CONSTRUCTING QUARKS

A SOCIOLOGICAL HISTORY OF PARTICLE PHYSICS

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PREFACE

Etched into the history of twentieth-century physics are cycles of atomism. First came atomic physics, then nuclear physics and finally elementary-particle physics. Each was thought of as a journey deeper into the fine structure of matter. In the beginning was the nuclear atom. The early years of this century saw a growing conviction amongst scientists that the chemical elements were made up of atoms, each of which comprised a small, positively charged core or nucleus surrounded by a diffuse cloud of negatively charged particles known as electrons. Atomic physics was the study of the outer layer of the atom, the electron cloud. Nuclear physics concentrated in turn upon the nucleus, which was itself regarded as composite: the atomic nucleus of each chemical element was supposed to be made up of a fixed number of positively charged particles called protons plus a number of electrically neutral neutrons. Protons, neutrons and electrons were the original 'elementary particles' – the building blocks from which physicists considered that all forms of matter were constructed. However, in the post-World War II period, many other particles were discovered which appeared to be just as elementary as the proton, neutron and electron, and a new specialty devoted to their study grew up within physics. The new specialty was known either as elementary-particle physics, after its subject matter, or as high-energy physics (HEP), after its primary experimental tool – the high-energy particle accelerator.

Atomic physics, nuclear physics and even the early years of elementary-particle physics have all been subject to historical scrutiny, but the story of the latest cycle of atomism has yet to be told. Not content with regarding protons, neutrons and the like as truly elementary particles, in the 1960s and 1970s high-energy physicists became increasingly confident that they had plumbed a new stratum of matter: *quarks*. Gross matter was composed of atoms; within the atom was the nucleus; within the nucleus were protons and neutrons; and, finally, within protons and neutrons (and all sorts of other particles) were quarks. The aim of this book is to document and analyse this latest step into the heart of the material world.

The analysis given here is somewhat unconventional in that its thrust is sociological. Rather than treat the quark view of matter as an abstract conceptual system, I seek to analyse its construction, elaboration and use in the context of the developing practice of the HEP community. My explanation of why the reality of quarks came to be accepted relates to the dynamics of that practice, a dynamics which is at once social and conceptual. I try to avoid the circular idiom of naive realism whereby the product of a historical process, in this case the perceived reality of quarks, is held to determine the process itself. The view taken here is that the reality of quarks was the upshot of particle physicists' practice, and not the reverse: hence the title of the book, *Constructing Quarks*.

The sociology of science is often taken to relate purely to the social relations between scientists, and hence to exclude esoteric technical and conceptual matters. That is not the case here. There is no escape from such topics because the emphasis is on practice and, in HEP, practice is irredeemably esoteric. This, of course, creates a communication problem: arcane practices are best described in arcane language, otherwise known as scientific jargon. To ameliorate the problem, I have sought to explain each item of HEP jargon whenever it first enters the text. I have also cited popular accounts of the developments at issue, for the reader who feels he would benefit from more background on particular cases. In this way I have tried to make the text accessible to anyone with a basic scientific education. It is perhaps appropriate to add that the main object of my account is not to explain technical matters *per se* but to explain how knowledge is developed and transformed in the course of scientific practice. My belief is that the processes of transformation are easier to grasp than the full ramifications of, say, a given theoretical structure at a particular time. My hope is to give the reader some feeling for what scientists do and how science develops, not to equip him or her as a particle physicist.

