Synthese Library 38
Studies in Epistemology, Logic, Methodology, and Philosophy of Science

Norwood Russell Hanson
Matthew D. Lund *Editor*

What I Do Not Believe, and Other Essays

Second Edition

Springer

Synthese Library

Studies in Epistemology, Logic, Methodology, and Philosophy of Science

Volume 38

The aim of *Synthese Library* is to provide a forum for the best current work in the methodology and philosophy of science and in epistemology. A wide variety of different approaches have traditionally been represented in the Library, and every effort is made to maintain this variety, not for its own sake, but because we believe that there are many fruitful and illuminating approaches to the philosophy of science and related disciplines.

Special attention is paid to methodological studies which illustrate the interplay of empirical and philosophical viewpoints and to contributions to the formal (logical, set-theoretical, mathematical, information-theoretical, decision-theoretical, etc.) methodology of empirical sciences. Likewise, the applications of logical methods to epistemology as well as philosophically and methodologically relevant studies in logic are strongly encouraged. The emphasis on logic will be tempered by interest in the psychological, historical, and sociological aspects of science.

Besides monographs *Synthese Library* publishes thematically unified anthologies and edited volumes with a well-defined topical focus inside the aim and scope of the book series. The contributions in the volumes are expected to be focused and structurally organized in accordance with the central theme(s), and should be tied together by an extensive editorial introduction or set of introductions if the volume is divided into parts. An extensive bibliography and index are mandatory.

More information about this series at http://www.springer.com/series/6607

Norwood Russell Hanson

Matthew D. Lund
Editor

What I Do Not Believe, and Other Essays

Second Edition

First Edition Edited by
Stephen Toulmin
Michigan State University

And

Harry Woolf
Johns Hopkins University

Springer

Norwood Russell Hanson (deceased)

Editor
Matthew D. Lund
Department of Philosophy and Religion Studies
Rowan University
Glassboro, NJ, USA

The first edition of this book was published by D. Reidel Publishing Company, Dordrecht, Holland in 1971 publishing with ISBN-13:978-94-010-3110-3. The editors of the first edition were Stephen Toulmin and Harry Woolf.

Synthese Library
ISBN 978-94-024-1738-8 ISBN 978-94-024-1739-5 (eBook)
https://doi.org/10.1007/978-94-024-1739-5

This Springer imprint is published by the registered company Springer Nature B.V.
The registered company address is: Van Godewijckstraat 30, 3311 GX Dordrecht, The Netherlands

Acknowledgments

First Edition

We thank the publishers of *Philosophy of Science* for permission to reprint the papers 'On Elementary Particle Theory' and 'Stability Proofs and Consistency Proofs: A Loose Analogy'; of *Scientia* for 'The Contributions of Other Disciplines to 19th Century Physics' and 'Mental Events Yet Again'; of *Philosophical Studies* for 'It's Actual, so it's Possible' and 'On the Impossibility of any Future Metaphysics'; of *The Journal of Philosophy* for 'A Budget of Cross-Type Inferences, or Invention is the Mother of Necessity' and 'The Irrelevance of History of Science to Philosophy of Science'. Further, we thank the publishers of the *Delaware Symposium on Philosophy of Science* for permission to reprint the paper 'Some Philosophical Aspects of Contemporary Cosmologies'; of *Isis* for 'Leverrier: The Zenith and Nadir of Newtonian Mechanics'; of *The Review of Metaphysics* for 'On Being in two Places at Once'; of the *Journal of the History of Ideas* for 'Copernicus' Role in Kant's Revolution'; of *Mind* for 'On Having the Same Visual Experiences'; of *Analysis* for 'Imagining the Impossible'; of the *Philosophical Quarterly* for 'Good Inductive Reasons'; of *Dialogue* for 'The Idea of a Logic of Discovery'; of *The American Rationalist* for 'The Agnostic's Dilemma'; and of *Continuum* for 'What I Don't Believe'. We gratefully acknowledge that, for the benefit of the late Professor Hanson's children, all publishers concerned had the kindness to waive the reprint fee.

Stephen Toulmin
Harry Woolf

Second Edition

I thank Harper-Collins for the permission to reprint the short book *Observation and Explanation: A Guide to Philosophy of Science* here as a long chapter. I also thank Amy Jaffe Barzach and the University of Hartford Women's Education and

Leadership Fund for the permission to reprint ‘The Trial of Galileo’. The process of updating the scholarship and style of the original edition of *What I Do Not Believe, and Other Essays* could not have been accomplished without the generous help of William Sheehan (concerning nineteenth-century astronomy) and Bruce Paternoster (concerning ancient philosophy). I would also like to thank the following individuals at Springer who helped shape this project through many unexpected turns and delays: Natalie Rieborn, Ingrid van Laarhoven, Christi Lue, Ties Nijssen, and Stephen O’Reilly. Finally, I wish to thank the entire Hanson family for all their interest and encouragement throughout this process.

Matthew D. Lund

Introductory Note for the First Edition

This collection of essays by Norwood Russell Hanson is one of a pair of volumes being prepared for the Synthese Library from his posthumous papers. The present book comprises two major items which have not previously been published – the opening essay, entitled 'A Picture Theory of Theory-Meaning', and the set of three Harris Lectures on The Theory of Flight originally delivered at Northwestern University, and edited here by the Rev. Prof. Edward MacKinnon S.J., from a verbatim transcript – together with some of Hanson's less readily accessible, or less well-known published papers and articles. The other, companion book will contain a single connected analysis of the historical development of ideas about scientific explanation, as exemplified in theories about planetary motion from the Greeks up to the seventeenth century. (We have provisionally entitled this companion volume *Constellations and Conjectures*: at the time of Hanson's death it had been almost completely re-edited by the author from an earlier manuscript, and it is being prepared for publication by Professor Willard C. Humphreys jr., who is familiar with Hanson's work in this area.)

In making the selection of essays for this present book, we have been guided by two main considerations. In the first place, it is even truer of Russ Hanson than of most other men that *le style, c'était l'homme même*; and we have tried to choose items which are capable of conveying, to people who never knew Hanson the man, something of the individual flavour of his mind and personality. Robust, pugnacious, intolerant of humbug and self-deceit, he was quick to master any of the techniques (or games) of the scholarly and scientific life, but would never allow them to master him in turn. Thus, a few introductory undergraduate courses aside, Hanson's knowledge of theoretical physics was largely self-taught; yet he was soon capable of discussing the philosophical significance and epistemological status of quantum physics or cosmology with a P.A.M. Dirac or a Fred Hoyle – both of them colleagues of his at St. John's College Cambridge, during the 1950s – on a basis of mutual respect. And he could do so, not just in general or abstract terms, but from a familiarity with specific details of the scientific ideas and arguments involved as extensive and penetrating as that possessed by many university professors of physics.

