THE ROLE OF INTERESTS IN HIGH-ENERGY PHYSICS:

The Choice Between Charm And Colour*

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Introduction

The process through which scientists choose between competing theories has long been of interest to analysts of science, particularly philosophers, but has yet to receive as much detailed attention as it deserves from sociologists. This paper is intended as a contribution towards remedying that deficiency.

For the sociologist, an interesting way to look at scientific theory-choice is in terms of the reality of scientific concepts, objects and their relations. The problem is one of understanding how, for instance, the reality of objects in the natural world — their 'out-thereness' — can be seen as the outcome of a social process. The aim of this paper is to sketch a simple model of the social construction of 'out-thereness', and to illustrate how the model can be applied to a particularly significant episode in the history of theoretical high-energy physics (HEP).

The episode in question concerns the discovery, and theoretical interpretation, of a number of very unusual elementary particles, in the period from 1974 to 1976. A variety of theoretical models of these particles were proposed during this period — foremost amongst them the models known as 'charm' and 'colour' — and an analysis will be given of the process whereby the charm model became established and colour rejected. In this introductory section I want to explain the general form which the analysis will take; the following sections will deal with its application to the example chosen.

Professional discourse amongst theoretical physicists centres on ways of looking at the world — theories, models, idealisations of data, mathematical

techniques and so on — and the problem of understanding the accomplishment of 'out-thereness' is equivalent to that of understanding the ways in which the components of such (often small-scale) 'world views' become entrenched in the practice of the community of theorists. In order to discuss this latter problem, some image is required of the social and cognitive structure and dynamics of scientific work, and such a schematic image — which one might call the 'interest model' — will now be discussed.

The interest model has two key concepts: that of an 'exemplar' and, of course, that of an 'interest'. Let us take 'exemplars' first. Kuhn first introduced the term in the 'Postscript' to his *Structure of Scientific Revolutions*, and the present usage of the term is based upon the quite extensive discussion he gave there (1). Kuhn used the term 'exemplar' as a shorthand for 'shared example'. He emphasised that such a shared example derives from the concrete demonstration in some practical situation of the utility of a cultural product — a new experimental technique, a new theoretical model, or whatever — and that it is precisely through such demonstrations that new concepts are related to the natural world and acquire their meanings. Particular research networks within a given scientific community are to be seen as engaged in the articulation — the working-out in practice — of particular exemplars. As Kuhn put it:

More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science (2).

The idea of an exemplar is perhaps best clarified through historical examples — such as those given by Kuhn, and those discussed later in this paper — but a few remarks are required here, before turning to 'interests'.

Kuhn's work suggests that an exemplar may best be seen as a novel combination of cultural resources, the deployment of which constitutes a recognisable practical achievement. A question which naturally arises at this point is why one piece of work rather than another constitutes such a recognisable achievement — what, in particular, does 'recognisable' mean in this context? Consideration of this question will bring us to the concept of 'interest'. For the purposes of the present work, an exemplar can be regarded as an *analogy* drawn between some new aspect of the subject matter of the field and some other field of discourse (3). The aspect in question is conceptualised and organised by analogy with some other aspect of the field; the analogy being made concrete by the exemplary work. Thus the exemplar refers back, from

the problem to be understood at the research front, to some existing body of practice. Phrased in this way, it is clear that an exemplar is an example *for some particular group* — the group which has established the preceding body of practice — and not for others. One can speak of the group or groups having expertise relevant to the articulation of some exemplar as having an 'investment' in that expertise, and, as a corollary, as having an 'interest' in the deployment of their expertise in the articulation of the exemplar (4). An 'interest', then, is a particular constructive cognitive orientation towards the field of discourse. As a shorthand description one can refer to an exemplar being so constructed that it 'intersects with the interests' of some particular group or groups.

The outline of the 'interest model' is now almost complete, but one point remains to be stressed. The description of an exemplar as an analogy tends to suggest that it is a monolithic, unitary entity, but this is misleading and as open to criticism as the monolithic interpretation often put upon Kuhn's original description of a 'paradigm' (5). The important observation here is that an exemplar is typically a multi-dimensional construct, referring back not to a single unitary body of practice, but to several, possibly completely disjoint traditions. Thus individuals can set about articulating an exemplar along one of several dimensions, according to their prior investments and interests; and, in so doing, may well acquire new expertise and interests relevant to other dimensions of the exemplar.

To sum up, then, the image of the evolution of scientific culture implicit in the interest model is the following. The research front of a scientific speciality is to be seen as broadly patterned into research networks, each network being engaged in the elaboration of a particular exemplary achievement (6). Members of a given network can be described as having an interest in creating knowledge of a form characteristic of the original exemplar. However, the research front is further patterned, at a much finer level, by the constellation of interests proper to individual researchers; this constellation being idiosyncratic and determined by the researcher's professional evolution, moving from one network to another. This idiosyncrasy at the individual level allows for fluidity and the rearrangement of the pattern of networks: as new exemplars are created, new research networks can form as individuals engage in elaboration of the original achievement along dimensions appropriate to their pre-existing interests.

In outline, one can understand how the 'reality' of concepts emerges from this flux of changing networks in the following way. Firstly, consider the particular network (or cluster of networks) engaged in the elaboration of a particular exemplar. Entrenched within the practice of this network will be certain concepts central to the exemplary achievement. These concepts will be at the heart of the way in which members of the network make sense of their own and each other's work in the particular area of research. Because of this, a limited 'out-thereness' is achieved – the concepts become the relatively impersonal 'property' of the network rather than the personal 'property' of their creator. Secondly, because of the multidimensional aspect of exemplars noted above, it is possible for members of distinct networks to be engaged in the elaboration of the same concepts in different research areas. In this case, as we shall see, not only does the community of discourse about the shared concepts extend beyond a single network, but the entire practice of one network can come to be seen as important and relevant to the practice of another. Thus, the concepts originally entrenched in the practice of a single network can come to be entrenched in the practice of others, and their 'out-thereness' correspondingly increased.

One can at least imagine a situation in which all the members of a particular research specialty were engaged in the articulation of a single exemplar, or of a single cluster of exemplars related by unifying links (say, by a single explanatory theory and a unified ontology). This is the circumstance idealised by Kuhn in his idea of a community-wide 'paradigm', and it is clear that, here, scientists would speak unproblematically in terms of real objects and so on, deriving from the fundamental exemplar(s). The situation to be discussed in the case study approximates to that just described. It is, however, worthwhile to indicate in advance the differences between the actual situation and the ideal, in order both to illustrate the concept of consensus implicit in this paper, and to facilitate the introduction of a sociologically useful definition of 'ad hocness'.

We will see that by mid-1976, in consequence of various exemplary achievements, the existence of charm had become entrenched in the practice of several influential networks working in a variety of research areas. In contrast, although it remained in principle possible to reconcile colour (and other alternative models) with the data, the protagonists of colour had failed to achieve *exemplary* solutions to the misfits between their model and the

data. Although the responses of colour's protagonists to these misfits were logically sound, they could be described as 'sociologically *ad hoc*'. That is, they failed to intersect with existing interests within the HEP community, since they did not make any analogical connection with existing bodies of practice, and hence failed to lead to the establishment of any new bodies of practice.

Thus, by mid-1976, there existed what one might call an 'active' consensus on charm within the cluster of networks engaged in the elaboration of exemplars related in one way or another to the existence of charm. Actively opposed to this consensus were only a handful of theorists, each committed to one of a disparate set of alternative perspectives. (In fact, all of the public defenders of colour eventually abandoned their position. The ultimate residue of anti-charm theorists will not be further discussed in the case study, since this would require considerable additional technical detail and contribute no new insight into the process of consensus formation).

Finally, one can also speak of a community-wide 'passive' consensus existing on charm by mid-1976 (with, of course, the exception of the *isolated* opponents just mentioned). By that time, any physicist otherwise uninvolved in the charm-colour debate but obliged to choose between the two in the course of his research – for instance, to calculate the background contribution from the new particles to some experimental data – would naturally have chosen charm. The passive consensus implied by the existence of such a natural choice is readily understandable if one remarks that this hypothetical physicist would wish his own research product to be of utility within the HEP community at large, rather than to single, isolated, individuals.

