Theory construction and selection in modern physics: the S matrix

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THE *S* MATRIX

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TO NHC

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Preface

A major, overarching cluster of problems central to the philosophy of science and certainly underlying much of the debate in the recent literature is how scientific theories are constructed, how they are judged or selected, and what type of knowledge they give us. There are two aspects of answers to any of these three questions: what has actually occurred according to the historical record and what is the rational status of each of these activities or of the knowledge produced. A simple schema, that is based on induction and the hypothetical-deductive method and that provides answers to the above queries, is the sequence: observation, hypothesis, prediction, confirmation. This model or picture of science has a long tradition. We can see its roots already in Bacon's (1620 (1960, pp. 43–4 and 98-100)) advocating a slow and careful ascent from particulars to generalities (Aphorisms, Bk. I, XIV, XXII, CIII–CVII). He urged use of a combination of induction and deduction in arriving at knowledge. In Bacon's ladder of axiom, one is to make modest generalizations based on specific observations and data, check these modest theories by comparing their predictions with facts once again, then combine these generalizations into more general ones, check their predictions against observations, and in this way carefully proceed to the most general axioms, theories or laws. Whewell* (1857, Vol. I, p. 146) speaks of the epochs of induction, development, verification, application and extension. This is often taken as the hallmark of the scientific method that results in truth, true knowledge or true theories about the world. While the proverbial 'man

* At the end of this book there are both a glossary of technical terms (used in physics and in philosophy) and a list of key figures, along with their major positions. Since not *every* reader will need *all* of this information repeated, I have not put it into the text proper, where it might interfere with the flow of the narrative. If in doubt, check!

in the street' may subscribe to this representation of science, as many working scientists seem still to do in general outline, few philosophers of science would accept so simplistic a response. It would be nice if the world were as simple as this, but such is not the case. Let us refer to this as the *simple model* of science. Although we shall expand at length in the text proper on several developments of this model in the philosophy of science, let us sketch here the evolution of the position we shall follow.

The logical positivists sought to elaborate and formalize this model by attempting to base an explanation of the rationality of science upon its empirical foundation and even to develop a logic of induction. The program did not succeed, both because its foundationist assumptions led to a description of science (or, actually, what science should be like) that just did not accord with actual historical scientific practice and because of internal problems that have become evident in retrospect (Friedman, 1988). One of the difficulties with a reliance on the straightforward inductive-hypothetical-deductive method is the so-called Duhem–Quine thesis according to which any theory is underdetermined by the empirical facts upon which it is based and which serve to confirm the theory. A reaction to this is instrumentalism which sees the goal of science as constructing laws and theories that provide a means for calculating and correlating empirical results, but which need not give us a true picture (at the level of theoretical entities) of the world. Furthermore, another difficulty for any fixed truth claims made on behalf of science is that the historical record of the development of science provides ample evidence of laws, concepts and theories, once held to be true, that have later been abandoned as false at a foundational level (e.g., classical mechanics being replaced by relativity and quantum theory).

To cope with these and other shortcomings of the simple model, philosophers of science have come to view the functioning scientific enterprise in terms of a three-level scheme: practice (theory), methods and goals. Theories function to explain, correlate and organize phenomena. The heading 'practice' also includes experiments and the activity of judging the empirical adequacy of a proposed theory. For many scientists, and perhaps for most people in general, this may seem to be just about the whole of science. However, the question remains of *how* theories are to be evaluated. What standards or rules are to be applied in testing and selecting theories? This is the level of method. Some examples of such criteria would be predictive accuracy, simplicity, coherence and fertility. Finally, there is the metalevel of the goals or aims of science (such as giving true explanations of phenomena versus merely providing rules that allow us to calculate and predict without necessarily providing literally true pictures of the world; or, a goal for science could be the control of nature). A fallback position from the simple model is to admit the obvious corrigibility of our theories (i.e., they *do* change and develop), but to hold out for stability or 'fixedness' at the (meta) levels of methods and of goals in order to underpin an invariant rationality that characterizes science and the knowledge it obtains. Even here though, a concession is usually made that the goal of science is approximate truth about nature (in some sense of a correspondence with reality). It is also typical to make a distinction between the *discovery* of a scientific theory and the *justification* of an already formulated theory. Postpostivist philosophy of science tends to bracket the problem of the means by which scientific theories and hypotheses are discovered or constructed (leaving these to some other area such as psychology, luck, inspired guess, etc.) and to concentrate on the justification of an articulated theory (whatever its origin). The rationality of science is to be located in the logic of justification of its theories. It is to this aspect of the scientific enterprise that the three-tiered schema of practice, methods, goals is to apply. But, one has now to decide the status of these allegedly fixed methods and goals (that are to underpin the *rationality* of science). Are these to be argued for and justified on the basis of some logically necessary first principles (the foundationist approach) or on the basis of (contingent) historical fact? Examples of methodologies of the first type, the foundationist or rationalist school, are those of Popper (1963), Lakatos (1970, 1976) and Watkins (1984), while the best-known proponent of the historicist school is Kuhn (1970).