Similarly elsewhere: while quickly making himself at home in the mysteries of symbolic logic or rational mechanics, Hanson was not a man to lose sight of the deeper intellectual issues underlying those formal systems, or to be stampeded into accepting mere techniques as philosophical or scientific panaceas.

The other aspect of Hanson's work which we have tried to illustrate here is his versatility. Unlike those scholars who build a whole career around a single idea, Russ Hanson was an intellectual prodigal, who turned from field to field with a quite uncommon ease and insouciance. Yet he was not just given to piecemeal polemics against targets of opportunity. Re-reading these essays all together, one comes to see how far his excursions into different academic disciplines were made from a consistent standpoint and in the service of a unified philosophical point of view. Whether he is discussing arguments from logic or theology, psychology or astronomy, aerodynamics or philosophy of language, his attitude is the same: *See it like it is* – or, as Bishop Butler put it, "Things are what they are, and their consequences will be what they will be: why then should we seek to be deceived?" And a few key concepts, notably those of *necessity*, *good reasons* and *understanding*, gave a direction to Hanson's arguments in all these different fields. So, in the last resort, we see him rebutting attacks on the Copenhagen Interpretation of quantum mechanics in just the same terms as he uses to explain his own preference for a frank atheism over a tepid agnosticism: *We must stand openly by what there is reason to believe, until there is sufficiently good reason to believe otherwise.*

Finally: we are glad to have the opportunity of including in this collection the only available record of the work that Russ Hanson was doing in his early forties, on the development of ideas in the theory of flight. He himself was, of course, passionately devoted to this own spare-time occupation as a flyer. He never lost his taste for that combination of physical exhilaration and intellectual mastery which is required of a naval pilot and which he first learned when flying from carriers in the Pacific during the second World War; and in his last years his energies were equally divided between his scholarly work, his family and the Grumman Bearcat which he was grooming for an attack on the speed record for piston-engined airplanes. Still, flying was always as much an intellectual as a physical challenge to him, and in this last phase of his work we can see him attempting to build the results of all his hard work on aerodynamics and airfoil design into the same conceptual framework that he had constructed for the rest of his ideas. We must all be grateful to Ed MacKinnon for the great effort and intelligence he has given to the task of preparing a publishable version of the Harris Lectures for inclusion here. At any rate, the ill-adjusted altimeter which (it seems) was responsible for the crash in April 1967 in which Hanson was killed and his Bearcat destroyed did not rob us entirely of the thoughts on which so much of his final years' work was concentrated.

East Lansing, MI, USA — Stephen Toulmin
Baltimore, MD, USA — Harry Woolf
May 1971

Introduction

In 1967, Norwood Russell Hanson was killed in a plane crash. Despite having passed away at the relatively young age of 42, Hanson had already made indelible contributions to the philosophy of science with his work on observation, the interpretation of quantum theory, and the logic of discovery. He had also created the first History and Philosophy of Science (HPS) Department in the United States and made history of science an earnest concern for philosophers. While Hanson is still recognized for his critical work in the philosophy of science, his scholarly versatility is no longer given its proper due. This expanded edition of *What I Do Not Believe, and Other Essays* presents today's reader with Hanson's best work, some of which was much discussed in his lifetime and shortly after and some of which might have been quite influential had Hanson lived long enough to develop it fully.

Hanson's extraordinary range in intellectual matters is sometimes overlooked because of his prodigious talents in nonacademic domains. Hanson was skilled at nearly everything – boxing, playing the trumpet, drawing, shot-putting, and flying airplanes. He also had the forceful personality and daring to pull off things that others would never have thought possible. Hanson's intellectual versatility was, thus, not achieved through an abridgment or stunting of his other interests and capacities. Yet, as is so admirably expressed by Toulmin and Woolf in the Introductory Note, for all Hanson's wide-ranging inquiry, one detects a singularity of perspective and purpose within the vastness of the subject matters he surveys. Since there is so much of Hanson's thought contained in this volume, an introduction that narrowly summarizes each article would be tedious, if not unreadable. Therefore, I will instead discuss most of the main parts by focusing on one or two of the most significant articles in each part.[1]

Hanson's inventive application of the concepts and history of science to general philosophical problems is striking. For instance, in "A Picture Theory of Theory-Meaning," Hanson brought his considerable knowledge of the history and practice of

[1] Since Part VI, The Theory of Flight, is nicely introduced by Edward MacKinnon on pages 331–332, it is not discussed here.

aerodynamics to the questions of how theories originate and how they represent the world. Philosophers of science both before and after Hanson have felt most comfortable in the realm of linguistic representation, and their theories of science have, unsurprisingly, struggled to illuminate other forms of representation. By contrast, Hanson did not confine his analysis to linguistic structures only and instead addresses the epistemic roles played by charts, maps, and curves. In his discussion, Hanson anticipates many of the philosophical points emphasized by Ronald Giere, Hanson's successor at the Indiana University's History and Philosophy of Science Department. It is in this essay where Hanson most explicitly presents his denouement of the thesis of theory-laden observation. Hanson argued that theoretical representation can take many distinct forms (visual and functional analogy, mapping, curves, models, algebraic formulae, diagrams, etc.) and that each of these representations succeeds by sharing the structure of the phenomena. For Hanson, theories and forms of representation are neither wholly "out there" nor "in here." They are "Janus-faced" entities, which point both outward toward the external world and inward to the realm of the mind. Hanson demonstrates not only the richness of different forms of representation but also underscores that our theories cannot be separated from the phenomena. Once a theory promotes our capacity to select out orderly subsets from the overwhelming phenomenological chaos, and these subsets are thereby rendered intelligible, we can no longer experience them at all in any useful sense without the theories.

"A Picture Theory of Theory-Meaning" represents the most successful fusion Hanson ever effected between his professional interests in philosophy of science and his avocational passion for aeronautics. While Hanson indicates that his motivations for the piece come from Wittgenstein's *Tractatus Logico-Philosophicus* and Wisdom's "Logical Constructions," it is clear that his own reflections drawn from aeronautic maps, airfoil designs, and engine parametric and lift charts propel his thought far beyond that of his mentors. Hanson notes repeatedly that picturing is only one way in which theories advance our understanding. At the close of the article, he even explicitly acknowledges that the article might have been better titled the "Structural Representation Theory of What Theories Do." Thus, in this article, one of the last Hanson was to write, he offers a very complete and probing account of how it is that theories represent, and he shows how theory-laden observation is the means through which the new and foreign are made intelligible.