The 'New Particles': Charm and Colour

We can now turn from the abstract sketch of the interest model to its application to an historical example. The example concerns a major development which took place in HEP between 1974 and 1976 (7). During this period several highly unusual elementary particles – the 'new particles' in the parlance of the time – were discovered, and a great deal of theoretical effort was directed towards an explanation of their properties. HEP theorists initially articulated a wide range of theoretical models of the new particles, but by mid-1976 a consensus had arisen that one of these models – known as 'charm'

— was right, and its competitors — principally a model known as 'colour' — were abandoned. The aim of the following sections of this paper will be to use the interest model to illuminate the way in which the reality of charm was accomplished, in comparison with the failure of the alternative models to achieve that status. I want first to give a broad outline and some general discussion of the developments at issue, and then to turn to a more detailed analysis in terms of the interest model. I should, however, clarify one point in advance. Several recent studies of the social construction of scientific knowledge have focussed on debates over experimental techniques: Collins, for instance, has characterised his study of the gravitational radiation controversy as a study of negotiations over what constitutes a 'working' gravitational wave detector (8). The study discussed in this paper concerns negotiations of a different kind. Throughout the period considered here, members of the HEP community treated experimental data as facts: no important negotiations over experimental techniques took place, but instead negotiations concerned how well the various theoretical models could cope with the data in the context of the existing traditions of HEP (9). With this proviso in mind, then, let me now turn to an outline of the actual events.

On November 11th, 1974, two groups of HEP experimenters, one from the East Coast and one from the West Coast of the U.S.A., announced their independent discoveries of a new and highly unusual elementary particle (10). One group named it the 'J', the other the 'psi', and by common consent it has become known as the 'J-psi'. I will return to the question of why it was unusual later; for the moment it is sufficient to note that its properties were not easily reconciled with expectations then current within the community. Within days of the discovery announcement the journals were flooded with a variety of theoretical speculations on the nature of the J-psi. Ten days after the announcement of the J-psi further experiment revealed the existence of another unusual particle — the 'psi-prime' (11) — and detailed measurements were soon made of the properties of both the J-psi and psi-prime. In the light of these observations, many of the early speculations on the nature of the J-psi quickly came to be seen as untenable, and in early 1975 only two theoretical models survived as serious contenders for the explanation of the new particles. These models were known as 'charm' and 'colour', and it is the way in which the choice was made between them that I want to discuss here (12).

The new particles had immediately become a major focus for experimental effort, and in response to the accumulating data the charm and colour models were progressively refined and articulated. In essence, one can say that each model came to be embodied as an ever more complex string of statements about the interactions of elementary particles. The individual statements comprising the string associated with a particular model were quite distinct and carried quite different associations from those of its rival, but nonetheless the protagonists of both models were sufficiently ingenious to avoid contradiction with the data: each new datum was accommodated by an appropriate adjustment somewhere in the string of statements — the adjustments, of course, differing according to the model used. Thus, at any particular time, the two models were able to give quite different explanations of the same set of observations.

This circumstance offers further support, should such be needed, to the idea that there is no such thing as a crucial experiment in science; and, from a somewhat wider perspective, constitutes a neat exemplification of the Duhem—Quine thesis, the latter being the holistic proposition that scientific knowledge is an inter-connected network and that unexpected experimental observations can, in principle at least, be accommodated by appropriate adjustments anywhere within the system (13). Contemporary philosophers of science have approached the problems posed by Duhem and Quine in a variety of ways, but in this paper I want to look at how they are handled in practice by a real scientific community (14).

In outline, consensus developed as follows. In late 1974 there was, as I have mentioned, no obvious preferred explanation for the new particles, although charm and colour were the most frequently discussed candidates (15). In the first six months of 1975 little new data of any significance appeared, but the colour explanation came to be seen as increasingly unlikely, although several individuals and groups continued to defend it in the literature and in conference talks. In July 1975 the discovery of the first of what proved to be a new family of related particles was announced, and this led to the wholesale abandonment of colour. From this time onwards only three theorists continued to advocate colour in the literature and at conferences. In the spring of 1976 a second family of related particles were discovered, which were almost universally interpreted as charmed particles, and by summer 1976 'charm [was] ready for the textbooks' (16), as one leading

theorist put it. Nonetheless, the recalcitrant colour theorists interpreted this second family as coloured particles and maintained their position. It was only in the summer of 1977, when detailed measurements had been reported on members of this second family, that they concluded that 'there was no way' (17) that colour could be defended, and abandoned it.

There were thus, as I will elaborate below, three distinct 'epochs' in the establishment of charm. In the first, from November 1974 to June 1975, a loose consensus formed in favour of charm: in the second, from July 1975 to May 1976, there was a relatively solid consensus favouring charm, and alternative models — principally colour — received little attention; and in the third, from June 1976 onwards, charm was regarded by the HEP community as a whole as established (18). The changes in consensus going from the first to the second epoch and from the second to the third were quite clearly associated with important qualitative changes in the data — the successive discoveries of new families of particles related to the J-psi and psi-prime — and there is a great temptation to treat this as the entire analysis: 'the data decided', one wants to say — but this would be a mistake. It would be a mistake because, on a rather abstract level, it ignores the consequences of the Duhem—Quine thesis, and it would be a mistake because, on a quite concrete level, it ignores the fact that the colour theorists were able to hold their model immune from falsification until well into the third epoch, and, in principle, for ever.

How, then, was the choice made by the community between the competing models? In the remainder of this paper I will argue and illustrate the thesis that it was the interaction of the differing theoretical responses of the two models to the emerging data with the pre-existing matrix of 'interests' supported by the HEP community which determined the triumph of charm and the demise of colour. What I want to show is the way in which the elements of the charm model intersected with the interests of an ever increasing number of the sub-cultures of HEP, and the failure of colour to emulate this. One consequence of this style of analysis is that it is inevitably technical, and it should be borne in mind in what follows that the discussion of technical points will necessarily be over-simplified and abbreviated, not least because of the very success of charm's protagonists in intertwining their chosen model with all the most promising (and popular) theoretical lines in HEP in the early 1970's.

First some technical preliminaries. Three distinct forces are recognised as important in the world of elementary particles; in order of decreasing strength these are known as the strong, electromagnetic and weak interactions (the fourth, gravitational, force is too weak to have any perceptible effect). Most particles, for instance the proton and neutron, experience the strong inter- action, and such particles are known as 'hadrons'. A subdivision of the family of hadrons, which will be significant in this discussion, is made on the basis of their intrinsic angular momentum or 'spin': particles having integral spin are known as 'mesons', while particles having half-integral spin are known as 'baryons'. A few particles, for instance the electron, experience only the weak and electromagnetic interactions, and these are known as 'leptons'.

This paper will be mainly concerned with conjectures as to the properties of hadrons, since both the charm and colour explanations of the J-psi and its subsequently discovered relatives asserted that that was what they were. As soon as the J-psi was discovered it was clear that if it was a hadron, it was highly unusual. Quite simply, it lived too long. At the time it was the heaviest particle known (more than three times more massive than the proton), and it was well established that massive hadrons are unstable, decaying rapidly to lighter hadrons with a lifetime of the order of 10^{-23} seconds; a lifetime, furthermore, which decreases with increasing mass of the parent. The J-psi was observed to live around 2000 times longer than any straightforward estimate of its expected lifetime, and the immediate theoretical problem was to find an explanation for this.

Both charm and colour gave explanations which depended upon variants of the 'quark' model. In this model hadrons are not seen as truly elementary particles, but rather as composites of more fundamental entities known as quarks. Mesons are pictured as made up of a quark plus an antiquark, while baryons are pictured as comprising three quarks. Quarks are thought to come in several different types, and the various species of quark are distinguished by assigning them different 'quantum numbers', in just the same way as is routinely done for the hadrons of which they are constituents. Some of these quantum numbers can be understood in terms of their macroscopic equivalents — one can think of the 'spin' of an elementary particle in analogy with the angular momentum of a spinning top — but others cannot, and are best thought of as book-keeping devices used to explain the various conser- vation laws which are observed to hold in the interactions of elementary

particles. Quarks are thought to carry two distinct sets of these book-keeping quantum numbers, quaintly known as 'flavours' and 'colours', and these were at the heart of the competing explanations for the new particles.

Let me give the charm explanation first. In the pre-psi era there were three established flavours of quarks — 'up', 'down' and 'strange'. The charm model asserted that a fourth flavour — 'charm' — existed, and that the J-psi was a meson composed of a charmed quark plus a charmed antiquark. Because the book-keeping rules for charm were those of simple addition, the net charm of the J-psi was supposed to be zero (the quark having charm +1, the antiquark charm −1). The charm explanation of the long lifetime of the J-psi was based on an empirical rule known as the 'Zweig rule'. The Zweig rule stated that decays of hadrons were inhibited whenever they involved the mutual annihilation of constituent quarks. This rule had been observed to hold in the few previous instances where it appeared to be applicable, and since the only way the J-psi could decay to the old hadrons was through mutual annihilation of its constituent charmed quarks, it clearly offered at least a qualitative understanding of its longevity.

