

**ANOMIE AND NORMAL SCIENCE:
A CASE STUDY IN SEDIMENTOLOGY.**

**John Law,
Department of Sociology,
University of Keele,
Keele,
Staffs,
UK.**

14th October, 1976.

Introduction

In this paper I discuss the intellectual development of a generally unsuccessful scientific technique that has evolved within a successful scientific specialty - that of particle size analysis and its use as a diagnostic environmental tool within American sedimentology. The method I have used is quite standard. I have collected data on intellectual and social change amongst the group of practitioners from a variety of sources, including the scientific literature, secondary publications, and a set of extended interviews with those who have made important contributions to the literature of the field. From such data I have obtained a number of different, and sometimes conflicting accounts of the nature, relevance, and importance of various events that are held to have contributed to the current state of the art.

Having obtained such data, I have tried, in the traditional way, to piece together such accounts to make a single, coherent description of cultural and social change in the specialty. Such methods are, of course, standard in the history of science, and have also been used in the sociology of science by such authors as Edge and Mulkay, Mullins, and indeed myself in earlier work on protein X-ray crystallography.⁽¹⁾ In such work certain inevitable difficulties arise. Accounts provided by practitioners are never in full agreement, and are sometimes incompatible. The area of particle size analysis I have found to be particularly prone to such difficulties. Not only has there been controversy in the field, but in addition there has been what must be described as massive cultural breakdown. This breakdown contrasts rather

strikingly with the stringent, coherent and agreed standards laid down and obeyed by X-ray crystallographers. In the latter case it was reasonably easy to write an account which all practitioners could recognise as accurate. In the case of particle size analysis this has proved to be a great deal more difficult. However, the very incoherence of the field is itself a challenge as it raises problems about how we might conceptualise situations where there has been massive cultural breakdown.

The aims of this paper are thus fourfold. First I will illustrate the above - an instance where, after general initial scientific agreement, the methods, and ultimately the goals of a group of practitioners working within a specialty became problematic. Secondly, I shall use a version of R.K.Merton's theory of anomie⁽²⁾ to display the way in which this change occurred. Thirdly I shall attempt to draw out certain implications of this theory in order to allow it to play a more satisfactorily explanatory role in the present context. This will concomitantly mean that other aspects of the theory will not be explored. Fourthly, I shall draw attention to the relationship between anomie theory and Kuhn's notions of "normal science" and science in crisis.

My method of presentation will be to weave the theory and data together as closely as possible. I will therefore introduce theoretical notions at the various points throughout the paper where they become relevant. Throughout, however, I have made use of three very basic assumptions, and it is appropriate to mention these here. First, I assume that scientific knowledge in situations of normal science is normative. That is, I assume that theories, methods, findings, rules of thumb and values to some degree specific

to each community are used by specialists to orient their actions,
and judge one another's contributions. When I talk of 'scientific culture' it is to such norms that I refer. This first assumption is not novel. Kuhn's work seems to suggest such a possibility, and it has been explored by such writers as Mulkay, Barnes and Doby, and Mullins.

The second assumption is no more unusual. This is that the scientific community may be seen as being divided into specialties, research fronts, or invisible colleges, and that scientific contributions are judged in the first instance by fellow specialists.⁽³⁾ I am aware of the difficulties surrounding the use of the term specialty⁽⁴⁾ and in particular the circularity involved in Kuhn's original discussion of the subject. In this paper I have not escaped this circularity for most of the data is cultural, but since I am primarily concerned with an analysis of cultural change, I do not think that in this case the circularity is serious.

Thirdly, I assume that honorific rewards - publication, citation, professional prizes and offices - are made available to those whose contributions are held to be satisfactory. This is an equally standard theme, outlined and developed by such writers as Merton and Hagstrom.⁽⁵⁾

Defining the Puzzle

It will be remembered that Kuhn tells us that disciplines, specialties, or research fronts may be seen as paradigm bound during periods of normal science.⁽⁶⁾ Certain implications of this position have already been briefly mentioned above - it has been suggested that a paradigm should be seen as a set of norms. In brief, Kuhn

argues that a paradigm has four elements. First there are certain general rules, or law like statements. Secondly, and this is perhaps the most characteristic feature of the concept, there are certain exemplary applications of those rules. The rules are not, as it were, free floating. In each community they are embodied in routines or sets of actions which give them meaning.⁽⁷⁾ Thirdly, there are certain underlying models - perhaps, as Barnes suggests, metaphors⁽⁸⁾ - which help to guide the extension of the laws and methods to further exemplary applications. Finally, there are sets of values, which are often rather loose and vague.