There remain, nevertheless, sections of the account which may prove difficult for outsiders to physics. I have in mind here especially the passages in Part II of the account which deal with the formal development of 'gauge theory'. Gauge theory provided the theoretical framework within which quark physics was eventually set, and the development of gauge theory was intrinsic to the establishment of the quark picture. It would therefore be inappropriate to omit these passages from a historical account. However, the key idea which I use in analysing the development of theories is that of modelling or analogy and, unfortunately in the present context, gauge theory was modelled upon a highly complex, mathematically-sophisticated theory known as quantum electrodynamics (QED). QED was taken for granted by physicists during the period we will be considering, and it is beyond the scope of this work to go at all deeply into its origins. Thus, in discussing the formal development of gauge theory, I have to refer to accepted properties of QED (and quantum field theory in general) which I can only partially explain. This is the origin of the communication gap which remains in the text. On the positive side, I should stress that the more difficult phases of the narrative are non-cumulative in effect. For example, the uses to which particle physicists put gauge theory, discussed in Part III of the account, are much more easily understood than the prior process of theory development, discussed in Part II. The conceptual intricacies of the early discussions of field theory and gauge theory need not, therefore, deter the reader from reading on.

It remains for me to acknowledge some of my many debts: intellectual, material and financial. Taking the latter first, the research on which this book is based has been supported by grants from the UK Social Science Research Council. Without their support, it would have been impossible. The active co-operation of the HEP community was likewise crucial, and I would like to thank the many physicists who found the time for interview and correspondence. In the following pages I argue that research is a social activity, and I am happy to apply the same argument to my own work. The account of particle physics offered here is intended as a contribution to the 'relativist-constructivist' programme in the sociology of scientific knowledge, and I owe a considerable debt to all of those working in this tradition. I am particularly grateful to Harry Collins, Trevor Pinch, Dave Travis and John Law. The Science Studies Unit of the University of Edinburgh has been my base for this work; it has supported me in many ways and I am indebted to everyone there, especially Mike Barfoot, Barry Barnes, David Edge, Bill Harvey, Malcolm Nicolson and Steve Shapin. Thanks also to Peter Higgs and the other members of the Edinburgh HEP group for much information and many useful discussions. Moyra Forrest prepared the Index, and her assistance with library materials has also been invaluable. The typing of Carole Tansley, Margaret Merchant and, especially, Jane Flaxington has been heroic. To Jane F. likewise my thanks for life-support while this book was being written.

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1

INTRODUCTION

The scientist . . . must appear to the systematic epistemologist as an unscrupulous opportunist. ALBERT EINSTEIN[1]

The historian of modern science has to come to terms with the fact that the scientists have got there first. Very many accounts of the topics to be examined here have already been presented by particle physicists in the popular scientific press, as well as in the professional literature of high-energy physics (HEP).[2] These accounts all have a similar form – they are contributions to a well-established genre of scientific writing – and present a vision of science which is in some ways the mirror image – or reverse – of that developed in the following pages. I want therefore to begin by sketching out the archetypal 'scientist's account' of the history of HEP, in order both to indicate its shortcomings and to introduce the motivations behind my own approach.[3] This sketch will involve the use of some technical terms which will only be explained in later chapters, but the detailed meanings of the terms are not important in the present context.

The scientist's account begins in the early 1960s. At that time, particle physicists recognised four fundamental forces of nature, known, in order of decreasing strength, as the strong, electromagnetic, weak and gravitational interactions. The strong force, responsible for the binding of neutrons and protons in nuclei, was of short range and was the dominant force in elementary-particle interactions. The electromagnetic force, around 10^3 (i.e. 1000) times weaker than the strong force and acting at long range, was responsible for binding nuclei and electrons together in atoms, and also for macroscopic electromagnetic phenomena: light, radio waves and so on. The weak force, of short range and around 10^5 (100,000) times weaker than the strong force, had, except in special circumstances, negligible effects. Those special circumstances pertained to certain radioactive decays of nuclei and elementary particles, and to processes of energy generation in stars. Finally the gravitational force was a long-range force like electromagnetism. It was responsible for macroscopic gravitational phenomena – apples falling from trees, the earth orbiting the sun – but it was also 10^{38} times weaker than the strong force, and its effects were considered to be completely negligible in the world of elementary particles.