Thus the charm explanation depended upon the existence of a new quark which carried a new flavour. The colour explanation, on the other hand, required no new quarks, exploiting instead the freedom of the colour quantum numbers. Quarks are supposed to come in three colours, — say, red, yellow, and blue. Colour labels combine together like vectors, and the old hadrons were all supposed to be colourless — each red quark, for instance, having its colour cancelled by a red antiquark, and so on. The colour model asserted that, for the first time, in the J-psi colour was visible; that in the J-psi the colour quantum numbers of the constituent quarks did not cancel out. Thus the J-psi was supposed to be a red particle (or blue, or yellow — which particular colour was immaterial). Hadronic colour was supposed to be conserved by the strong interactions; hence the J-psi could not decay to old colourless hadrons via the strong interactions; and hence its anomalously long life span.

These, then, are the essences of the two models, and we can now proceed to look at their respective fortunes.

The First Epoch

I will discuss the three epochs in chronological order. In the first epoch, when

only the J-psi and psi-prime had been discovered, the two models faced superficially equal obstacles to acceptance. Nonetheless, the charm model outstripped its rival in popularity, and the explanation for this lies in the differing extent to which the articulations of the two models intersected with the interests of various subcultures.

The immediate obstacle to the acceptance of charm was that the Zweig rule went some way towards explaining the lifetime of the J-psi and psi-prime, but not far enough. A straightforward extrapolation of the suppression of decays, from the few old hadrons to which the rule was applicable to the new particles, led one to expect that the J-psi would decay around 40 times faster than it actually did. There were, of course, two ways of looking at this discrepancy: as one group of theorists put it, 'this is either a serious problem or an important result, depending on one's point of view' (19). Theorists predisposed to charm saw it as the latter, and those to colour as the former, but it is highly significant that there was a third, ostensibly neutral, group which was inclined to side with charm. This was the school of 'hadrody-namicists', theorists who had invented, elaborated and used the Zweig rule throughout the 1960's. These theorists had invested time and energy in the Zweig rule; they had the expertise required to see the world constructively from its perspective, and one can straightforwardly say that they had an interest in the elaboration of the rule in the fresh context offered by the new particles. As I have mentioned, the Zweig rule was supported by relatively few observations, and if one accepted charm then a striking new datum was added to the set — namely, that the rule became more effective at higher energies — and there was new work to be done. Thus the most striking mismatch between the charm model and the data could be construed as a puzzle which intersected with the interest of an existing research network, and normal science could be, and was, done on it (20).

Furthermore, the proponents of charm themselves provided a possible set of tools for attacking this particular puzzle. To understand why they were in a position to do this some background is called for. The existence of charm was volubly advocated by a group of theorists centered on Harvard even before the J-psi was discovered. These physicists were the spearhead of what might be called the 'gauge theory revolution'. Gauge theory is a particular example of a quantum field theory, and as such is seen as a possible explana-tory base for the entire subject matter of HEP. Quantum field theory in

general had been in the doldrums from the late 1950's onwards, facing intransigent mathematical problems, until it was rescued by important theoretical developments in the early 1970's (21). As a result of these developments those few physicists who had retained an active interest in field theory came to realise that gauge theory in particular had many attractive features. Harvard was the most influential centre of field theoretic expertise, and throughout the 1970's theorists there (and, later, elsewhere) devoted a great deal of effort to the construction of models of elementary particle interactions consistent with gauge theoretical ideas. Again, this stream of practice is readily understood in terms of the prior interests of the theorists involved. Furthermore, as we shall see, several of these models came to constitute exemplars for the work of theorists not initially having an interest in gauge theory. What must be stressed at the outset is that gauge theorists did not confine their attention exclusively to charm, and that, since many physicists do not dissect their knowledge in the way I am presently attempting, successes for gauge theory anywhere fed-back as support for charm (and vice versa) (22).

This offers a nice illustration of the general remark made in the introduction concerning work on different exemplars sharing a common element. The point here is the following. During the period under consideration various gauge-theoretic exemplars were constructed, and the birth of any new stream of practice based on a gauge-theoretic exemplar increased the proportion of the community for whom gauge-theory constituted a central item of discourse. Gauge theory would enter the vocabulary of the theorists elaborating the model, the experimenters involved in the production of relevant and potentially relevant data, and so on. This enlargement of the 'gauge-theory speaking' community continually acted to further the 'out-thereness' of gauge theory, offering an immediate boost to the 'reality' of any new gauge-theoretic model. There was, in a very real sense, a revolution in progress in the period I am considering — and, as its most visible specific focus, charm both marked a watershed in the revolution and was reinforced by it.

To return to the story then, by mid-1974 gauge theorists had become convinced, for reasons which it is unnecessary to go into here, that for their approach to make sense — for their long-term goals in theoretical development to be accomplished — charm must exist (23). When the J-psi was discovered, they immediately interpreted it in terms of charm, and also gave a distinctive explanation of the super-Zweig rule required to explain its longevity. I will

not give details of this explanation, but simply note for the moment that it was based on a simple model motivated by the gauge theory candidate for the theory of the strong interactions, now known as 'quantum chromodynamics', or 'QCD' for short. Two facets of the QCD model, or the 'charmonium' model as it was called, are relevant to the present discussion. Firstly, it made concrete predictions for the existence of yet more long-lived new particles, and I will return to this in my analysis of the second epoch. Secondly, it constituted an exemplar for work outside the charmonium programme itself. Let me explain what I mean by this. The charmonium model asserted that the charmed quark and anti-quark which compose the J-psi interact in a simple way consistent with the general structure of QCD. As such it was an example of a long tradition of 'spectroscopic' quark models. It differed from its predecessors in specifying the model parameters in terms of the expectations of QCD. There was, of course, no reason to limit the application of the model to the new particles, and one group of Harvard physicists published an exten-sive analysis of the spectroscopy of the old hadrons using an appropriate formulation of the new model, in which they showed how existing puzzles could be seen in a new light, how new puzzles could be created and solved, and so on (24). The new model showed the existing network of quark spec-troscopists how they could enrich their approach and generate new soluble puzzles, and — in the background — the rising tide of gauge theory guaranteed that such work would be of interest to an increasing fraction of the com-munity, and not simply within the specialist network. It intersected with the interests of quark spectroscopists — they accordingly took it up, and this work fed back, by enlarging the 'charm-speaking community', as support for the reality of charm (25).

Here we can see clearly both the analogical and multidimensional aspects of exemplars. The analogical element of the new spectroscopic quark model resided in its treatment of the quark interactions within hadrons: these were treated in essentially the same way as the interactions of charged particles within well-understood atomic systems (more on this in the discussion of the second epoch). The multi-dimensionality of the exemplar arose from the juxtaposition of at least four identifiable elements: the atomic analogy just mentioned; the gauge-theory idiom in which it was couched; the contemporary practice associated with the spectroscopic quark model; and the set of relevant data, which was redefined by the model itself. Each of these dimensions

intersected with the interests of some group or groups, although, as it happened, only the dimension relating to contemporary quark modelling required any degree of sophistication, and hence it was quark-modellers (rather than, say, atomic physicists) who set about elaborating the new model.

To summarise, then: what charm did in the first epoch was:

(i) it generated a puzzle — the super-Zweig rule — which intersected with the interests of hadrodynamicists,

(ii) it supplied its own solution to this puzzle — the charmonium model — which had predictive power (and hence was welcomed by experimenters), and which intersected with the interests of hadron spectroscopists by enriching both their puzzles and their techniques, and

(iii) in a wider context it supported, and was supported by, the gauge theory revolution.

If we now turn to what colour did in the same period we will be able to gain some inkling of why the consensus moved strongly in favour of charm. There is something of a problem here since there was considerable diversity amongst specific colour models, which I will attempt to circumvent by dealing only with the most tenaciously defended variant of the model — that proposed by Gordon Feldman (of Johns Hopkins University) and Paul Matthews (then of Imperial College London, now Vice-Chancellor of the University of Bath) (26). (The third theorist mentioned earlier who continued active work on colour into the second and third epochs was Berthold Stech, of Heidelberg University. His version of the colour model differed in detail from that of Feldman and Matthews, but encountered analogous obstacles to acceptance. Where relevant I will indicate these in footnotes).