Hanson is best remembered for his thesis that observation is theory-laden, but that specific thesis figures surprisingly little in the pages of this volume. Hanson came increasingly to emphasize that the theory-laden character of observation is merely one consequence of the conceptual and logical layout of science. Since our theories have to make contact with the empirical world somewhere, observations are necessitated – by the conceptual and logical rules of the game of empirical knowledge – to be imbued with theory. Since Hanson saw the theory-ladenness of observation as a consequence of the overarching conceptual structure of science, he was not interested in using the theory-laden observation thesis as a ground for arguing against scientific objectivity or scientific realism, as many other philosophers were inclined to do. Instead, Hanson's primary interest lay in discerning the types of good reasoning that take place within the realm of empirical fact. Philosophers,

he thought, had been so enamored of deductive logic that they either translated all empirical reasoning into a deductive mold or ignored empirical reasoning altogether. Either way, traditional philosophy not only leaves empirical reasoning unilluminated but obscured. In showing how different forms of representation facilitate the creation of new theories, Hanson revisits his earlier analysis of those cognitive processes that allow us to see anomalous gestalts suddenly in terms of familiar conceptual arrangements. Such cognitive processes are very significant contributors to our empirical reasoning, and Hanson makes a strong case that it is not only impossible to cleanse their influence from our account of nature but that the attempt to do so robs us of the capacity to understand how science advances. Once we learn how to "read" all these representations – and this is a matter that is far from trivial – we are able to understand parts of the world that were previously slippery and amorphous. The chaotic and strange has coalesced into something stable, predictable, and well-formed. Again, Hanson's distaste for the idea of clamping an interpretation onto raw data emerges forcefully: what the very data are, and which other items of would-be data fade impotently into the irrelevant background, is mediated and settled by the structures through which the data are made intelligible.

Hanson's account has two advantages over the main treatments of theories in philosophy of science. First, Hanson does not suppose that theories are just intellectual posits spawned by scientific whimsy; if that were so, we would be set upon by such a multitude of theories, each with an equal claim to our consideration, that serious testing and development would never be able to get underway. Hanson points out that the creation of a structure of representation capable of rendering the phenomena intelligible is a difficult undertaking, one requiring knowledge, patience, luck, and creativity. He urged that philosophers pay more attention to how such creations were produced and that their concern should be not with "theory-using, but with theory-finding" ([1958] 2010, 3). Second, on Hanson's account, structures of representation are the product of a temporally extended process – there is a beginning, middle, and end to it. Because of this, it is possible to analyze the creation of a theory in stepwise fashion. Since the creation of these structures is therefore an object of study (in principle at least), study of historical processes of theory construction may provide lessons for how theory creation should be pursued. Hanson addressed this theme in his many articles on the logic of discovery.

Hanson's are the eyes of the perennial outsider. Once he masters the new conceptual terrain of cosmology, logic, religion, or the theory of flight, Hanson then extracts the logical commitments implicit in that domain and subjects them to rigorous scrutiny. Hanson's reflections and arguments never exactly emulate those of the true expert in a field, though they often stimulate new paths of thought and speculation that had eluded even the brightest minds. A nice case in point of this thesis is Hanson's discussion of rival cosmological theories in "Some Philosophical Aspects of Contemporary Cosmologies." Though Hanson was a colleague of Fred Hoyle at St. John's College at Cambridge, and the two had many friendly exchanges, Hanson does not provide an advertisement for Hoyle's cosmology. What he does do is show that many of Hoyle's assumptions and motivations are not only naïve but perhaps absurd; however, Hanson then shows the same to be true of the orientation of the

Big Bang theorists, and he makes a convincing case that the rhetoric of each theory renders the other implausible from the get go. Hanson's discussion, however, is not intended to disparage cosmology as a nonscientific pursuit, a mere philosophical idle. Instead, he wants to clarify the far-reaching consequences of what are, initially at least, some rather innocent-looking conceptual assumptions. For instance, are we to interpret the principle of the conservation of energy as applying only to the part of the universe that is observable to us (as Hoyle does) or as applying to the whole of the universe, including areas forever outside of our observational reach (as the Big Bangers do)? In the end, neither theory seems capable of being wholly right or wholly wrong. The lesson to be drawn is that the empirical future of cosmology must ever remain in close contact with its deeply philosophical past.

Throughout his unfortunately short career, Hanson found himself pulled in multiple directions. Often his thought started with a defense of a specific position, to be followed by a slightly modified defense, and another still, until his last position was reached, which could not in truth be said to have been an extension of the original one. While this observation applies to Hanson's work generally, his writings on logic and levels of discourse fit this pattern most dramatically. Early on, Hanson argued that necessary and contingent truths occupy separate logical spaces and that inferential commerce between the two is always fallacious and therefore to be avoided. However, as Hanson proceeded through his typology of logical types, he came to recognize that just as there are different grades of possibility – a position philosophers have long held – so too are there different grades of necessity.[2] According to Hanson, while logical necessities have inconsistent negations, conceptual necessities have unintelligible negations. Hanson argued that many apparent paradoxes in philosophy were due to superimposing the terms and methods of deductive logic onto conceptual problems, the problem of induction being the most illustrious of these manufactured difficulties. On Hanson's account, inductive inference surely cannot be deductively justified (as Hume implied was necessary), but it cannot be dispensed with without making the empirical world unintelligible. Hanson was intent on defining enough of the critical concepts within these non-deductive logical realms to produce the appropriate logics, but he was never able to create a variant system – due either to the lack of time or (more likely) to the difficulty if not impossibility of the task. Instead, Hanson started with a bold, interesting position and then slowly modified it until he was left with an inventory, budget, or anatomy of the new field along with a few directives regarding how to approach it.

Hanson had decried the neglect of Leverrier's story, with all of its importance for both the history of celestial mechanics and scientific methodology, in his first book, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science.* He himself finally wrote the history of Leverrier ("Leverrier: The Zenith and Nadir of Newtonian Mechanics"), and it is one of the finest exemplars of Hanson's work as a historian. This is not to say that the essay lacks philosophical interest. Hanson is as

[2] For logical reasons, distinct grades of necessity must accompany distinct types of possibility. Necessities are just alternative ways of stating impossibilities.

keen as ever to trace out the patterns of hypothesis generation, but his careful engagement with the fascinating details of the discovery of Neptune (complete with an explanation for why Leverrier's work was more significant than that of John Couch Adams, who is ordinarily considered to have been a co-discoverer) display his skill as a historian. Moreover, the even more fascinating "observations" of Vulcan, and other supposed intra-Mercurial planetary objects, make for very entertaining reading. While the contemporary reader may feel the need to suppress a scornful smile at reading of the "planet" Vulcan, Hanson shows that some very rigorous and diverse theoretical approaches were directed at the problem caused by Mercury's classically recalcitrant orbit. For the most part, the Vulcan hunters were not quacks but talented scientists, attempting to work out a thorny problem as best their theoretical and observational resources would allow. Finally, this article bears some striking thematic affinities with another work that spun off the press in 1962: Thomas Kuhn's *The Structure of Scientific Revolutions.* Hanson points out that Newtonianism entered a state of crisis, to use Kuhn's term, after the failure of the Vulcan conjecture: "Even confidence in the lawgiver Sir Isaac, declined somewhat" (140, this volume); also, when the Vulcan hypothesis first came into vogue, scores of observers began noticing intra-Mercurial bodies where they hadn't seen them before.