Kuhn sees the paradigm as defining possible goals or objects which the scientist should attempt to achieve. It also legislates on permissible methods which may be used to achieve those goals. Kuhn uses a slightly different language. He talks about puzzles, and puzzle solutions, solutions which may be achieved by rule bound procedures. The puzzles are difficult, but it is thought, soluble. Kuhn writes that what challenges the scientist:

is the conviction that, if only he is skilful enough, he will succeed in solving a puzzle that no one before has solved or solved so well.⁽⁹⁾

How might Kuhn's analysis be applied to particle size analysis in sedimentology? Consider first, geology in general: here we find a very basic principle, that of uniformitarianism - the notion that the present is the key to the past, and that identical geological processes have been at work during the history of the earth so that whatever is visible in the geological record should be interpretable in terms of processes that are currently to be found at work. In and of itself, this does not constitute a guide to action, as it is far too general. It requires interpretation into action, and indeed it

has been so interpreted in a great many ways since Lyell's Geology. Rendered more specifically into the context of sedimentology, it might be restated : use contemporary phenomena of deposition to help determine the environment of deposition of consolidated sediments. Note that this too can be operationalised in a great many ways. One currently fashionable approach is that of "facies analysis" which involves study of the way in which typical sequences of sediments are laid down in particular environments. Another, and that with which I am particularly concerned, relates to particle size phenomena, and may be stated in the following way: if samples of particles are drawn from different kinds of sediments, and the size of those particles is plotted against their frequency of occurrence, then it may be that different sediments will reveal different size-frequency curves. If this can be established for modern deposits (for instance, for beach and dune sands, glacial deposits, etc.) then it may be possible to use typical size profiles as a diagnostic measure when studying ancient sediments.

In fact, this principle was formulated and developed primarily by two important American sedimentologists, Underhill and Warren.⁽¹⁰⁾ Underhill, the earlier of the two collected literally thousands of samples of sands and deposits from different environments, measured their size frequency distributions, and developed a set of rules - later felt to be inadequate - for distinguishing between the different environments in terms of such size frequency distributions. Between them they established the essentials of the method - the process of sieving, the scale - in fact logarithmic - to be used in calculation, and the graphic method of presenting data. This

basic work had been undertaken by the end of World War 1.

This achievement can be represented as a Kuhnian paradigm, albeit perhaps, rather a weak one. The general rules or law like statements were on the one hand rather vague such as: the sizes of grains deposited relates to the energy of the environment: and on the other hand they were specific, but rather tentative: that, for instance, glacial waters deposited sediments skewed with respect to size-frequency distribution. The exemplary applications were, of course, to be found in the literature, particularly in Underhill's extensive work. An important aspect of the applications were particularly clear instructions about methods to be employed. Indeed, it might be suggested that the heart of the programme lay in the methods rather than the findings⁽¹¹⁾ though this is not an issue that will be explored here.

It is less clear what we should count as being the underlying model, though I think it might be plausibly argued that it constitutes the basic notion that a sediment may be seen as a population of individuals that may be statistically distinguished from alternative populations derived from an alternative sediment. Likewise, it seems that the emphasis on quantification might also count as an example of the value component of the paradigm. In a more general sense we may note that the achievement of Underhill and Warren was paradigmatic in that it laid down goals and methods for achieving those goals, and was capable - indeed invited - further extension and application in the process that Kuhn calls articulation.

The Initial Difficulties

Sedimentology in the U.S.A. grew up coincidentally with the development of particle size analysis. At the time when Underhill was active in the field, (between 1885 and 1915) it was scarcely recognised as a special field in geology at all. In the early 1920s, for a complex of reasons, this situation began to change. In part the pressure came from petroleum companies and more particularly from geologists employed by the petroleum industry. This pressure was reflected in the formation of the American Society of Petroleum Geologists in 1917, and more specifically in the formation of the Society of Economic Palaeontologists and Mineralogists 1926, with the publication of the first issue of its Journal of Sedimentary Petrology in 1931. Again, pressure came from the National Research Council, which in the early twenties set up an ad hoc committee on sedimentation which reported in 1928.