As I mentioned above, at the heart of the colour model was the proposition that the J-psi and psi-prime were manifestations of a new conserved quantum number called colour. This immediately explained the longevity of the new particles, and was, furthermore, historically dignified: the flavour known as 'strangeness' had been invented and accepted in similar circumstances in the 1950's. Thus, the colour model was, if anything, faced with fewer problems than charm when first proposed. However, it soon acquired a comparable obstacle to acceptance. Early in 1975, detailed experiment on the psi and psi-prime had revealed that photons (quanta of the electromagnetic field) were only to be found infrequently amongst their decay products. This was a considerable setback for the colour model since, straightforwardly

articulated, the model predicted that decays involving photons would be the predominant mode (27). The response of Feldman and Matthews to this observation, which was known as the 'radiative damping' problem, was to equate it with the problem of the super-Zweig rule which charm generated, and then essentially to ignore it (28). They felt that the QCD explanation for the super-Zweig rule was based on unjustified approximations, and thus, even if they themselves failed to offer an explanation for the radiative damping problem there would still be no rational grounds for choice between the two models (17). Logically, this reasoning was probably correct, but it generated little support for colour. And the reason for this was that the radiative damping problem, unlike the Zweig rule, intersected with no interests whatsoever within the community.

This is an important point which requires some elaboration. I have already stressed that the essential characteristic of an exemplar is that it refers back to an existing body of practice and associated interests as well as forwards to a new area of research, and it was in the respect of referring back that charm was so successful, and colour so unsuccessful. Feldman and Mathews's first response to the radiative damping problem illustrates this nicely, being clearly *ad hoc* in the sociological sense explained in the introduction. Feldman and Matthews invoked what they referred to as the 'Feynman rule'. If one follows up the reference they give for this rule, one finds, in a paper co-authored by Richard Feynman, the somewhat apologetic introduction of an arbitrary suppression factor, designed simply to improve the fit to the data of a phenomenological spectroscopic quark model. It is to this arbitrary factor that Feldman and Matthews refer. Being arbitrary, the factor was intrinsic to no body of practice – there was no expertise involved in its use, no interests were bound up in its deployment – and thus Feldman and Matthews' response to the radiative damping problem failed to intersect with any interests within the HEP community (29). As a consequence, no new body of practice came into being around the 'Feynman Rule'; there was no enlargement of the community of colour-related discourse, and no increase in the 'out-thereness' of colour.

Thus, in contrast with charm and the Zweig rule, colour's response to the radiative damping problem was completely *ad hoc*. In terms of the generation of either interesting puzzles or new exemplars, charm had a distinct and unarguable edge over colour (30). Also, although I will not discuss it in detail

here, the wider context of the gauge theory revolution favoured charm: in essence this was because the charm model as articulated by most theorists was based on fractionally charged quarks, while those theorists who related colour to their long-term interest in gauge theories did so on the basis of the unfashionable idea of integrally charged quarks (31).

The Second Epoch

For all of these reasons consensus with the HEP community moved strongly in favour of charm and against colour during the first epoch, but nonetheless the charm consensus remained tentative; at an international conference held in June 1975, Gordon Feldman recalls that around 10–15% of the submitted papers favoured colour rather than charm (17), and there were no less than four independent review talks on colour given, as compared with only one on charm (32). However, a dramatic change soon took place. In July, 1975, the discovery of what proved to be the first of a whole new family of related long-lived particles (which I will refer to as the 'chi' particles) was announced, and at another international conference held in August 1975 (at which time evidence had been announced for three such chi particles), a marked change in the balance of power was evident; Haim Harari, who gave the theoretical review of the new particles, only briefly discussed colour and concluded that 'in spite of the ingenuity that went into some of these models their gross features are incompatible with the experimental observations' (33); and James Bjorken, in his conference summary talk, described charm and the charmonium model as 'standards of reference [which] an experimentalist will naturally use to interpret his data' (34). The July 1975 announcement of the first chi particle thus marked the beginning of the second epoch, and from that point onwards Feldman, Matthews and Stech were left as the only public advocates of colour.

This transformation from the loose consensus of epoch one to the more solid consensus of epoch two is extremely interesting. The first point to be made is that, as I mentioned earlier, the charm model did predict the existence of a new family of particles. The 'charmonium' model, as the Harvard theorists called it, required the existence of five new long-lived particles less massive than the psi-prime, into which the psi-prime would occasionally decay with specified characteristics. Thus, at first glance, one is tempted to assert that

the HEP community acted in accordance with some model of scientific rationality akin to that implicit in Lakatos' idea of a 'research programme' in favouring charm in the second epoch; the charmonium model, one might want to say, involved a progressive problem-shift which received increasing empirical support throughout the second epoch. In more straightforward language, one might rephrase this as that it was obvious to all concerned that the charmonium model was right. This was, I think, how the situation appeared to the majority of the HEP community, but, even so, some explanation of why certain explanations are seen to be obvious is called for, and this point requires some emphasis before I give my own analysis of 'obviousness'.

The difficulty which arises in any simple explanation of the success of the charmonium model is connected with the lack of explicit criteria of 'confirmation'. Why, for instance, in August 1975, were the three chi particles then established seen as empirical support rather than as refutation for the charmonium model which predicted five? This question is not without point, since, as we shall see, the colour model, too, led one to expect the existence of particles intermediate between the psi and psi-prime. One might answer the question with the assertion that the community was guessing that a further two particles would be discovered. This is true, but the question then remains as to which factors disposed physicists to make that particular guess. Furthermore, by the summer of 1976 two further chi particles had been tentatively identified — making up the required total of five — and hailed as confirmation of the model (35). However, these latter two particles had properties which were in quite clear disagreement with the straightforward predictions of the model, and the 'it's obvious' explanation remains problematic even taking these into account.

This objection might seem a mere quibble, in the light of the evident weight given by the HEP community to the correct prediction of five chi particles, but to indicate that the quibble must be taken seriously let me refer briefly to later developments. In the period subsequent to 1976 the charmonium model was worked out by theorists in increasing detail and with increasing sophistication, and these theorists came to conclude that at least one of the chi particles — the least massive — had properties inconsistent with the predictions of the model. They concluded that, if this particle actually was a member of the same family as the others, then something must be radically wrong with the model. At the same time a high-precision experiment

was underway at Stanford to investigate the properties of the chi particle. This 'Crystal Ball' experiment, as it was known, failed to find any evidence of the lightest chi particle at the previously reported mass — but did find evidence for the 'same' particle at a considerably higher mass, much closer to the prediction of the model (36). This change in observed mass caused little surprise in the community, since the original observation had been made with very poor statistics.

This is not the place to elaborate on these later developments, but it is clear that, during the second epoch, the partially worked-out charmonium model was instrumental in maintaining the reality of some rather tenuous data, even while the data was seen as supporting the reality of the model. Hence one can only conclude that the dominance of charm in the second epoch stands in need of some explanation, and that an explanation in terms of, say, 'research programmes' and 'novel facts' would run into severe difficulties — since at least some of the 'novel facts' in question were stabilised only by the model they were apparently confirming (37).

How then is one to understand the success of the model? The explanation I will give is in terms of the intersection of interests. The forces which had been at work in the first epoch — the active streams of work on the Zweig rule and the new QCD-related quark spectroscopy, and the increasing momentum of the gauge theory revolution — could only be reinforced by any sign of confirmation of the charmonium model, and vice versa. And to these forces were added new ones.

To unravel the new sources of support for charm in the second epoch it is necessary first to outline the theoretical basis of the charmonium model. The model interpreted both the psis and the chis as bound states of charmed quarks, and it departed radically from earlier quark models in its assumption that charmed quarks are much heavier than the old up, down and strange quarks — so heavy, in fact, that within hadrons such as the psis and chis the constituent charmed quarks move very slowly (nonrelativistically) in comparison with the highly relativistic motion entailed by quark models of the old hadrons. Now, non-relativistic motion is much more tractable theoretically than relativistic motion. In particular, the charmonium model depended upon an image of a charmed quark non-relativistically orbitting its antiparticle under the influence of a central potential, in exact analogy with the atomic system of an electron orbitting a positron (the antiparticle

of the electron). The latter system is known as positronium, and is one of the textbook applications of quantum mechanics to atomic physics, being almost identical to the hydrogen atom in its formal treatment. The name charmonium was coined to make explicit the parallel between the envisaged structure of the new particles and the simplest and best understood atomic structure; and the psis and chis were pictured in the charmonium model as energy levels of the charmonium system in exact analogy with the energy levels of the positronium spectrum. The charmonium model illustrates perfectly the role of an exemplar as the concrete embodiment of an analogy relating a new field of research back to an established body of practice.