I will now turn to two of the most influential areas of Hanson's thought – the relation between philosophy of science and history of science and his advocacy for a logic of discovery. Definitive essays on these topics ("The Irrelevance of History of Science to Philosophy of Science" and "The Idea of a Logic of Discovery") appear in Part IV: Logic of the present volume.[3]

The first essay is largely the fruit of Hanson's institution building. In creating a space for history and philosophy of science, it was necessary to define the regions of overlap and difference for the two disciplines. Hanson had always believed that history and philosophy of science mutually enrich one another. His title for the article made it sound as though history is not important for philosophy of science; after all, to say it is "irrelevant" entails that it is unimportant. Or so it would seem. Let me explain Hanson's specific conception of relevance. Hanson identified philosophy of science with the logic of science; thus, the job of the logician of science is to appraise the logical character of arguments that crop up in the history of science. Logicians, of course, assess the validity of arguments – whether such arguments have true premises is an extralogical matter. Hanson's piece presents his interpretation of the maxim that "philosophy of science without history of science is empty; history of science without philosophy of science is blind."[4] According to

[3] Hanson certainly would have placed these articles in the Logic section – during his years at Indiana University (1958–1963), Hanson preferred the expression logic of science to philosophy of science. However, the contemporary reader would likely place them in Part I: Philosophy of Science.

[4] This maxim is usually attributed to Imre Lakatos. Though Hanson was the first to discuss the maxim in print, he gave credit to Lakatos as the original source of it. Herbert Feigl also discussed the maxim in isolation from both Hanson and Lakatos. For more details on the history and use of this maxim, see Lund (2010, 136–137).

Hanson, even though history and philosophy of science are not *logically* related, they are very tightly interconnected. Philosophy of science needs to select its content from the history of science to ensure that it is actually about real, and not pretended or fantastical, science. Historians of science must consult logical canons to appraise the arguments used by historical scientists. Historians don't just chronicle facts; instead, they order their narratives around the significant concepts and arguments that led to critical changes in the history of science. Without an appreciation for what makes the history go, history of science is blind.

While this famous article certainly formulated the outlook of Hanson's History and Logic of Science Department at Indiana University, it did not represent Hanson's last word on the issue. This article was published in 1962, near the midpoint of Hanson's abbreviated career, and Hanson came later to question his earlier position on the genetic fallacy. One commits the genetic fallacy when one assumes that the provenance of a proposition or argument is relevant to its truth or validity. The early Hanson argued that anyone who supposed that the goodness of an argument was a function of the argument's conditions of origin committed the genetic fallacy. Later, Hanson came to believe that, in special circumstances, the structure of an object is explainable, at least in part, by its history; in other words, he came to believe that some genetic arguments may not be fallacious. Such an insight would have led to a less differentiated view of HPS, but Hanson's efforts toward such a synthesis were mere gropings.

The logic of discovery was a central concern for Hanson throughout his career. Initially, he had argued that there can be good reasons for suggesting a hypothesis in the first place and these reasons need not be identical to the reasons for acceptance. As time went by, Hanson drifted away from the strong thesis that there can be a logical method for conjuring up worthwhile hypotheses toward the position that there exists a logical analysis of hypothesis plausibility. This is another area where Hanson seemed to have had strong intuitions running counter to those of mainstream philosophy of science, but where he ended up sticking, to a surprising degree, with the orthodox position. One reason for Hanson's adherence to orthodoxy was an apparent confusion about the relation between the psychological and logical – Hanson assumed that the two forms of analysis were entirely distinct, though he elsewhere argued (especially concerning the relation between history and philosophy of science) that different forms of analysis can apply to the same subject matter. While Hanson's basic position remained that there exists a logical appraisal of untested hypotheses, the examples he gives of strategies used in discovery – arguments from analogy, simplicity, aesthetic elegance, and explanatory fertility – all seem too remote from deductive logic to be analyzed down into anything commanding the respect of philosophers. Perhaps, Hanson would have been better off extending his accounts of good inductive reasons and his exploration of cross-type inference to have shed some light on the inference patterns so often active in discovery. Hanson's conceptual arsenal was rich enough to mount such an attack, but he seemed resigned to progressively limit his notion of the logic of discovery. Perhaps in this area more than any other might, we have expected the mercurial Hanson to have changed his mind once more and to have offered up an argument for a stronger notion of the logic of discovery had he lived.

We see in the essays on religious belief (Part V) Hanson's propensity to speak his mind. For all of his outspoken unbelief on the subject of religion, Hanson enjoyed many friendships with devout believers and was not one to shy away from discussing such higher matters. The longer of the two essays, "What I Don't Believe," was solicited by Hanson's friend Edward MacKinnon, at that time a Catholic priest. The piece was meant to be the first installment in an exchange between Hanson and MacKinnon concerning the rationality of religious belief, but Hanson was killed as MacKinnon's private response was making its way through the mail.

Even on the subject of religion, seemingly far removed from philosophy of science, we can find significant traces of Hanson's thought on science. In fact, though it may pass by unnoticed upon first reading these essays on religion, they encapsulate much of Hanson's mature philosophy of science. If we regard theistic existential claims as factual claims, as Hanson hurriedly argues we must, then we must treat the claims in accord with their logical type, whether we are confirming or disconfirming them. Despite the wonderful reputation for neutrality the agnostic enjoys, the agnostic is actually guilty of some logical double dealing. When considering the factual claim that God exists, the agnostic cannot confirm the claim due to the lack of evidence in its favor. As Hanson vividly portrays in both essays, he knows exactly what kind of evidence would convince him of the existence of God – the sky could open up, and the "Michelangeloid" God could show Himself, letting it be known how little He cares for Hanson's theological quibbling into the bargain. Since phenomena of this type, and others less dramatic, have not been observed, Hanson believes there is no evidence to support the claim that God exists.

Since there is no evidence for God's existence, the case against God ought to be closed. However, the agnostic just won't listen to reason. Instead of considering "God exists" as being disconfirmed in the same way that "the Loch Ness Monster exists" is disconfirmed (viz., by the absolute paucity of confirmatory evidence), the agnostic shifts ground and claims that no evidence – or, better put, lack of evidence – could ever disconfirm God's existence. This is not fair dealing: if the claim is regarded as confirmable, then it must also be disconfirmable. If the evidence does not support the existence of x, then the evidence disconfirms x.