Although intellectual developments took place in a number of areas of sedimentology - for instance the mineralogy of sedimentary rocks was a well developed area, as was the technique of thin section microscopy - in the 1920s and 1930s particle size analysis was one of the most important and promising of these. As a result both of this, and the gradual introduction of sedimentology into University Geology courses in the 1930s, an increasingly large amount of work was undertaken in the particle size area. A large number of younger scientists came into the area, and made what were taken to be significant contributions to the basic programme.

Indeed, for a period from the late 1930s (when a particularly significant textbook was written by two eminent leaders in the

field, King and Polworth) until perhaps the middle 1950s, it was probably the single most important technique used among American sedimentologists. As a rather qualitative, but none the less numerical indicator of its importance in the postwar period, I have done a rough content analysis on papers published in the Journal of Sedimentary Petrology.⁽¹²⁾ These figures suggest that in the years 1950 and 1951, over 40% of the papers published in the journal made major use of particle size analysis. This percentage had fallen to the low twenties by 1955 to 1956, and despite some divergence, do not seem to have risen significantly in the period 1960 - 1961.

These figures may be taken as a rough indicator of the slow development of a feeling in the 1950s that the method was not proving very satisfactory, and specifically was failing to predict environments of deposition when applied to consolidated sediments.

There is nothing very unusual about the development of such a feeling in an area of science. What is more important is the reaction of the community to such a development. Kuhn tells us that if a group of practitioners fails to solve the puzzles that it has set itself, then their first reaction is to blame themselves.⁽¹³⁾

Such a feeling of failure may be seen as a possible precursor to the situation described by Merton as anomie.⁽¹⁴⁾ It will be recalled that Merton distinguished between culturally prescribed goals, on the one hand, and culturally legitimate means for achieving those goals on the other. He writes that:

anomie is then conceived as a breakdown in the cultural structure, occurring particularly when there is an acute disjunction between the cultural norms and goals and the socially structured capacities of members of the group to act in accord with them. (15)

He indicates that anomie is particularly likely to result where emphasis is disproportionally placed on either goals or means, and it will be remembered that his analysis takes as an instance of this the claimed heavy emphasis of American culture on monetary success, with a correspondingly less heavy emphasis on the prescribed means for achieving this goal. (16) Of particular importance in the context of the current discussion is Merton's analysis of the ideology which is used to defend this system against the attack of those who claim it to be unjust. Thus, we find:

the deflection of criticism of the social structure onto one's self among those so situated in the society that they do not have full and equal access to opportunity; second, the preservation of a structure of social power by having individuals in the lower social strata identify themselves, not with their compeers, but with those at the top (whom they will ultimately join); and third, providing pressures for conformity with the cultural dictates of unslackened ambition by the threat of less than full membership in the society for those who fail to conform. (17)

In brief, I am proposing that the Mertonian analysis of the relationship between goals and institutionalised means, with the attendant strain towards anomie under certain circumstances, may be applied to the Kuhnian model of science, where goals (in this case environmental determination) and institutionalised means (particle size analysis) govern action. If the feeling that the permitted technical means do not achieve the scientific goals develops, then there is a strain towards the various modes of adaptation outlined by Merton. Such a strain doubtless results from the paucity of institutional rewards - if there is prolonged lack of success then prestige and honourific rewards become scarce. A general failure is possible, however, and would reveal one way in which the present situation differs from that analysed by Merton -

for here there is the possibility that no-one can achieve the goals by legitimate means, and all practitioners are in the same structural position vis à vis achievement of the goals.⁽¹⁸⁾

I suggested earlier that the empirical situation outlined may be represented as a precursor to Mertonian anomie. If anomie is cultural breakdown, then a possible precursor to anomie is a widespread belief among scientists that the scientific means that they are using are not achieving the goals that they have set themselves. For the purposes of exposition I shall call this subjective feeling First Order Anomie.