The model was intuitively transparent to any trained physicist, and this had two important consequences. Firstly, whenever the model encountered a mismatch with reality the resources were available to essentially anyone to attempt to fix it up, and for others to appreciate such work. In order to calculate the charmonium spectrum one had first to specify the potential in which the quarks moved. The earliest models chose simple potentials, and as data accumulated and discrepancies were noted with the predictions, more sophisticated potentials — involving 'hyperfine' splittings — were introduced to explain them in complete analogy to the detailed treatment of atomic spectra. Given the undergraduate training of physicists and the crude statement of the charmonium model, the pattern of extension of the model to the puzzles posed by the chi particles was obvious — and as data accumulated on the chi particles it became food for an active stream of normal science (38).

This stream of normal science was one clear source of fresh support for charm during the second epoch. Another source was less direct but more fundamental: namely, the charmonium model made quarks themselves 'real'. To fully appreciate this argument requires some knowledge of the history of the quark concept, which I can only outline here. Since their invention in 1964 it had never been clear to physicists what to make of quarks: were they real objects, or were they no more than mnemonics for observed regularities? Had it proved possible to isolate a single quark and study its properties this question would have been resolved, but an extensive programme of experiment had failed to do so; quarks — if they existed — remained obstinately confined within hadrons. Theoretical models based on quarks had been enormously successful in explaining various features of the data throughout

the 1960's and 1970's, but nonetheless all of these models were based on assumptions or approximations which made little sense if taken too seriously. The constituent quark model of hadron masses, for instance, was based on approximations which could not be justified in the context of the theory of special relativity — and relativity has always carried more weight in HEP than quarks.

Now, what the charmonium model did was to show that *if*, by some lucky chance, there happened to exist a species of quark which was very heavy, *then* it was possible to construct models of hadrons containing them which were not based on 'nonsensical' assumptions. No fundamental principles had to be ignored in the charmonium model: if charmed quarks were heavy, then the charmonium spectrum made just as much sense as the atomic spectra on which physicists are weaned. And furthermore, if the charmonium model worked — if it fitted the data — then quarks were just as real as electrons and positrons. Of course, this reasoning would strictly speaking only apply to the heavy, charmed quarks, but since these were to be seen as simply a new flavour of the old, light quarks, these, in their turn would be seen as real. There was thus great scope for positive feedback here. If the charmonium model worked, quarks would become real and doubts over the significance of the considerable amount of work being done on the basis of the quark model would be reduced; the entire community would be disposed to see puzzles thrown up by any variant of the model as valid and worthy of attention, and so on. It was a classic bootstrap situation in which the more one believed in and utilised the quark concept the more one was inclined to see any indication from the data as validation of the charmonium model, and the more one believed the charmonium model to have been confirmed, the more one was inclined to believed in and utilise quarks.

This massive intersection of interests is, I believe, the only possible explanation for the dramatic loss of support for colour in the second epoch. It is particularly noteworthy that those physicists supporting colour on the basis of models which could encompass both colour and charm at once abandoned the former for the latter (39). Also, in the third epoch, when charm had finally become institutionalised, all of the major retrospective review talks concentrated on the quark-charm-gauge theory nexus (40).

The activities of Feldman and Matthews in defence of colour during the second epoch only highlight the importance of the intersection of interests.

Their response to the discovery of the chi states was completely *ad hoc* in my sociological sense. They pointed out that the existence of the chi's did not falsify colour, but beyond that they had little to say. The colour model predicted the existence of coloured versions of all the old colourless hadrons. Feldman and Matthews had interpreted the J-psi and psi-prime as coloured versions of particles called 'rho mesons', and similarly they were at liberty to interpret the chi particles as coloured 'pions' and so on. Unfortunately they were unable to discuss what the masses of the chi's should be, how they related to the psi's, how long they should live – in short, all of the questions experiment could investigate (41). This was because their style of explanation was based on group-theoretical symmetry arguments and, for reasons I will not go into here, could not easily be adapted to the dynamical perspectives mentioned above which made charmonium so attractive. Thus the discovery of the chi states did not lead the colour model to intersect with any new interests – its simply remained silent on the topic – and it was stripped of almost all of its adherents by the success of charm (42).

The Third Epoch

So much, then, for the second epoch. It is now time to go on to the third and final epoch in which charm was enthroned as the real thing. Despite the success of the charmonium model in the second epoch, it could not be said it demonstrated that charm, as required by gauge theories, existed. The charmonium model only required that some new species of heavy quark existed, not necessarily carrying the specific quantum numbers associated with charm. As a dynamical model anathema to the philosophy of colour its success was quite sufficient to effectively bring about the demise of the latter, but to establish the existence of charm something more was required.

That something more was the observation of 'naked charm' – particles made up of a single charmed quark plus an old non-charmed quark and hence carrying non-zero total charm, in contrast to psis and chis in which the charm of quarks and antiquarks cancelled out. To cut a long story short, the first manifestation of naked charm was found in April 1976, in the shape of a long-lived meson known as the 'D'. The decays of this particle were seen to be highly unusual; and unusual, furthermore, in exactly the way the protagonists of charm had long predicted (43). This was enough to institutionalise charm

(44). In view of the forces at work which I have discussed earlier, and the ability of charm to provide an acceptable framework for normal science on this most recent new family of particles (45), this surely calls for no further explanation, and none will be given.

From the point of view of this paper the most interesting event in the third epoch was the abandonment of colour by Feldman and Matthews. Interestingly enough, this did not come about at once — indeed, as soon as the discovery of the D had been announced they published a paper entitled 'The Discovery of Coloured Kaons?' (46) — but by the summer of 1977 they had been worn down by the accumulating data. Their reasons for abandoning colour centred on rather technical issues, but I will attempt to give a brief sketch. In the 'coloured kaons' paper they had given definite predictions, which differed markedly from the equivalent predictions of charm, for a more massive particle related to the D. Unfortunately, subsequent experiment categorically supported charm and refuted colour. Since Feldman and Matthews were hardly in a position to challenge the fundamentals of the experimental method they sorted through the resources of their model, searching for a new candidate having the required quantum numbers to partner the D in agreement with experiment. Here they collapsed. Their only suitable candidate explained the existing observations, but also predicted phenomena which were not observed. They could have fixed this up by the invention of some mechanism, in a way analogous to that used to explain away the old radiative damping problem, but this would have been hopelessly *ad hoc*. Gordon Feldman certainly saw it as a totally meaningless manoeuvre and, as he put it, 'I made sure we were wrong ... there was no way ... these particles are presumably not ... colour ' (17). Colour could have been preserved from falsification, but the cause was manifestly lost — it would have served no purpose (47).

The point to be stressed here is that by this time it made essentially no difference to the reality of charm whether Feldman and Matthews maintained their defence of colour or not. Their sociologically *ad hoc* avoidance of falsification had failed to give birth to any new streams of practice, and they had been reduced to a discourse-community of two. Quite literally, they were reduced to talking to themselves. As Professor Feldman put it: "just to be able to talk to other physicists I have to be able to speak about it [i.e. charm]" (17). In the eyes of the HEP community, the colour model was

irrevocably attached to its authors, Feldman and Matthews, rather than residing 'out there' in the real world.

Summary and Conclusions

This completes my account of the establishment of charm and the demise of its principal rival, and the interpretation of this episode in terms of the 'interest model'. The concepts of 'exemplars' and 'interests' have been used to explain why the existence of charm came to be embodied in the practice of an increasing number of groups of HEP theorists, and why a similar development failed to occur in the case of colour. Through a series of exemplary achievements — giving solutions to its initial problems which were fruitful in a whole variety of directions, and tying these solutions into a gauge-theoretic framework — and the *ad hoc* failure of colour to emulate these, charm became entrenched in the practice of several sub-cultures of theoretical HEP, and in the process became real.

In conclusion, let me make three comments. Firstly, for reasons of space, this paper contains something of an omission. It has concentrated on HEP theorists and underemphasised their experimental colleagues. In fact, it must be acknowledge that the interplay between these two groups was very important in the establishment of charm. During the three epochs I have discussed, charm became central to the practice of many experimenters as well as theorists. Indeed, it was only after a determined and goal-oriented programme of experiment and analysis that the D-mesons, which set the final seal on charm, were found. Without such involvement of the experiment community in the domain of discourse it is hard to see how the reality of any theoretical construct could come about. Even so, if this paper were expanded to include the development of charm-related experimental programmes, the form of the analysis would not be altered — although some discussion would be called for of the role of theoretical models in the way experimenters make sense of their practice.