Judgments of the quality of Hanson's writing on religious belief no doubt hinge on the reader's religious convictions. Even if one doesn't like Hanson's beliefs, it is hard to criticize his general strategy of moving the discussion from the otherworldly back to the shared commitments of scientific inquiry. The focus on the nature of religious belief and the logic of evidence appears to be a fruitful path, both toward a mutual understanding of one another's world views and toward exorcizing the pernicious subjectivity that hides behind the idea that differences in religious belief always come down to a *Weltanschauungskampf*, a battle between eternally incommensurable worldviews.

In this expanded edition of *What I Do Not Believe, and Other Essays*, we have been able to include two additional pieces. The first is Hanson's enigmatic essay "Observation and Explanation: A Guide to Philosophy of Science," which was published as a free standing book by Harper and Row. Sadly, practically nothing is known about when this short piece was composed or what its relation was to Hanson's

other posthumously published textbook in philosophy of science, *Perception and Discovery.* The work does, however, seem to have been composed near the end of Hanson's life and expresses his mature philosophy of science. In it, he counters popular objections to his earlier published views, though in his usual indirect way. Here, as elsewhere in Hanson's work of the 1960s, we find a muted impatience with the "vogue" status of the theory-laden observation thesis. For Hanson, theory-laden observation marked just one aspect of the conceptual structure of science – a *significant* aspect, to be sure, but one whose full significance could only be appraised after studying it alongside the other concepts at the epistemological core of science. For Hanson, science represents a concerted attempt to render the world intelligible, and the various concepts central to that attempt are interdependent and cannot operate, or even be fully understood, independently. Concepts like fact, discovery, explanation, and cause are just as weighty as the concept of observation, though their perplexities are not as dramatically revealed as those of observation.

In "Observation and Explanation," Hanson calls for moderation and argues for a *via media* between the extremes of "dustbowl" empiricism and formalism. In this essay, Hanson launches once more his own distinctive philosophy, this time not portrayed as an overdue philosophical analysis of science as in *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* but as the only way past seductive false philosophies of science. At the same time, though, the study of the extreme positions is indispensable for finding the elusive middle course. The essay's style is fresh and engaging, and it is rife with Hanson's aphoristic brevity. One well-acquainted with Hanson's work might accuse him of passing off his own philosophy as *the* philosophy of science; however, Hanson is probably no more guilty of that crime than were Hempel, Nagel, or other writers of introductory books in philosophy of science. It is fairer to Hanson to emphasize the substantial effort he had put into the philosophical education of science students. From the beginning of his career at Cambridge through his years at Indiana, Hanson acted as a philosophical ambassador to science, and this essay, along with *Perception and Discovery*, represented his final contributions to the pedagogy of philosophy of science.

The second new addition, Hanson's essay "The Trial of Galileo," is something of a "lost" work – it was published in a small run by the now defunct Hartford College for Women and was left out of the published lists of Hanson's works. The published version of the essay was put together by Stephen Toulmin from an audio recording of the lecture. Toulmin knew Hanson's literary style so well that the published version is indistinguishable from one of Hanson's self-edited works. Hanson's lecture was one of the six sponsored by the Hartford College for Women on the theme of trials where justice and the law came into conflict. What better topic to exemplify the decaying regard for authoritative institutions in the latter half of the 1960s? This setting for the lecture explains great deal about the essay's goals and its direct, and somewhat didactic, style. Hanson, ever the loud and pugnacious advocate for freedom of inquiry and expression, saw much in Galileo's story that reflected the problems of Cold War America.

Hanson's closeness with some of the best Galileo scholars of his day is evident in the piece, as Hanson expertly lays out the rich medieval ferment in physical and

theological thought that set the stage for Galileo. Hanson shows how the courageous Galileo, with his unexampled powers of debate and irrepressibly sharp tongue, was bound to clash with small men and the Mother Church that emboldened them. Hanson himself was clearly able to see many of his own struggles reflected in the mighty travails of Galileo. Hanson was a vituperous advocate for freedom of thought, speech, and religion; his firebranding certainly earned him some recognition, not all of which was positive. Like Galileo, Hanson was something of a member of the Catholic Church's loyal opposition. Galileo, of course, remained a devout Catholic all his life, but he sought to moderate its dogmatic position on natural philosophical inquiry; Galileo was concerned not just for the future of natural philosophy (science) but for the Church itself, regarding it a tragic outcome should the new knowledge not issue from Catholic soil. Hanson, though baptized and raised Catholic, "converted" to atheism in adulthood. Nonetheless, he retained a great deal of respect for the Catholic intellectual tradition and especially loved the musical and artistic expressions of Catholicism. In short, Hanson was charmed enough by Catholicism to feel the profound tensions that must have animated Galileo in his fateful struggle. Hanson, ever the polemicist, ends the piece with a warning about the unchanging weakness of human nature and the necessity for those who respect truth to resist dogmatism and institutionalized thinking.

Even as we reach the 50th anniversary of Hanson's death, it is impossible to read the essays in this volume without feeling remorse at how much was lost in the plane crash that took his life – so much talent, humor, boldness, passion, and humanity and so many more intellectual vistas to have been taken in. Hanson's remark about Galileo that "intellectual gadflies are rarely stationary" (164) applies to himself as well – sadly, some intellectual gadflies don't live long either.

[A note on the text: since Hanson's career was split between the English and American academic worlds, his works were published in both British and American styles. The styles of the original publications have been retained to reflect their places of origin.]

Department of Philosophy and Religion Studies Matthew D. Lund
Rowan University
Glassboro, NJ, USA

References

Hanson, Norwood Russell. [1958] 2010. *Patterns of discovery: An inquiry into the conceptual foundations of science.* Cambridge: Cambridge University Press.
Lund, Matthew D. 2010. *N.R. Hanson: Observation, discovery, and scientific change*. Amherst: Humanity Books.

Contents

Part I
Philosophy of Science

Chapter 1
A Picture Theory of Theory-Meaning

> His (Kepler's) admirable method of thinking consisted in forming in his mind a diagrammatic or outline representation of the entangled state of things before him, omitting all that was accidental, observing suggestive relations between the parts of his diagram, performing divers experiments upon it, or upon the natural objects, and noting the results. –C.S. Peirce, *Values in a Universe of Chance*

Perplexities concerning Scientific Theories persist because the usual 'singled valued' philosophical analyses cannot do justice to the problematic features of so complex a semantical entity. The components of theories are like law statements, and like models and hypotheses, being conceptual entities which are used in a variety of ways – not all of these being always compatible with the others. Thus many physicists characterize the classical laws of motion, as if they functioned in a definitional way.[1] But sometimes these laws seem remarkably empirical.[2] Others characterize such laws as 'conventional'; they shape entire disciplines much as the rules shape the game of chess.[3] Law statements are not exclusively any one of these – definitions, factual claims or conventions. They are *all* these things.