Responses to Initial Lack of Success

The question now arises - how do scientists respond to first order anomie? Our answer, which we may draw from either Merton's or Kuhn's analysis, may be summarised as: blame themselves, and try harder. Translated into the empirical area of particle size analysis, we find that there is increasing methodological innovation. Here we have to be careful, because Merton uses the term innovation to refer to rule breaking but (it is hoped) technically efficacious alterations to means - in other words cheating. Although it is possible that sedimentologists have on occasions cheated, the dominant response was that of technical, but conforming, improvement. For Kuhn, such behaviour would constitute a form of articulation.⁽¹⁹⁾

Empirically, we find that such methodological innovations⁽²⁰⁾, became common in the middle 1930s. I have already mentioned the textbook by King and Polworth, and one of my respondents suggested that:

with the advent of King's early papers, people became aware that these were problems that had been handled in a much more sophisticated way by statisticians for a long time. There had to be some adaptation of the standard statistical procedures to the study of grain size in particular (21)

Prior to the early 1930s there had been no significant use of conventional parametric statistics in the analysis of particle size data (or indeed in any area of geology). From 1935, certain obstacles to the introduction of such conventional statistics were overcome - specifically the difficulty of their application to interval data displayed on a logarithmic scale .

Following the literature through, one discovers that better and better empirical equivalents are calculated for such expressions as the standard deviation, kurtosis, and skewness. (22)

This, however, was only one of the ways in which attempts were made to improve methods. At the same time, debate developed about methods of measurement, and more specifically, about what was actually being measured. Up until the middle 1930s, measurement had been a predominantly empirical "art". After about 1936 (and the trend can again be very clearly seen in the textbook by King and Polworth (23)) serious attention was paid to the theory of measurement. Stokes Law was invoked in the attempt to analyse the characteristics of the various settling methods. Attempts were made to understand the far from intuitively obvious behaviour of particles in sieves. In addition serious attempts were made to assess the relationship between different methods of measurement. (24)

A third area of technical improvement lay in the development of sampling theory in its application to particle size analysis - work that is especially associated with the name of Ollman who invented the "sedimentation unit" which still constitutes one of the bases for

stratified sampling in particle size analysis.

For this empirical area, then, these were the first reactions of practitioners to first order anomie - to the sense that the goal was not being achieved by the culturally prescribed means. Such activity differs little from some aspects of the process of articulation in normal science described by Kuhn, although it is perhaps significant that retrospectively the decade of the thirties is seen more as one of technical advance than discovery of significant findings or establishment of sedimentological theory.

How should the response of the sedimentologists be categorised in terms of Merton's five modes of adaptation? There seems little doubt that it has, indeed, to be seen as a version of conformity. Merton, whose essays on anomie are primarily concerned with deviant modes of adaptation and their distribution, says relatively little about this mode, though he notes that it is the most widespread type of adaptation - and indeed must be if "society" is to exist. He also notes in passing, that the culture may be in a process of change.⁽²⁵⁾ This is a point to emphasise. In a number of manifest ways, which I have just outlined, the scientific subculture in particle size analysis was in a process of rapid, but conforming change. We must therefore be clear that cultural change may take place by specifying, or reinterpreting rules. It may also take place by inserting new packages of rules into gaps in the existing framework. There is thus no simple correspondence between deviance and change. The analogy with the case treated by Merton is clear. Desire for monetary reward may lead to conforming innovation - exploration of new markets, manufacture of novel products, better methods of accounting, and so on - without anyone blowing the whistle. There is

in other words nothing necessarily static about conformity.

Response to Continuing Difficulty

It is a matter of historical record that the practitioners in particle size analysis, despite their conforming technical developments, generally continued to feel that they were failing to achieve their goals. This had several consequences. One of these was the widespread reduction of interest in the area that I have previously mentioned. Another, and that which I shall discuss here, was that practitioners began to develop divergent lines of research. As one of my respondents put it:

If we had things neatly locked up like elementary algebra, we would not be developing a wide variety of approaches. (26)

A consequence of such divergence continued over a period of time without unambiguous achievement of goals was the development of arguments about the probable efficacy of technical means, and the significance of such results as were obtained. Thus, another of my interviewees suggested that:

Everybody is just arguing over how to do things; not only do they not know how to interpret them, but they are not agreed on how to analyse them and manipulate the data. (27)

Thus cultural breakdown, or to be more precise, cultural fragmentation, began to develop in the area of technical means. As an analytically separable, but empirically intertwined process, the goals of those working on particle size analysis were gradually altered in a number of directions. I will call this process of cultural fragmentation, engendered by prolonged first order anomie, second order anomie. Note that unlike first order anomie, second

order anomie is not a subjective phenomenon alone: it is objective, and refers to cultural fragmentation with the possibility of conflict.