The second comment is more important, and more positive. It is that the scheme of scientific development outlined by Kuhn in the 'Postscript' to his *Structure of Scientific Revolutions* seems to work, at least in the interpretation adopted in the interest model. This point needs to be made explicit, since despite the widespread interest and controversy which have surrounded

Kuhn's work, very little attention has been paid to empirical investigation of his assertions. From the present case study it appears that bodies of scientific practice devoted to the elaboration of particular exemplars *can* be identified, and that the outcome of this episode, which has been very influential in the subsequent development of HEP, can be understood in terms of these bodies of practice. Besides the demonstration of this, the most important points to emerge from the study concern the nature of exemplars. Their defining feature is seen to be their analogical role — referring back to established bodies of knowledge and practice. It is precisely the presence or absence of such 'referring-back' which enables one to distinguish between exemplary achievements and those which I have called *ad hoc*. Finally, as the concrete embodiment of analogy, the aspect of exemplars which must be stressed is their multidimensional nature. It is only because exemplars are multidimensional constructs that one can conceive of constellations of *related* bodies of practice based upon different exemplars — and the solidity of our knowledge is clearly dependent upon the extent of such constellations.

The third comment concerns a possible direction in which the image of science implicit in the interest model may lead us, and can be introduced by reference to the work of the philosopher of science, Mary Hesse. In looking at the developments through which charm became established one cannot fail to be reminded of Hesse's network model of knowledge (48). Charm became intrinsic to the practice of the HEP community through becoming a principal focus of just such an interconnected network of generalisations as Hesse describes. Conversely, colour and the other alternative models failed because they did not become tied into the network; they floated free and continued to be identified with their creators rather than with the natural world. In this sense the interest model is a sociological counterpart of Hesse's network, setting out to understand the social dynamics underlying the network's evolution. One particularly interesting observation which comes out of the present case study concerns the role of the charm model in relation to the ambiguous data on the chi particles during the second epoch. It is clear that as theoretical models become more securely tied within the whole network of theory they can play an increasing role in the 'stabilisation' of data. Furthermore it can, I believe, be argued that this role extends far beyond that of simply removing the ambiguity inherent in data of poor statistics, ultimately to the level of stabilising experimental techniques, methods and

procedures (49). It may be possible, by pursuing this line of thought, to recover Kuhn's full-blooded conception of a large-scale, community-wide paradigm, as a coherent network of theoretical and experimental practice. This would clear a path for the exploration of the associated concepts of incommensurability and revolution – an exploration which has been bogged-down for too long in philosophical debate.

Notes and References

1. T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago University Press, Chicago and London, 2nd edn., 1970, pp. 174–210.
2. *Ibid.*, p. 187.
3. The relation between exemplars and analogies has been emphasised by Margaret Masterman, 'The Nature of a Paradigm'. In I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970. The central role of analogy in scientific thought is treated from a philo-sophical perspective in M. Hesse, *The Structure of Scientific Inference*, MacMillan, London, 1974. For a sociologically oriented discussion of analogical thought, see K. D. Knoor, 'The Scientist as an Analogical Reasoner: A Critique of the Metaphor Theory of Innovation', this volume.
4. For the use of 'investment' in the analysis of science, see B. Latour and S. Woolgar, *Laboratory Life: the Social Construction of Scientific Facts*, Sage, Beverly Hills, 1979, and K. D. Knorr, 'Producing and Reproducing Knowledge: Descriptive or Constructive?' *Social Science Information* **16**, 669–696 (1977). Work which refers to 'interests' includes S. B. Barnes, *Interests and the Growth of Knowledge*, Routledge and Kegan Paul, London, 1977; D. A. MacKenzie, 'Statistical Theory and Social Interests: a Case-Study', *Social Studies of Science* **8**, 1978, 35–83 (1978); S. B. Barnes and D. A. MacKenzie, 'On the Role of Interests in Scientific Change'; and S. Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', both in R. Wallis (ed.), *On The Margins of Science: The Social Construction of Rejected Knowledge*, Keele, *Socio-logical Review Monograph* **27**, 1979.

 As Kuhn has remarked (*op. cit.*, Note 1, 187–204) the utility of concepts such as exemplars (and, by implication, expertise, investments, interests and so on) derives from the existence of an inarticulated, tacit component of scientific practice (see M. Polanyi, *Personal Knowledge*, Routledge and Kegan Paul, London, 1973; H. M. Collins, 'The TEA Set: Tacit Knowledge and Scientific Networks', *Science Studies* **4**, 165–186 (1974), and 'Building a TEA Laser: the Caprices of Com-munication, *Social Studies of Science* **5**, 441–50 (1975)). In the present paper exemplars and interests are used as mediating concepts in the analysis of the process whereby the idiosyncratic and personal practice and experience of individual scientists is organised into a relatively impersonal system of articulated knowledge. It is this transmutation of personal experience into articulated knowledge which allows one to speak of the *social construction* of 'objects'.

5. The monolithic characterisation of a 'paradigm', and the refutation of such a concept, are typified by the approach of J. W. N. Watkins, 'Against "Normal Science"'. In I. Lakatos and A. Musgrave (eds.), *op. cit.*, Note 3.

6. 'Research network' here refers to a group of scientists sharing a common cognitive orientation, such as one might identify using the techniques of citation analysis, rather than to a group primarily identified by communication patterns.

7. The empirical data on which this paper is based derive from an extensive review of the HEP literature (technical and popular), and correspondence and interviews with around 60 leading physicists, including the main protagonists of the 'charm' and 'colour' models (see below). I have given a more extensive account and documentation of the period under consideration here in 'Model Choice and Cognitive Interests: A Case-Study in Elementary Particle Physics' Edinburgh preprint (1978, unpublished), and this will constitute part of my Ph.D. thesis (Edinburgh, forthcoming).

8. H. M. Collins, 'The Seven Sexes: a Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology* 6, 141–184 (1976).

9. HEP theorists and experimentalists constitute distinct professional groups. The accomplishment of the reality of objects clearly requires the involvement of both groups, and the simplest way in which this can come about is through each group regarding the products of the other as 'fact'. This is by no means always the case in HEP, but, since much of the data on the new particles was produced using *standard* techniques, solidly rooted in practice, there was little incentive for theorists to query the empirical material at their disposal. (For a minor qualification to this, see discussion of the 'chi' particles given below).

10. The discovery on the East Coast was made by a group from the Massachusetts Institute of Technology (MIT) led by Samuel C. C. Ting, working at the Brookhaven National Laboratory on Long Island. The West Coast discovery was made by a Stanford Linear Accelerator Center (SLAC) – Lawrence Berkeley Laboratory (LBL) collaboration led by Burton Richter, working at SLAC in Stanford, California. The publications announcing the discovery were, respectively, J. J. Aubert *et al.*, 'Experimental Observation of a Heavy Particle J', *Physical Review Letters* 33, 1404–5 (1974); and J.-E. Augustin *et al.*, 'Discovery of a Narrow Resonance in e^+e^- Annihilation', *Phys. Rev. Lett.* 33, 1406–7 (1974).

    Ting and Richter were rewarded for their discovery of the J-psi by the joint award of the 1976 Nobel Prize for Physics. The 1979 Nobel Prize for Physics was shared by three theorists whose work was central to the episode under discussion: S. L. Glashow of Harvard University, one of the inventors of charm and its most determined advocate; and S. Weinberg, also of Harvard, and A. Salam, of Imperial College London and the International Centre for Theoretical Physics, Trieste, who independently proposed the unified gauge theory (see below) of the weak and electromagnetic interactions, within which charm achieved major significance.

11. This particle was discovered by the SLAC-LBL group and was reported in G. S. Abrams *et al.*, 'Discovery of a Second Narrow Resonance', *Phys. Rev. Lett.* 33, 1453–4 (1974).

12. An analysis of the failure of other proposed models of the new particles would go along exactly the same lines as that to be given for colour, and will therefore not be explicitly given here.

13. For a discussion of the Duhem–Quine thesis and the philosophical problems associated with a belief in the existence of crucial experiments, see I. Lakatos, 'Falsification and the Methodology of Research Programmes' in I. Lakatos and A. Musgrave (eds.), *op. cit.*, Note 3.

14. For instance, Lakatos (*op. cit.*, Note 13) provides a normative methodology which he believes to be adequate. I will indicate below why Lakatos' technique of rational reconstruction is inapplicable to the episode under discussion here.