To set the scene between the opposing views, let us outline, as representative of each view, the positions of Kuhn and of Lakatos. Kuhn (1970) sees two essential components in science. Normal science, which is the activity the majority of scientists engage in most of the time, is guided by paradigms and consists largely in puzzle-solving within a fairly well-articulated set of ground rules. This phase of science defines problems to be solved. The pressure, rather than being on the theories themselves, is on the individual scientist to apply successfully the currently accepted paradigm in solving a problem that has been set (Kuhn 1970; pp. 4–5). In a crisis situation, the paradigm (or 'rules of the game') become loosened. While normal science recognizes anomalies and crises, it cannot, of itself, change the paradigm. It is then that a transition to revolutionary science takes place. By such revolutions, new paradigms are generated and science advances (or evolves). A successful revolution is followed by another period of normal science.

For Lakatos (1970, 1976), the proper unit for theory appraisal is the research program, an entity that establishes a tradition within which scientists work. This methodology of scientific research programs (MSRP) is intended to be a development of Popper's view of science according to which *the* hallmark of scientific theories is that they are (in principle) refutable or falsifiable. That is, bold hypotheses are exposed to the hazard of refutation (by observation and experiment) and the successful ones survive (for a longer or shorter time). For Lakatos, the process of refutation alone is not sufficient to represent actual science, but research programs are seen as essential for comprehending scientific practice. In Lakatos' theory of science, the various components of a research program are (i) the 'hard core' of assumptions which are kept unfalsifiable by methodological decision, (ii) the auxiliary hypotheses (or assumptions) to which the falsifiability criterion is directed when anomalies arise, and the (iii) the (positive) heuristic, a set of suggestions for modifying the auxiliary hypotheses. Roughly speaking, the negative heuristic (or hard core) tells one what *not* to do in the sense that certain assumptions are to be left largely untouched. The positive heuristic (often referred to hereafter simply as the heuristic) is a partially articulated research policy to guide one through all the possibilities allowed (or not forbidden) by the negative heuristic. The positive heuristic provides a direction for research and the evolution of a program. If MSRP is correct, then applications of the heuristic to specific problems should generate a sequence of theories by which a research program develops. These changes should then be classifiable as degenerating or progressive problem shifts, respectively, in terms of their *ad hocness* (or contrived nature) to meet anomaly or their fertility for further research and confirmation by experiment. Popper and Lakatos, like others in the rationalist school, see science as having its own distinctive *internal* logic by which it tests and selects theories. This exercise of theory justification is seen as (logically, even if not always temporally) distinct from the means by which theories are discovered, conjectured or constructed.

However, it has been demonstrated by case studies that discovery and justification are *not* disjoint enterprises. Galison (1983b) has shown that this distinction is not meaningful in modern experimental high-energy physics. We provide further examples of such blurring in the present book. The more closely one looks at the historical record of science, the more difficult it is to find *the* hallmark of science (valid for all science in all ages). As we shall see later, the writings of Fine (1984, 1986), Laudan (1984a), Nickles (1987), Nersessian (1984) and Shapere (1984) are particularly important in this regard. Their work has shown a trend toward the naturalization of the philosophy of science (i.e., to base work upon the actual historical record of science and to stress the use of the methods of science in studying the scientific enterprise itself). An important element introduced into the discussion has been the influence of factors (e.g., social ones) *external* to science proper upon the form and content of science, a point already made by Kuhn (1970), but more recently emphasized by Bloor (1976) and by Pickering (1984). These sociologists of knowledge have stressed important and previously undervalued (by philosophers of science) factors of scientific practice, but they go too far when they suggest that such factors account for *all* of science. Shapere (1986) has argued that the demarcation between internal and external factors is not static, but that science internalizes once-external factors as it develops and as it finds it useful to do so. A concurrent debate, which has paralleled and been interwoven with the evolution of views on methodology, is that of realism versus anti-realism in the philosophy of science. This is basically an argument over the degree of uniqueness or the tightness of constraint on theories and their worldviews as provided by empirical results. There is, of course, a whole spectrum of views on realism. Fine (1986) has recently argued that this is not really too fruitful a debate.