Associated with this classification of forces was a classification of elementary particles. Particles which experienced the strong force were called *hadrons*. There were many hadrons, including the proton and neutron, the constituents of atomic nuclei. Particles which were immune to the strong force – the electron and a handful of other particles – were known as *leptons*. The picture began to change in 1964, with the proposal that the constituents had constituents: hadrons were to be seen as made up of more fundamental entities known as *quarks*. Although it left many questions unanswered, the quark model explained certain experimentally-observed regularities of the hadron mass spectrum and of hadronic decay processes. Moreover, in the late 1960s and early 1970s it was seen that quarks could explain the phenomenon known as *scaling*, which had recently been discovered in experiments on the interaction of leptons with hadrons. In the scientist's account quarks thus represented the fundamental entities of a new layer of matter. Initially, though, the existence of quarks was not regarded as firmly established, principally because experimental searches had failed to detect any particles having the distinctive properties postulated for them (i.e. their having fractional electric charges). Leptons were not subject to a parallel ontological transformation; unlike hadrons they continued to be regarded as truly elementary particles.

Early in the 1970s, new theories of the interactions of quarks and leptons began to be formulated. First came the realisation that the weak and electromagnetic interactions could be seen as manifestations of a single *electroweak* force within the context of a theoretical approach known as gauge theory. This unification, reminiscent of Maxwell's nineteenth-century unification of electricity and magnetism, carried with it the prediction of the existence of the *weak neutral current*, which was verified in 1973, and of *charmed particles*, which was verified in 1974. Meanwhile, it had been realised in 1973 that a particular gauge theory, known as *quantum chromodynamics* or QCD, was a possible theory of the strong interactions of quarks. It was found first to explain scaling, and later to explain observed deviations from scaling. It explained certain interesting properties of charmed and other particles, and various other hadronic phenomena. Therefore QCD became the accepted theory of the strong interactions. Quarks had still not been observed in isolation. But both electroweak theory and QCD assumed the validity of the quark picture, and thus the existence of quarks was established simultaneously with the establishment of the gauge-theory description of their interactions. In the late 1970s, particle physicists were agreed that the world of elementary particles was one of quarks and leptons interacting according to the dictates of the twin gauge theories: electroweak theory and QCD. Finally it was noticed that since the unified electroweak theory and QCD were both gauge theories, they could, in their turn, be unified with one another. This last unification brought with it more fascinating predictions, which began to arouse the interests of experimenters in 1979. These predictions were not immediately verified, but many physicists were confident that they would be. Thus, not only was a new and fundamental layer of structure discovered, in the shape of quarks, but three forces previously thought to be quite different from one another – the strong, electromagnetic and weak interactions – stood revealed as but particular manifestations of a single force.

Apart from brevity, this sketch of the scientist's account of the history of quarks has the virtue that it names key developments. For example, it indicates that the quark idea was only one theoretical component in the developments we will discuss. The other component was gauge theory, which eventually provided the framework within which the interactions of quarks and leptons were understood. This is something to keep firmly in mind: it is impossible to understand the establishment of the quark picture without at the same time understanding the perceived virtues of gauge theory. The scientist's account also points to the role of new phenomena – scaling, neutral currents, charmed particles and the like – in supporting the quark–gauge theory view. The observation that the quark–gauge theory picture referred to new phenomena, quite different from those which supported the pre-quark view of the world, will also figure prominently in the following account. However, beyond specifying the key developments in HEP, the scientist's account goes on, either explicitly or implicitly, to specify a relationship between them. I want now to discuss this relationship, in order to highlight where the present approach departs from that of the scientist.

In the scientist's account, experiment is seen as the supreme arbiter of theory. Experimental facts dictate which theories are to be accepted and which rejected. Experimental data on scaling, neutral currents and charmed particles, for example, dictated that the quark–gauge theory picture was to be preferred over alternative descriptions of the world. There are, though, two well-known and forceful philosophical objections to this view, each of which implies that experiment cannot *oblige* scientists to make a particular choice of theories.[4] First, even if one were to accept that experiment produces unequivocal fact, it would remain the case that choice of a theory is underdetermined by any finite set of data. It is always possible to invent an unlimited set of theories, each one capable of explaining a given set of facts. Of course, many of these theories may seem implausible, but to speak of plausibility is to point to a role for scientific *judgment*: the relative plausibility of competing theories cannot be seen as residing in data which are equally well explained by all of them. Such judgments are intrinsic to theory choice, and clearly entail something more than a straightforward comparison of predictions with data. Furthermore, whilst one could in principle imagine that a given theory might be in perfect agreement with all of the relevant facts, historically this seems never to be the case. There are always misfits between theoretical predictions and contemporary experimental data. Again judgments are inevitable: which theories merit elaboration in the face of apparent empirical falsification, and which do not?