15. The three major rapid publication HEP journals are *Physical Review Letters, Physics Letters* and *Lettere al Nuovo Cimento*. Between them they published 62 theoretical papers submitted before the end of January 1975 on the J-psi and the psi-prime. Of these articles 24 favoured charm, 7 colour, 11 the so-called 'IVB hypothesis' which was quickly abandoned, and 14 unconventional models (the remaining 6 concerned the application of conventional techniques to the data). It would be unwise to make quantitative inferences from these numbers alone since many papers went unpublished during this period, and I base my assertion that there was no clearly preferred theoretical explanation for the new particles on interview material and the production of inconclusive contemporary review articles at the major research centres (for instance H. Harari, 'PSIchology' SLAC preprint PUB-1514, 1974 and 'CERN Boson Theory Workshop', CERN preprint TH. 1964, 6.12.74).

16. J. Ellis, 'Charm, Apres Charm and Beyond', Lectures presented at the Cargese Summer Institute on High Energy Physics, Cargese, Corsica, 5–22 July 1977 (CERN preprint TH. 2365 [1977] p. 1).

17. G. Feldman, interview, 10.5.78.

18. I will not document these changes in consensus in detail here. Since the new particles generated a great deal of excitement within the HEP community they were intensively reported in the scientific press, and one can obtained an overview of the evolution of consensus from the pattern of this reporting. Thus *Science* carried one report (186, 909–911 (1974)), in epoch one, which indicated that there was no theoretical consensus on the nature of the new particles, and two reports (189, 443–5 (1975) and 191, 452, 492 (1975)) in epoch two which discussed only charm. The most detailed contemporary popular reporting was in the *New Scientist*; from November 1974 to March 1975 a variety of theoretical explanations were noted, but in the thirteen articles subsequently published during the first two epochs, charm was the only explanation to be discussed.

19. M. K. Gaillard, B. W. Lee and J. L. Rosner, 'Search for Charm', *Reviews of Modern Physics* 47, 277–310 (1975); at p. 300.

20. To give some sort of quantitative feel for this, let me note that *Physics Letters* (the major European rapid publication journal) published 14 papers on the Zweig rule *per se* (i.e. treating it as a fact to which the new particles were relevant rather than as part of a putative explanation of the new particles) during epochs one and two (submission dates of these papers range from 12.5.75 to 7.6.76). I have been unable to locate any analogous papers, in any journal, devoted to the parallel problem engendered by colour (see below).

The pre-existing interest in the Zweig rule was indicated by the uneasiness manifested by the HEP community over its 'proper' name when it came to prominence in connection with the new particles. Many physicists, anxious to give due credit,

began to refer to it as the Okubo-Zweig-Iizuka rule, after the three theorists who had independently contributed to the original exemplary achievement between 1963 and 1966. In a 1977 paper (Fermilab preprint Conf. − 77/76-THY) Lipkin solved the problem of eponymy thus:

> The rule forbidding [various reactions] has been credited to a number of physicists in various combinations. To avoid arguments about credit, this paper refers to the A . . . Z rule and allows the reader to insert the names of all desired friends from Alexander to Zweig. (p. 2)

In a recent letter, Professor Kalman of Concordia University pointed out to me an alternative solution: 'so many authors opposed [the Zweig rule] that perhaps the widely used term Quark Line Rule is better.' This latter title is interesting since it suggests, quite correctly, that during the late 1960's and early 1970's the rule had been extensively discussed in terms of the quark model, and hence draws attention to the great significance of the meshing of charm and the quark model (see below). However, I retain the name 'Zweig rule' because it should be emphasised that the rule was not the exclusive property of quark theorists (indeed, when Okubo first proposed it in 1963, quarks had yet to be invented).

21. The important breakthrough was the demonstration by G. 't Hooft, then a graduate student at the University of Utrecht, that gauge theories possessed a most desirable mathematical property known as 'renormalisability' (G. 't Hooft, 'Renormalisation of Massless Yang−Mills Fields', *Nuclear Physics* **B33**, 173−199 (1971)).

22. The most important independent source of support for gauge theories during this period came from the accumulating data on 'weak neutral currents', predicted by gauge theory models and first discovered in 1973. For popular accounts of the relevance of these, see 'Neutral Currents: New Hope for a Unified Field Theory', *Science* **182**, 372−374 (1973), and 'The Detection of Neutral Weak Currents', *Scientific American* **231**, 108−119 (Dec. 1974).

23. In essence, the existence of charm was the simplest explanation for the set of experimental data relating to weak neutral currents (see note 22).

24. A. De Rujula, H. Georgi and S. L. Glashow, 'Hadron Masses in a Gauge Theory', *Physical Review* **D12** 147−162 (1975).

25. For instance, O. W. Greenberg of the University of Maryland was, in the 1960's, one of the pioneers of both colour and quark spectroscopy. During epoch one he propounded colour as an explanation of the new particles. Discussing his reasons for subsequently abandoning colour in favour of charm (interview, 11.5.78) he remarked that 'QCD has made a lot of impressive successes' and cited a recent fit of baryon masses, based on the exemplar discussed here, as a 'great advance'.

26. The lack of unanimity amongst colour modellers was clearly significant in their failure to achieve objective status for their viewpoint: each variant of the model was readily seen to be proper to the individual or small group proposing it. The diversity of approaches reflected the failure of each of the groups involved to achieve exemplary solutions to the problems thrown up by the relation of the model to the data (see below).

27. This problem had become apparent by January 1975. See O. W. Greenberg's review of colour models, 'Electron-Positron Annihilation to Hadrons and Color

Symmetries of Elementary Particles', in A. Perlmutter and S. Widmayer (eds.), *Theories and Experiments in High-Energy Physics*, Plenum Press, New York, 1975.

    A similar problem concerned colour's prediction of electrically-charged partners of the J-psi and psi-prime. See B. Stech, 'Broken Color Symmetry and Weak Currents', in H. Faissner *et al.*, (eds.), *Proceedings of the International Neutrino Conference, Aachen, 1976*. It should be noted here that the following account will focus on those features of the data which determined the final outcome of the battle between charm and colour. In fact, some of the most straightforward predictions of the colour model related to experiments such as the production of the new particles in hadronic or neutrino-induced reactions which gave inconclusive results, especially favouring the predictions of neither model.

28. In their first paper on colour and the new particles ('Has ψ_4 Already Been Observed at Stanford Linear Accelerator Center?', *Phys. Rev. Lett.* **35**, 344–6 (1975)) Feldman and Matthews listed predominantly radiative decays as a prediction of their model, but disavowed this in a short footnote which read 'These modes are damped by the Feynman rule'.

29. One could labour this point further, but probably the following comment of Feynman and his colleagues on introducing the suppression factor in their original paper will suffice: ' . . . in a most unsatisfactory way, [we] have included . . . an adjustment factor F . . . . This is frankly just empirical fitting' (R. P. Feynman, M. Kislinger and F. Ravndal, 'Current Matrix Elements from a Relativistic Quark Model', *Phys. Rev.* D3, 2707 (1971)).

30. Stech's response to the radiative damping problem was to point out that if the typical radius of the new particles was large compared with the wavelength of the emitted radiation, then straightforward estimates of the rate of radiative decay would be too large, 'Thus', as he put it (private communication), 'there was an answer to the critics and no objection was raised against this answer'. However, this response also failed to coincide with any current interests, and Stech himself did not indicate how it might be amplified into a phenomenological programme; it, too, was *ad hoc*.

31. In the original formulation, given in 1964 by M. Gell-Mann and G. Zweig, quarks were supposed to carry fractional electric changes (either one- or two-thirds of the charge of the electron). Building upon the exemplary achievement of these two physicists the main line of quark theory, including gauge theory formulations, had continued to assume fractionally charged quarks. With the introduction of the colour quantum number it became possible to envisage integrally charged quarks, and, in papers published in 1973, J. C. Pati (of the University of Maryland) and A. Salam (of Imperial College London) had constructed a gauge theory model containing such quarks (see note 39). However, since there was no empirical evidence at that time favouring integrally charged quarks the majority of theorists continued to work with the fractionally charged versions. The advocacy of colour as an explanation of the new particles by Pati and Salam can be seen as an attempt to demonstrate the relevance of their approach to the 'fractional charge speaking' community.

32. A. Zichichi (ed.), *Proceedings of the EPS International Conference*, Palermo, Italy, 23–28 June 1975, Editrice Compositori, Bologna, 1976.

33. W. T. Kirk (ed.), *Proceedings of the International Symposium on Lepton and Photon Interactions at High Energies*, Stanford, 21–27 August 1975, p. 323.

34. *Ibid.*, p. 991.

35. See, for instance, the review given by B. H. Wiik, 'Plenary Report on New Particle Production in e^+e^- Colliding Beams', in *Proceedings of the 18th International Conference on High Energy Physics*, Tbilisi, U.S.S.R., July 1976, pp. N75–85.