The purpose of the present monograph is to use an extensive and detailed case study of a research program in modern theoretical physics to examine how theories are constructed, selected and justified in actual scientific practice. The book is intended both for philosophers of science and for interested physicists. Since the text mixes physics and philosophy, each group will probably find parts of it difficult. Hence, there is an extensive glossary of terms and a list of the main players collected at the end of the book so as not to clutter up the narrative text too much (because 'half' of the intended audience will already be familiar with 'half' the material, and vice versa). My claim is that the origin of methodologically interesting ideas and questions, at least in modern physics, lies in the (highly) *technical* details of practice. For that reason, I present the details on key developments for those who can and wish to follow them. Extended instances of this are set off in smaller type (to allow the more casual reader to pass over them easily). Each chapter begins with an extensive discussion of the historical relevance, to the overall program, of the detailed developments to be presented. There are verbal, nontechnical summaries of such material in the text proper and at the end of the chapters. The entire case study is summarized early in Chapter 9. The first eight chapters are devoted to the case study itself, with philosophically relevant comments interspersed throughout. The concluding Chapter 10 assesses the import of the case study and of other episodes in modern physics for these basic philosophical issues, illustrates several themes from 'naturalized' philosophy of science and suggests a useful framework within which to view scientific practice. These last two chapters can be taken on their own, provided one is willing to accept my summaries of the details of the case study of the previous chapters. It is essential to realize that these conclusions are based mainly on *one* case study so that no claim to universality would be warranted. My final position is not a wholly skeptical one that claims we have not learned anything, but rather one that asks how much and what part of that is peculiar to *science*. If science is to have any universal characteristics (as some claim), then it is fair to examine a field (e.g., modern theoretical physics) that is commonly acknowledged to be scientific to see whether it conforms to this general characterization. It is true that the case study proper (Chapters 1 through 8) and the general discussion of methodology (Chapter 10) form two separate parts, but ones which are, I claim, connected in an important way. A detailed case study of a 'failed' research program (*S*-matrix theory) is perhaps of relatively little interest unless it is connected to some larger philosophical or methodological issues. The analysis of Chapter 10 is based on history – including not just the direction in which things finally went ('good' science), but also a consideration of how things *might* (consistently) have gone a very different way and why they did not.

Perhaps it is of some relevance to mention that when, as a practicing scientist, I became interested in the philosophy of science some ten years or so ago, I had no particular methodological ax to grind. Rather naively I attempted to apply some currently fashionable methodologies from the philosophy of science to an area of high-energy physics I was familiar with. Things didn't mesh too well and I have been brought to a rather skeptical position, as is evident from the tenor of this book. Being an autodidact in this business, I am certain the following pages contain many statements that philosophers of science will find outrageous. I hope that some of the material may be useful, nevertheless.

I wish to thank all of those scientists who contributed their recollections and comments to this case study. The list is too large to reproduce here. They are acknowledged in appropriate footnotes. Several colleagues in the philosophy of science have been supportive of my work over the last several years, most notably Professor Ernan McMullin of the University of Notre Dame. Professor J. W. N. Watkins of the London School of Economics has welcomed me as an Academic Visitor and Professor M. L. G. Redhead of Cambridge University extended his hospitality to me as a Visiting Scholar. The appointment as a Visiting Fellow at St. Edmund's College, Cambridge, furnished an atmosphere conducive to writing a complete draft of the manuscript. The National Science Foundation, through its Program in the History and Philosophy of Science, provided partial support for several years while the research for this work was being done (grants Nos. SES-8318884, SES-8606472 and SES-8705469).[1] Ernan McMullin, John Polkinghorne and Michael Redhead contributed useful comments and criticisms on a late draft of the manuscript. Over the years my colleague, Professor Gerald L. Jones, has provided trenchant and commonsense observations to keep me in touch with the real world of physics. These acknowledgments are not meant to imply that the people cited necessarily agree with my conclusions or that they are in any way responsible for the errors that remain. The comments and criticisms of an anonymous referee were especially helpful for the last revisions prior to publication. Neal Nash generously offered to redraw the figures for the text. Finally, Susan Varnak has done a remarkable job of coping with an typing several successive versions of a difficult manuscript.

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American Journal of Physics: Cushing (1986b) (Section 10.2).
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