The second objection to the scientist's version is that the idea that experiment produces unequivocal fact is deeply problematic. At the heart of the scientist's version is the image of experimental apparatus as a 'closed', perfectly well understood system. Just because the apparatus is closed in this sense, whatever data it produces must command universal assent; if everyone agrees upon how an experiment works and that it has been competently performed, there is no way in which its findings can be disputed. However, it appears that this is not an adequate image of actual experiments. They are better regarded as being performed upon 'open', imperfectly understood systems, and therefore experimental reports are *fallible*. This fallibility arises in two ways. First, scientists' understanding of any experiment is dependent upon theories of how the apparatus performs, and if these theories change then so will the data produced. More far reaching than this, though, is the observation that experimental reports necessarily rest upon incomplete foundations. To give a relevant example, one can note that much of the effort which goes into the performance and interpretation of HEP experiments is devoted to minimising 'background' – physical processes which are uninteresting in themselves, but which can mimic the phenomenon under investigation. Experimenters do their best, of course, to eliminate all possible sources of background, but it is a commonplace of experimental science that this process has to stop somewhere if results are ever to be presented. Again a *judgment* is required, that *enough* has been done by the experimenters to make it probable that background effects cannot explain the reported signal, and such judgments can always, in principle, be called into question. The determined critic can always concoct some possible, if improbable, source of error which has not been ruled out by the experimenters.[5]

Missing from the scientist's account, then, is any apparent reference to the judgments entailed in the production of scientific knowledge – judgments relating to the acceptability of experimental data as facts about natural phenomena, and judgments relating to the plausibility of theories. But this lack is only apparent. The scientist's account avoids any explicit reference to judgments by *retrospectively adjudicating upon their validity*. By this I mean the following. Theoretical entities like quarks, and conceptualisations of natural phenomena like the weak neutral current, are in the first instance *theoretical constructs*: they appear as terms in theories elaborated by scientists. However, scientists typically make the realist identification of these constructs with the contents of nature, and then use this identification retrospectively to legitimate and make unproblematic existing scientific judgments. Thus, for example, the experiments which discovered the weak neutral current are now represented in the scientist's account as closed systems just because the neutral current is seen to be real. Conversely, other observation reports which were once taken to imply the non-existence of the neutral current are now represented as being erroneous: clearly, if one accepts the reality of the neutral current, this must be the case. Similarly, by interpreting quarks and so on as real entities, the choice of quark models and gauge theories is made to seem unproblematic: if quarks really are the fundamental building blocks of the world, why should anyone want to explore alternative theories?

Most scientists think of it as their purpose to explore the underlying structure of material reality, and it therefore seems quite reasonable for them to view their history in this way.[6] But from the perspective of the historian the realist idiom is considerably less attractive. Its most serious shortcoming is that it is retrospective. One can only appeal to the reality of theoretical constructs to legitimate scientific judgments when one has already decided *which* constructs are real. And consensus over the reality of particular constructs is the outcome of a historical process. Thus, if one is interested in the nature of the process itself rather than in simply its conclusion, recourse to the reality of natural phenomena and theoretical entities is self-defeating.