Consider: "The sun rises in the east". It is impossible from only hearing or seeing these words in isolation to know whether this claim is functioning in a definitional way or in a descriptive way. Thus if tomorrow the sun parts the horizon 90° from where it arose this morning, it might still be rising in the east *if* one treats "east" as the *name* of that place where the sun rises (wherever that may be). If one defines "east" in the terms of celestial coordinates though, it will be an empirical/factual/synthetic claim that the sun rises in the east. So the very meaning of "The sun rises in the east" is elusive until one comprehends this assertion's local use in a specific context. This latter is quite free to change.

Much this same diversity and flexibility should mark our understanding of scientific theories. What a scientific theory *is* cannot be finally determined – for theories

[1] Kolin, and sometimes Poincaré, for example.

[2] Mach and Broad frame the Second Law as fundamentally a factual statement based on experience.

[3] Reichenbach, and Poincaré again, are cases in point.

N. R. Hanson, *What I Do Not Believe, and Other Essays*, Synthese Library 38,
https://doi.org/10.1007/978-94-024-1739-5_1

are context-dependent instruments of conceptualization. Tomorrow's enquiries can transform yesterday's scientific theories into semantical structures different from what today's philosophers pronounce them to be. (Who now reads Mach for insights into contemporary Quantum Theory? or even Schrödinger?)

Let us look at theories in a way different from those which dominate discussions in philosophy of science. Think of theories not as ideal deductive systems, as precise languages, or as convenient empirical shorthands. That is, they are used sometimes as if they were definitional/analytical/calculational systems; sometimes as if they were ideal languages (well-chiseled logicians' Esperanto); sometimes as if they were elegant compendia of factual information. Theories are all these things – but they are more too. Explore yet another facet of scientific theories – one which disappears in the glare of the analytical spotlight.

How can theories enable us to *understand* a subject matter? What is the difference between a heap, or a list, of descriptive assertions and a theory –which is itself largely constituted of those same descriptions? These questions recall the contrast between a mere generalization (e.g., that all white, blue-eyed, tom cats are deaf), as against a law of nature (e.g., that all bird's wings have a convex top-side). If the generalization is imagined refuted, we are required only to effect a quantitative readjustment; we may have to say that 99% of all white, male, blue-eyed cats are deaf, rather than all of them. We will still know what cats are, however. No conceptual readjustment is forced on us by a feline counterinstance. With a law of nature, such as that wings of birds have convex top-sides – if one were to encounter a counterinstance of this, *conceptual* difficulties would ensue at once. The full concept of bird *flight* requires a wing imagined so shaped. Faced (*per impossibile)* with a bird wing curved otherwise, one might come to doubt what a bird wing *is,* and what role it plays in flight – doubts which do *not* now punctuate the thinking of aerodynamicists and ornithologists. It is as if one imagined an exception to: *all unsupported bodies in terrestrial space move toward the center of the earth*. An exception to this would have to be a body in a state of levitation or 'negative gravity', either of which possibilities raises doubts as to what *bodies* were in the first place.

It is sometimes said that a Law of Nature explains its subject matter, helps us to understand it, makes it more intelligible and comprehensible -as against a generalization which only correlates observables via actuarial techniques; these observables may concern 'unrelata' like the simultaneous occurrence of sun spots and wheat failures, where no conceptual link binds such phenomena. Analogously, a scientific theory entices philosophers because it somehow explains its subject matter; it helps us understand 'interconceptions' between phenomena.

What does all this mean? What is it in a theory such that before it was formulated all the data, the descriptions, the initial conditions – however accurately recorded – did not compose into a coherent and intelligible subject matter, whereas after the theory has been generated and coupled with observations one can *comprehend* the subject matter?

Consider theories *pro tem* as conceptual entities located at the crossroads between epistemology and philosophical psychology. Think no more, for now, of the logical and the semantical aspects of scientific theories; everyone always talks about that.

Fig. 1.1

Fig. 1.2

Let us view theories as instruments of intelligibility. Ask with me: "How does the conceptual structure of a theory make understanding possible?"

Reflect on those picture-puzzles, dear to learning theorists and Gestalt psychologists. The sheep-in-the-tree (Fig. 1.1), and the figure on page 14 of *Patterns of Discovery* (Hanson [1958] 2010) (reproduced here as Fig. 1.2).

These constellations of lines cohere dramatically when once it is signaled what they are. The cluster of dots and blobs and shapes set out just above (in Fig. 1.2) can be seen as a medieval Christ-like representation. Often this appears as an unintelligible chaos of patches, and lines – before it constitutes a picture of any significance. How is it that a conceptual structure, a pattern in imagination, can give meaning to gaggles of dots, shapes, lines and points? How is scientific observation possible?

Consider Fig. 1.3: When labelled 'a Mexican on a bicycle (seen from above)' something happens within the perceptual field. The experience now is qualitatively different from what it had been before when this was a mere configuration of lines. How so?

How doesn't matter (the problem is philosophical, not psychological; conceptual, not factual). *That* patterns affect the significance of lines, dots, shapes, and patches – which might have been in perceptual turbulence otherwise – this is our fundamental datum. It has profound epistemological consequences. Knowledge is

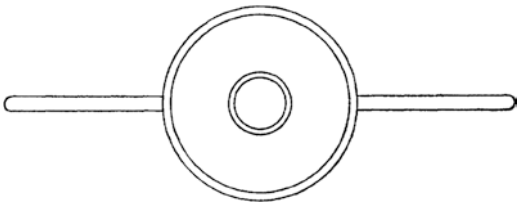

Fig. 1.3

a function of how our experiences cohere. Observations made *before* the perceptual pattern is appreciated, are epistemically distinct from the observations, (and their descriptions), made *after* that pattern has cast them into intelligible constellations – although the observations and descriptions, those before and those after, might be 'congruent'.[4] The descriptive terms, the assertions, the observations themselves, when considered in terms of repeatability and what is 'written on the page', these might be identical both before and after the pattern is appreciated. The lines in the drawing above did not shift geometrically when the caption was assigned. Yet there is an epistemic distinction between the earlier and later encounters, a distinction of deep importance.

Clearly, talk about *patterns* differs in type from talk of lines, shapes and dots. Patterns do not fill the same logical space as do the lines and dots being patterned: a pattern – e.g. of this Mexican atop a bicycle – is not itself detectable or visible or drawable, not as the shapes and the lines are. This is not to say that they are not detectable or visible at all. How else should we come to know them? Patterns are detectable and can be made visible to those who cannot see them – but not necessarily by adding more lines. Describing this encounter differs from speaking of objects of sensation as appreciated by all normal observers. 20-20 vision is no guarantee of seeing the Mexican on the bicycle. Patterns are not *elements* in an epistemic configuration. Rather, the pattern is the configuration itself. By analogy, the plot of a novel is not another cluster of words; the form of a sonata is not just another cluster of notes; the planform design of a building is not merely more bricks and beams; the aerodynamic structure of an aircraft wing – its airfoil section – isn't just more ribs and skin plates; indeed, the meaning of a proposition isn't only another articulated term!