36. I am extremely grateful to Professor James Bjorken of the Stanford Linear Acelerator Center who first made me aware of these developments. Bjorken described the situation in February 1979 thus: 'The theoretical status [of the two most recently identified chi particles] has been evolving; by now they have been firmly denounced by several of the charmonium experts I most respect . . . as not having anything to do with the charmonium model. Thus, predicting three out of five observed states [i.e. particles] may not be that much of a triumph' (personal communication). And he continued by describing the outcome of confirmation of the two dubious chi particles by the 'Crystal Ball' experiment as possibly 'leading to a new theoretical crisis'. Before the 'Crystal Ball' data became available I was able to confirm Bjorken's assessment of the situation in correspondence with two of the charmonium experts involved: H. J. Schnitzer of Brandeis University, and L. Okun of The Institute of Theoretical and Experimental Physics, Moscow (see note 38). (The Russian group had outlined their position in, 'η_c Puzzle in Quantum Chromodynamics', M. A. Shifman *et al.*, *Physics Letters* 77B 80–83 (1978)). The first indication that the lowest mass chi particle could not be found at the mass previously reported came in E. D. Bloom, 'Initial Studies of the Charmonium System Using the Crystal Ball Data at SPEAR', XIVth Rencontre de Moriond, Les Arcs-Savoie, France, March 11–23, 1979. Since then I have heard from a third charmonium expert, Professor K. Gottfried of Cornell University, who wrote: ' . . . we are now in the happy situation of not having to deal with these embarassing objects. The charmonium model is therefore in a far healthier state than it has ever been' (personal communication, April 1979).

37. For several instances of unreflective reference to 'novel facts' in connection with Lakatos' methodology of research programmes, see the essays contained in *Method and Appraisal in the Physical Sciences*, C. Howson (ed.), University Press, Cambridge, 1976.

38. The first and simplest charmonium model was constructed by two Harvard theorists, H. D. Politzer and T. Appelquist. Although they had given seminars on the model, predicting the existence of very long-lived particles even before the discovery of the J-psi, they did not submit their work for publication until the discovery had been announced; (their first paper on the model was 'Heavy Quarks and e^+e^- Annihilation', *Phys. Rev. Lett.* 34 43–45 (1975)). The model was quickly adopted by theorists all over the world, leading to a stream of increasingly sophisticated phenomenology of the new particles which continues to the present. Most intensive work on the subject has been done by E. Eichten and K. Gottfried of Cornell University, H. J. Schnitzer of Brandeis University, and a Russian group led by L. B. Okun and V. I. Zakharov of the Institute for Theoretical and Experimental Physics, Moscow. References to this work and that of many others can be found in the major review article by V. A. Novikov *et al.*, 'Charmonium and Gluons', *Physics Reports* 41 1–133 (1978).

39. For instance J. C. Pati and A. Salam (of the University of Maryland and Imperial College London) were at the time working on a gauge theory model which required that both charmed and coloured particles exist. In his first post-psi review talk in January 1975, Pati listed three possible explanations of the J-psi and psi-prime in the Pati-Salam model, two of which were essentially colour and the third charm. In his next major review, in September 1975, he discussed only one theoretical interpretation, which was essentially equivalent to charm, and from this point on he and Salam ceased to advocate colour as the explanation of the new particles. Pati's reviews are reproduced as 'Particles, Forces and the New Mesons' in A. Perlmutter and S. Widmayer (eds.), *op. cit.*, Note 27, and 'The World of Basic Attributes: Valency and Colour', in R. Arnowitt and P. Nath (eds.), *Gauge Theories and Modern Field Theories*, MIT Press, Cambridge Mass., 1976.

40. See, for instance: S. D. Drell, 'Elementary Particle Physics', *Daedalus* 1 15–31 (1977); V. F. Weisskopf, 'New Trends in Particle Physics', talk delivered at the metting of the Societe Francaise de Physique in Poitiers, July 1 1977 (CERN preprint TH. 2379 [1977]);

L. Van Hove, "Hadrons and Quarks in High Energy Collisions', review article for a general audience of physicists to be published in Russian translation in *Uspekhi Fiz. Nauk.* (CERN preprint DG-3 [20.10.1977]);

S. D. Drell, 'When is a Particle?', Richtmeyer Memorial Lecture, San Francisco 24 January 1978, reprinted in *Physics Today* 31 (6), 23–32 (1978);

Y. Ne'eman, 'Progress in the Physics of Particles and Fields', *Physics Bulletin* 29, 422–24 (1978);

V. Weisskopf, 'What Happened in Physics in the Last Decade', *Phys. Bull.* 29, 401–403 (1978).

41. Feldman and Matthews stated their position on the chi particles in 'A Case for Colour', *Nuovo Cimento* 31A, 447–486 (1976), and 'Colour Symmetry and the Psi Particles', *Proceedings of the Royal Society, London* A355, 621–627 (1977).

42. Like Feldman and Matthews, Stech felt that the existence of the chi particles did not falsify colour, but he too was unable to provide a constructive approach to them. As he put it, 'our colour model required from the beginning (*without a good theoretical framework, however*) that the effective quark masses are not fixed but dynamical . . . and give rise to a spectrum of the kind which was found' (personal communication, emphasis added).

43. G. Goldhaber, F. M. Pierre *et al.*, 'Observation in e^+e^- Annihilation of a Narrow State at 1865 MeV/c^2 Decaying to $K\pi$ and $K\pi\pi\pi$', *Phys. Rev. Lett.* 37, 255–259 (1976).

44. See the *Proceedings of the 18th International Conference on High Energy Physics*, Tbilisi, U.S.S.R., July 1976 – for instance, the reviews given by R. Schwitters (pp. B34–39), B. H. Wiik (pp. N75–85) and A. De Rujula (pp. N111–127). See also the popular scientific press of the time, for instance 'Charmed Quarks Look Better Than Ever', *Science* 192, 1219–1220 (1976); 'Iliopoulos Wins His Bet', *Nature* 262, 537–538 (1976); and 'Naked Charm Revealed at Stanford', *New Scientist*, p. 413 (20.5.76).

45. Even before the discovery paper on the D's had been submitted for publication, the Harvard group had written a paper on the analysis of the data in terms of charm. Entitled 'Is Charm Found?' (A. De Rujula, H. Geogi and S. L. Glashow,

Phys. Rev. Lett. **37**, 398–401 (1976).) this paper went into the data in 'disgusting detail' (interview, A. De Rujula, 20.3.1978) to demonstrate its consistency with the predictions of the group's 1975 paper (*op. cit.*, Note 24).

46. G. Feldman and P. T. Matthews, 'The Discovery of Coloured Kaons?', *Physics. Lett.* **64B**, 353–358 (1976). This was the last paper which Feldman and Matthews were to publish on colour. My account of subsequent developments in their position is based on interview and correspondence with G. Feldman.

47. Although he adopted a completely different explanation of the D's from Feldman and Matthews, Stech ultimately encountered exactly the same obstacle. In a paper submitted for publication in November 1976 ('Are the D Mesons Diquarks?', *Phys. Rev. Lett.* **38**, 304–306 (1977): references to Stech's earlier work on colour are to be found in this paper) he suggested that the D's could be successfully accounted for as bound states of two quarks (n.b. *not* a quark and an antiquark, as in all conventional pictures of mesons). Stech's feelings at this time seem to sum up those of all of colour's public advocates and also those who had ceased to support it actively:

> On more general grounds, however, there was little hope that the colour model would survive. In talks and conversations I pointed out that the model and the data had now become so tight that there was no freedom for excuses in case even a single prediction went wrong'. (personal communication)

And indeed, like Feldman and Matthews, he now considers colour to have been 'indirectly disproved' by the accumulating data on charmed mesons *plus* the failure to find similar additional particles predicted by his model.

48. M. Hesse, *op. cit.*, Note 3.

49. The following case-studies are relevant here: S. Shapin, 'The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes', and H. M. Collins and T. J. Pinch, 'The Construction of the Paranormal: Nothing Unscientific is Happening', both in R. Wallis (ed.), *op. cit.*, Note 4; B. Wynne, 'C. G. Barkla and the J Phenomenon: a Case-Study in the Treatment of Deviance in Physics', *Social Studies of Science* **6** 307–47 (1976); H. M. Collins, 'The Seven Sexes', *op. cit.*, Note 8; 'The TEA Set' and 'Building a TEA Laser', *opera cit.*, Note 4; A. R. Pickering, 'The Hunting of the Quark', *Isis*, forthcoming, and 'Experts and Expertise: The Case of the Magnetic Monopole', *Social Studies of Science*, forthcoming.