How is one to escape from retrospection in analysing the history of science? To answer this question, it is useful to reformulate the objection to the scientist's account in terms of the location of *agency* in science. In the scientist's account, scientists do not appear as genuine agents. Scientists are represented rather as passive observers of nature: the facts of natural reality are revealed through experiment; the experimenter's duty is simply to report what he sees; the theorist accepts such reports and supplies apparently unproblematic explanations of them. One gets little feeling that scientists actually *do* anything in their day-to-day practice. Inasmuch as agency appears anywhere in the scientist's account it is ascribed to natural phenomena which, by manifesting themselves through the medium of experiment, somehow direct the evolution of science. Seen in this light, there is something odd about the scientist's account. The attribution of agency to inanimate matter rather than to human actors is not a routinely acceptable notion. In this book, the view will be that agency belongs to actors not phenomena: scientists make their own history, they are not the passive mouthpieces of nature. This perspective has two advantages for the historian. First, while it may be the scientist's job to discover the structure of nature, it is certainly not the historian's. The historian deals in texts, which give him access not to natural reality but to the actions of scientists – scientific practice.[7] The historian's methods are appropriate to the exploration of what scientists were *doing* at a given time, but will never lead him to a quark or a neutral current. And, by paying attention to texts as indicators of contemporary scientific practice, the historian can escape from the retrospective idiom of the scientist. He can, in this way, attempt to understand the process of scientific development, and the judgments entailed in it, in contemporary rather than retrospective terms – but only, of course, if he distances himself from the realist identification of theoretical constructs with the contents of nature.[8]

This is where the mirror symmetry arises between the scientist's account and that offered here. The scientist legitimates scientific judgments by reference to the state of nature; I attempt to understand them by reference to the cultural context in which they are made. I put scientific practice, which is accessible to the historian's methods, at the centre of my account, rather than the putative but inaccessible reality of theoretical constructs. My goal is to interpret the historical development of particle physics, including the pattern of scientific judgments entailed in it, in terms of the dynamics of research practice. To explain how I seek to accomplish this, I will sketch out here some of the salient features of the development of HEP, and describe the framework I adopt for their analysis.[9]

The establishment of the quark–gauge theory view of elementary particles did not take place in a single leap. As we shall see, it was a product of the founding and growth of a whole constellation of experimental and theoretical research traditions structured around the exploration and explanation of a circumscribed range of natural phenomena. Traditions within this constellation drew upon different aspects of the quark–gauge theory picture of elementary particles, and, as they grew during the late 1960s and 1970s, they eventually displaced traditions which drew upon alternative images of physical reality. Seen from this perspective, the problem of understanding the establishment of quarks and gauge theory in the practice of the HEP community is equivalent to that of understanding the dynamics of research traditions. To see what is involved here, consider an idealised discovery process.

Suppose that a group of experimenters sets out to investigate some facet of a phenomenon whose existence is taken by the scientific community to be well established. Suppose, further, that when the experimenters analyse their data they find that their results do not conform to prior expectations. They are then faced with one of the problems of scientific judgment noted above, that of the potential fallibility of all experiments. Have they discovered something new about the world or is something amiss with their performance or interpretation of the experiment? From an examination of the details of the experiment alone, it is impossible to answer this question. However thorough the experimenters have been, the possibility of undetected error remains.[10] Now suppose that a theorist enters the scene. He declares that the experimenters' findings are not unexpected to him – they are the manifestation of some novel phenomenon which has a central position in his latest theory. This creates a new set of options for research practice. First, by identifying the unexpected findings with an attribute of nature rather than with the possible inadequacy of a particular experiment, it points the way forward for further experimental investigation. And secondly, since the new phenomenon is conceptualised within a theoretical framework, the field is open for theorists to elaborate further the original proposal.

One can imagine a variety of sequels to this episode, but it is sufficient to outline two extreme cases. Suppose that a second generation of experiments is performed, aimed at further exploration of the new phenomenon, and that they find no trace of it. In this case, one would expect that suspicion will again fall upon the performance of the so-called discovery experiment, and that the theorist's conjectures will once more be seen as pure theory with little or no empirical support. Conversely, suppose that the second-generation experiments do find traces which conform in some degree with expectations deriving from the new theory. In this case, one would expect scientific realism to begin to take over. The new phenomenon