Much as the level of 'pattern talk' differs conceptually from that on which talk of dots, shapes, lines and patches obtains – so also theoretical talk differs conceptually from observational and descriptive talk. The more comprehensive suggestion is this: that just as perceptual pattern recognition at once gives significance to elements perceived and yet differs from any perception of dots, shapes and lines – so also *conceptual* pattern recognition at once gives significance to the observational elements within a theory and yet differs from any awareness of those elements *vis-à-vis* their primary relationship to events and objects. The ways in which theories, conceptual structures, are meaningful with respect to the observation statements is qualitatively a different type of concern from that involved in discussions of how observation statements are meaningful with respect to things.

[4] The temporal references, 'before' and 'after' are inessential. This exposition would not suffer were 'independently of' and 'dependent upon' introjected.

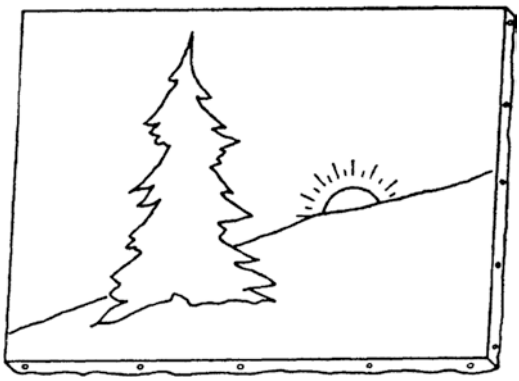

Fig. 1.4

At this juncture, some parenthetic autobiography. A psychological fact: there are moments when I find myself confronting a cluster of symbols, or observed anomalies, such that after having come to view these through the appropriate *scientific theory* they configure, cohere and collapse into meaningful patterns within a unified intellectual experience. This, to me, seems not unrelated to what is involved when I appreciate dots and lines in a qualitatively different way after having mastered the perceptual pattern structuring those marks.[5] Consider Boyle's law as understood in 1662, then simply a stack of statistical correlations; Boyle didn't extract that famous generalization himself, his followers did. That law – that correlation considered *before* the advent of kinetic theory and before classical statistical mechanics – resembles the dots without the pattern, the observations without the theory, the descriptions without the explanations. Boyle's Law began life as the *merest* correlation. It explained nothing. Only when general gas theory and the kinetic hypothesis caught up with it, did Boyle's generalization come to function as Laws of Nature are reputed to do. Bracket with this example the historical problem concerning the anomalous motions of Saturn and Jupiter. This was a descriptive thorn in the side of astronomical explanation, B.L. ('Before Laplace'). Laplace undertook to set out a conceptual framework for mechanical ideas, a Stability Proof in terms of which this anomaly – the apparently secular aberrations in the motions of Saturn and Jupiter – could be regarded as but local irregularities in what was really a 900 year cycle – a periodic, repetitive 'aberration'. It is a little like what one should expect in a microcinematographic film of meshing gears in a fine clock: crude and lopsided in fine scale, but precise and perfectly periodic at the macrochronometric level. Descriptions of Saturn and Jupiter B.L. were independent, unrelated and unsynchronized, whereas these same descriptions A.L. constituted almost different subjects for one's attention.

Please permit me to spell out this primitive analogy in more detail. Consider the concept of a *scene.* More specifically, think of a dawn seen from a hillside. There sits a landscape painter, busily conveying to his canvas a configuration like Figs. 1.4 and 1.5. Some passersby may say of this painting that it is 'true to life' (Fig. 1.4), that it captures what is significant 'out there' (Fig. 1.5). Painting is an activity of the

[5] Cf. the earlier illustrations.

Fig. 1.5

appropriate type *to* capture features of the original – the tree, the hill and the other landscape objects 'out there' and 'committable to' canvas. There is a structural identity between what can be seen by the painter from the hillside and what can be seen on the canvas he has painted. And this is just as important for his painting's being 'true to life' as is the identity at the color – shape – line level. *Of course* the tree should be painted green, as it is, and not pink, or silver. But no less important is it that it should be depicted as to the left of the sun – and not stretched horizontally above it. Something, which I shall designate 'the scene', is 'out there' for inspection; one can stand on the hillside and survey the scene to the east.

One can also describe what the artist has put on canvas as 'the scene he has painted'. The scene on his canvas and the scene 'out there' are structurally so related that it is meaningful to speak of the former as constituting a replication of the latter, something one cannot claim of sounds, textures or tastes, no ingenious combination of which can replicate the scene at dawn; the scene-as-paintable eludes the powers of music, of tactile sensation and even of cookery. Thus the term 'scene', from a conceptual point of view, is specific yet Janus-faced. It alludes to an objective subject matter 'out there', and it also refers to one's plastic representation of that subject matter.[6] The *same* scene can be both 'out there' and also on canvas.

That the artist has put the same scene on canvas as obtains 'out there' is pertinent to whether his rendition is veridical.

I don't want to refer to the scene *per se* as if it were an 'interim designatum'. That would proliferate entities, since Antiquity a philosophically suspect practice. Nonetheless, aspects of subject matters are reproducible in this way *because* of their possible structural identity with aspects of the reproduction – this is all I wish to remind you of.

[6]Yet the designation is specific in that it excludes myriad other kinds of representations of the world. The real steak's possession of properties which can induce gustatory delight in me is no part of any *scene* of that steak. The nightingale's song is replicable, but not because it is part of a scene. The scrape I endure may be due to the icy, rough surface of the granite I clamber upon, but the scrape is not a replication of any part of the granite, whereas my visual memory of the granite may indeed have properties of the granite block itself – such as those an artist could commit to canvas in a painting of that block.

Fig. 1.6

Many terms do similar work; 'landscape' has the same mirrorlike semantical quality. The landscape is something tended by a gardener, something one can view from a distance. It is also what is capturable on canvas by a draftsman, or painter. Again, the subject matter and its representations can share something of considerable conceptual importance. Were this not so, the subject matter would not be representable at all.

