's law, Boyle's law, Kepler's law, Faraday's law, Mendel's law, Pauli's law)? How is the character of our observational research influenced—if at all—by the notation in which we choose to express our questions? How are "the facts" influenced by our mode of expression? What would physics today be like had we adopted Newton's formulation of the differential calculus instead of Leibniz'? Is the uncertainty principle in quantum theory a description? If so, a description of what? Observations? Facts? Limitations in measuring instruments? What? What do we mean by the word "exist" in claims like "A striped coelacanth exists," "Carbon 14 exists," "An 'organism' exists," "An anti-neutrino exists," "A contradiction in his proof exists," "A solution to this problem exists," etc.?

In short, we will here consider certain puzzles about the languages, the observations, the data, and the methods of science for the solving of which you may not before have had the time, or the interest.

A word of caution. For a scientist or science student to expect all this to make any immediate difference in his laboratory work will be to beg for disappointment. Matters of *fact* are not our direct concern—matters of logic, of ideas and reasoning, are. Do not approach our analytical program with unreal expectations. Try, rather, to treat this material as cognate to, but not immediately intimate with, your own experimental work. A scientist's attitude towards his special science may possibly be the better for it. For a good part of science consists in asking questions systematically. Anything that can make one attend more closely to the logical character of scientific questions cannot be amiss.

But what odd chapters these will be: Just a string of questions? Not quite. They will prepare for questions to be worked over in more dialectical contexts elsewhere.

The next chapter, for example, will set out some difficulties inherent in our notions of definition. A definition can do more things in general than we suppose, and less in particular than we sometimes hope. In the third chapter problems connected with measurement will be examined.

These first chapters will thus be quite broad, ranging over a wide assortment of scientific attitudes and concepts. They will be full of questions designed to stir you out of your dogmatic slumbers—or at least to complicate your dreams. Hence, the first part of this work will be framed as a challenge; we may often set out arguments with tongue in cheek (but not, hopefully, with forked tongue in both cheeks). But whether or not I am doing so is for you to decide. These first chapters are thus designed to be targets for your intellectual arrows—salt for your cerebral wounds.

The chapters of the second and third parts will be no less targets for your attack, but our tongue will not be encheeked. The objective there is to worry you, systematically, about concepts like *observation, facts,* experimental *data, hypotheses, theories,* crucial *experiments,* scientific *language, induction* and *deduction,* and a host of closely related topics. These chapters will be calculated not *just* to incite intellectual riot as those in the first part will be. It is hoped that there we will get some insight into the logical foundations of scientific inquiry, that we will locate methodological and philosophical brambles in uncritical views of observation and experiment, and gain a more detailed appreciation of the rules of hypothesis and theory in laboratory research.

Finally, in Part IV, we will turn to consideration of the concepts of probability and probable inference in science, weaving in threads from our earlier discussions as we go.

Let us conclude this first chapter with some logical points. These could be essential. They make all the difference between being clearheaded and being muddleheaded about the languages of science. But even so, take these observations critically; there is more to be said on each of these matters.

Distinguish a *necessary* proposition from a *contingent* proposition. If I say, "Let X be $\sqrt{4}$ and let Y be 4^2," then the proposition "X + Y = 18" is necessary, or necessarily true.[1] It cannot be false. Its denial is self-contradictory. E.g., to say "X + Y ≠ 18" is to say either that X ≠ $\sqrt{4}$, or that Y ≠ 4^2; or both—which contradicts our assumptions. Or, put another way, assigning X the value $\sqrt{4}$ and Y the value 4^2 just

[1] Hanson makes a minor error here. Clearly, *X=±2,* which would mean that *X+Y* could just as well equal 14 as 18. However, nothing about Hanson's point is affected by this mistake—*MDL.*

is, in a way, to assert that X + Y =18. For the meaning of a claim is the entire set of its consequences. Thus part of the *meaning* of "X = $\sqrt{4}$ and Y = 4^2" is necessarily, that X + Y = 18.

A *contingent* proposition, on the other hand, *can* be false. Indeed, the logical possibility of its being false is, perhaps, part of what we mean when we say of some claim that it is contingent, or non-necessary. The proposition "When sucrose is heated with dilute mineral acids it takes up water and is converted into equal parts of glucose and fructose" may be denied without talking nonsense—without, that is, involving one in logical contradiction. A bona fide sample of sucrose may fail to behave in the stated way. This should make us curious, but it need not raise problems about the definitions of words or expressions. And if you would counter, "Oh, but if it does not convert into equal parts of glucose and fructose, then it just isn't sucrose"—if, that is, you make this particular behavior a defining characteristic of sucrose—then you cannot afford to skip the next chapter, where the concept of "definition" will be put under the microscope.

Clearly, most of the propositions within pure mathematics and symbolic logic are necessary (i.e., invulnerable), or analytic (i.e., with inconsistent negations), or true by definition. It is self-contradictory to accept the axioms and rules of a symbol system, a formal game, and then deny what follows from operations on those axioms in accordance with those rules. For this reason, and others, such systems are purely formal, i.e., tell us nothing about the world, are not descriptive of the 3-D arena of experience.

Most of the propositions within natural science, however, are contingent. They are about the world, about matters of fact. They purport to describe "what is the case." There are no ultimate axioms when it comes to matters of fact: No claim is in principle unrevisable. No statement about "the external world" is self-evidently true, necessarily true, true by definition or convention. One can accept that X is a genuine sample of sucrose, heat it with mineral acids, and then consistently report that no inversion from dextro-rotatory to laevo-rotatory optical power was encountered in the resultant solution. He can do this without being accused of talking pure nonsense. He may be accused of other things, but not self-contradiction.

The following propositions are now committed to your tender mercies. What is their logical-conceptual status? (a) "The chemical atomic mass of oxygen is 16." Could this be false? Under what conditions? (b) "Force is that physical quantity which will accelerate a mass—it is equal to the product of the mass and the acceleration." Could this be false? Under what circumstances? (c) "Put a few drops of copper sulphate solution into a test tube and add a few cubic centimeters of strong caustic potash." Is this true or false? Does it even make sense to ask whether it is true or false? Why? (d) "Every event has a cause." True or false? Delineate your notion of an uncaused event. (e) "Fehling's test on glucose is better than Trommer's test." True or false? How so?

Are these propositions necessary, or contingent, or neither? Which of them can be denied meaningfully? Which cannot? And how say you of the mathematically sophisticated propositions of thermodynamics, or of genetic theory? Are they necessary or contingent—or neither or *both* (watch out for this last one!). This