An incidental consequence of such second order anomie is a problem for the historiographer, for it is no longer possible to present a single account of developments in the field which will be acceptable to all practitioners. This is a problem that has received some recent attention in the literature.⁽²⁸⁾ Broadly speaking the preferred approach is that the different positions taken by different practitioners be displayed as equally reasonable responses to a situation where unambiguous rules as to what constitutes good science have broken down. In the space available such an approach cannot be adopted. I will merely content myself with enumerating and briefly outlining each position.

The first approach taken involved the redefinition and further specification of the goal, with concomitant redefinition of the means. This involved making explicit the idea that the puzzle had not previously been properly defined; that, for instance, it had been set up as an empirical rather than a theoretical problem, and that corresponding difficulties in the interpretation of the data might be solved if theory was developed. This feeling expressed itself in a number of different ways. The first of these involved the development and application of multivariate techniques. One of the leaders of the field expressed the thinking behind the development in the following way:

I think one of the things has been an increasing recognition (that) .. all of the things that we think of as separate, or if you like as separately measurable attributes of sediments, are part of a complex, interrelated, multivariable system.⁽²⁹⁾

Such an approach really only became possible on a large scale in the middle 1950s with the development of the computer. The same respondent told me that:

there were a number of us who were prepared for the kind of mathematical computation that a computer does very quickly, but none of us wanted to invest that time in it, because it would be a whole week's work, and you would never know whether you happened to get a set of samples giving you results or not when you (did) .. it. Now with a computer, you collect your samples, throw them in a computer with data, and if it isn't good, you throw it in the waste basket - to hell with it. (30)

Although there is quite widespread agreement in the field that this approach marks a step forward, there is not complete consensus about this. Thus one sedimentologist who has done work of this sort, although he is now primarily concerned with developments in hydraulics (which I shall discuss below), commented:

I don't think we're going to get... very much further by straightforward application of multivariate statistics ... to various descriptive measures which we choose more or less arbitrarily. (31)

The same man felt that excessive enthusiasm with statistics might lead to goal displacement, with consequent failure to tackle geological problems. (32) This feeling was echoed by another respondent who said:

the farther we get away from the particles, the greater the likelihood of getting just caught up in the statistics themselves, or getting away from what we're really studying. I don't mean to knock statistics, but that is a danger. (33)

The man who made this comment was highly numerate. Certain workers on particle size analysis do not have a background in mathematics or statistics, so this line of work is simply not available to them. One man told me:

I feel sort of right in a cage because I don't know enough of physics to get out of the cage and of course, finally I don't know enough mathematics either. (34)

The second approach, while sometimes using multivariate statistics as a tool, emphasised that too little was known about the hydraulic mechanisms and physical conditions leading to the deposition of particles. Earlier studies in particle size analysis had attempted to seek out empirical regularities, but if progress was to be made it was necessary to ground the theory in appropriate, for instance hydraulic, models. One of the most eminent men in the field put it in this way:

We don't know enough about the underlying dynamics, and I don't believe that we can set up any very good theoretical models (about) .. what size should be telling us, and how to measure it, and so on, until we know more about the dynamics of sediment transportation and deposition. (35)

Another man put the same point in a somewhat different way:

..there must be some way in which we can relate a specific event to a specific sediment, and until we know that better than we know now, I don't think you're going to be much further along except by using a little better statistics than Underhill used. (36)

Similar sentiments were expressed by several other interviewees. (37)

If the development of multivariate analysis demands statistical expertise, the development of hydraulic models also demands skills that have not in the past been generally available to sedimentologists. The approach demands knowledge from a number of different sources - from areas of physics, perhaps from engineering - as well as mathematical knowledge. One of the interviewees suggested that the dynamics of sediment transportation and deposition constitute :

a field that I'm not sure geologists as such can handle. I don't think we've got enough mathematical background for it. (38)

Some sedimentologists have indeed made themselves experts on fluid dynamics, just as a few, belonging to an earlier generation have taught themselves multivariate techniques in statistics. However, it is fair to say that this approach is not one that is easily available to many working sedimentologists.