There are myriad such 'bipartite' terms. The 'plan-form' of a bird's wing, as referred to by ornithologists and aerodynamicists, makes reference to such geometrical relationships as the chord-span ratio, the angular sweep back of the leading edge, the relative root-to-tip rate of narrowing, and the contour shape of the wing (elliptical?, rectangular?, triangular?) The wing's 'aspect ratio' is another such term – this is the relative thickness of the 'fuselage' as against the length of the wing, as viewed directly forwards or aft. The tip configuration of the wing, whether blunt or pointed or round, will also be part of the understood designatum of 'plan-form' (Fig. 1.6). The plan-form and the aspect ratio of a bird or an air craft can be drawn out on a piece of drafting paper, *and* it can also be inspected in the 3-D wing itself, as found on the living bird, or the operational aircraft. Wittgenstein's point about the structure of the bird's song as being something which is *in* the song itself, and also *in* the gramophone recording of the song – and also *in* the musical score which captures that song in notes (*à la* Delius) – this point is close to what I am groping for. The song, its recording and its score – share a common structure. The plan-form on paper and in the actual bird wing share a common structure, as is true also of the tip-configuration, the aspect ratio, the dihedral, etc. The landscape, the scene, *is* the common structure shared by objects-in-configuration 'out there', and color-patches-in-configuration on the painter's canvas. [Aside: *Facts* are the common structure shared by events 'out there' (as when they are 'hard', 'stubborn' and must 'be faced') and by the truth as stated about those events (as when we 'state the facts', 'list' them and base theories on them).]

My suggestion will be that, analogously, states of affairs, that is, constellations of phenomena, are often rendered understandable and intelligible and comprehensible *because* some objective, structural component of those phenomena is duplicated in a corresponding structural component within some scientific theory. Scientifically understanding phenomena *x*, *y* and *z* consists in perceiving what *kinds* of phenomena they are – how they relate each to the other within some larger epistemic context, how they are dependent upon, or interfere with, each other. Insights into such relations 'out there' are generable within our perceptions of the structures of theories; these theoretical structures function *vis-à-vis* our linguistic references to *x*, *y* and *z* in a way analogous to how the *scene* stands to the tree-and-hill 'out there', and also to the painted patches on canvas. Thus, in contrast to the delineation of theories

as 'ideal languages' or 'Euclidean hypothetico-deductive structures', I suggest that the important function of scientific theory is to provide structural representations of phenomena – such that to have understood how the elements in the theoretical representation 'hang together' is to have discovered *a* way in which the elements of the original phenomena *might* 'hang together'. In short, scientific theories do not always *argue* us into the truth; they do not always demonstrate deductively and forcefully what is the case. Often they *show* what could be the case with perplexing phenomena, by relating representations of those phenomena in ways which are themselves possible representations of relationships obtaining 'out there'. Theories provide patterns for ordering phenomena. This, just as much as they provide inference-channels through which to argue towards descriptions of phenomena.

Before proceeding, consider some classical objections to the so-called 'picture theory of meaning'. Clearly, if one takes all forms of representation to be fundamentally *iconic*, as one would in a landscape painting, then the painter will be felt to represent elements in the original 3-D configuration by way of *iconic* tokens in the copy configuration (2-D). That is, his tree here will share some properties of the tree out there, (perspectivally considered). Its shape, for example, oriented with respect to the sun, and the hill, will display ratios in relative height, width, and color, analogous to what obtains in the original. *The* sun and *his* sun will have a common geometry both internal, with respect to its discoid design and coherence, and external, with respect to its relations to tree and hill. A color transparency, e.g. of the Kodak variety, could be moved from its superposition on the scene out there, to superposition of the scene on canvas, and it would be logically possible for there to be shape-congruence and 'color-congruence' all the way through, both in superposition I and in superposition II. And so that representation on canvas will stand to the original (3-D) in a way which is designated as "iconic". This is proved by the Kodak transparency's congruence with each.

Now, *vis-à-vis* scientific theories, where the mode of representation (if there is one) is linguistic and descriptive, it is obvious that this is not any crudely iconic representation. Theories are not simple pictures. The word "tree" has nothing iconically in common with what this word may designate, namely some actual tree. (There is nothing arboreal about "tree"!) Similarly the word "sun" is not *iconically* connected with any perceptual object or any physical object. Words represent not because of property-sharing. They have no property in common with what they represent – save for onomatopoetics like "toot", "crash", "smooth" and "short". [These seem to me relatively unimportant, semantically; they certainly constitute no paradigm of word-object meaning]. It will be the *conventional correlation* of words with objects which holds our attention here. Consider a term well-known in analytical mechanics – *"syzygy"*. This word does not represent iconically any rectilinear configuration of moon, earth and sun (which is what the word means). It is not due to any iconic relationship with objects in the Solar system that this linguistic term means what it does – although you will perceive that there is something about these designations ('*y*', *'y'*, '*y*') which seems to tie in with the three bodied problem involved; sun, earth and moon. Nonetheless, *'syzygy'* is related to planets as a painting may be related to trees. To hear it for the first time, is not to know (simply from

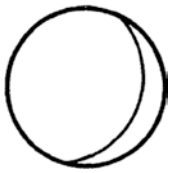

Fig. 1.7

the configuration of the sounds and symbols) that it connects semantically with moon, earth and sun – in the way in which "toot" might connect semantically with a passing train, or "buzz" with a passing saw. In other words, statements, paradigmatically, designate; then they characterize their designata as being of this or that type, or as having these or those properties. Thus, "The moon is a pocked sphere" – where "The moon" is the designation of an astronomical object, and "is a pocked sphere" characterizes that object, that designatum. Pictures represent in a non-designatory way; they are non-specific with respect to the attention-directing they may stimulate. Does Fig. 1.7 designate the moon, its sphericity, its discoidity, its pock-marks, its yellow color… or what? Statements place one's attention precisely on particular designata, and then they discriminate between, and select from the appropriate alternative characterizations of that designatum. Thus, of all the things that it may be true to say of the moon as depicted above – e.g. that it is spherical, that it appears as discoid, that it is pock-marked,… etc., – the statement "The moon is a pocked sphere" selects one of these specific data as its unique and direct message, and articulates it pointedly. That is why it is true that one picture is worth a thousand words; a picture is a thousand times less specific than a short sharp statement. But, by the same token one word is worth a thousand pictures; a statement can supply a focus for the attention quite different in type from anything generable *via* confrontation with a picture.

These objections to the picture theory are well-known, and yet I am going to suggest something sometimes suggested by others – that all this critical carping on the distinctions between originals and icons, as against originals and statements, really misses the profound point of the picture theory of meaning. Objections concerning the non-iconic ways in which words and statements represent, these really deal with the hyper-fine structure of discourse *versus* pictorial representation. These are directed to the ways in which words like "moon" are, or are not, correctable in function with line configurations such as shown in Fig. 1.8. Aside from such hyperfine structural differences, statements and drawings remain deeply analogous *vis-à-vis* representational features to be discussed in a moment. Thus the objections to the picture theory advanced by such people as Edna Daitz and Irving Copi concern just the minute superficialities of word tokens and claim tokens. What else could be the point of noting that "cat" does not look feline and that "moon" sheds no light?

However, let us attend rather to the structure of discursive knowledge in more general terms, and not restrict our interest to the indivisible tokens through which that structure is conveyed. Consider the structure of discourse itself, and the corresponding structure of representational knowledge. These different kinds of struc-