A third approach, which is not necessarily incompatible with the above, has led a few practitioners to argue that it is essential to sort out a variety of different mechanisms which lead to varying size distributions. Thus, it is argued that grain size reflects, not only the hydraulic conditions that exist at the point of deposition (although they undoubtedly do this in some measure), but also the sizes of crystalline components in the rock of origin, and the abrasion characteristics of the streams in which they have been transported. This approach stresses that each of these conditions may produce characteristic size distributions, and the discovered distribution is thus of complex origin. One of my respondents, who is well known for having developed work of this sort told me that:

The question is whether these apparent(ly).. logarithmically normal .. distributions.. (are) related to the process of transportation and deposition, or whether (they) .. are related to the process of abrasion, or inheritance of some sort from an earlier material. (39)

A fourth manner in which goal definition has taken place has been a concern with precision in measurement, sampling, and use of statistics. Such a concern has been quite general, and there has, for instance, been much work on the theory of measurement.

One writer in particular, however, is renowned for his attacks on the imprecision of most particle size work. This man, whose work is highly statistical, put it to me in this way:

The way I became more rigorous was because I started to get quantitative. As soon as I started to handle numbers, I had to be much more exact in what I was doing. I had to be careful about how I defined the variables I was measuring. I had to be careful about making the measurements so that they were meaningful ... (40)

In addition, he suggests, it is vital to know what you are looking for or trying to measure:

... I began to see that we had to know what the targets were, and there is no information in the literature ... on what a target is like, so that's like going to hunt for an elephant, but not knowing what an elephant is ... (41)

Other practitioners recognise the possible value in this approach, but sometimes find it irritating. Thus one sedimentologist, in the course of a discussion about sampling technique, told me:

At one extreme, however - .. facetiously I say this - ... is ... (Prof. X) at (Y.U.) (who) has spent essentially his career on figuring out how to take his first sample, and he hasn't taken it yet. ... He's really super critical .. - before he'll trust any data he wants to know what the variation between operators (is) and all the ramifications of it. ... really he's concerned more about the sampling technique than the results, and who's to say he's wrong, I mean, frankly? (42)

Another respondent described this man as one who carried "things to their ... logical absurdity". (43)

The four positions briefly outlined above may be seen as representing redefinition and further specification of the goal and technical means of particle size analysis. In addition to such redefinition, many writers, while not necessarily abandoning the

use of particle size analysis, have redefined its relevance.

Such redefinition of relevance thus constitutes a second general response to the continuing lack of success. To understand the rationale for this, it is important to remember that particle size analysis is only one of a number of different techniques for determining the environment of deposition of a sediment. Many of these alternatives were either unknown, or underdeveloped in the thirties, but were advanced greatly in the period 1935 - 1955, and became comparatively more attractive and potentially rewarding.⁽⁴⁴⁾ Thus, in the context of the changing intellectual structure of the discipline, it became possible to redefine the relevance of the technique, while still adhering to the most general overall goal - that of sedimentary environmental determination.

The first and most obvious way in which such redefinition has taken place is through the development of the view that particle size analysis is one, but only one, of the techniques to be used in the determination of environment of deposition. One of my interviewees, a leader in the field, put it to me in this way:

... if you try to estimate or try to reconstruct the environment under which you sediment was formed from its size properties alone, you aren't assured of very much success. You must look at the sizes of the associated sedimentary beds in a sequence so that in a way you've got to bring in the stratigraphic concept along with the texture...⁽⁴⁵⁾

Even so, there is a great deal of variation in the extent to which particle size analysis is felt to have a role to play. There is widespread agreement that it is a technique difficult to apply to consolidated sediments, so those whose primary interest is in the geological record rather than processes of deposition tend to

avoid its use. Another leader in the field, who has done extensive work on Precambrian sediments, and written several highly influential textbooks told me:

.. you always come up against this stone wall. That is very fine. You can use sieves and do this very careful screening and arrive at these results, but what am I going to do with this rock formation out here that holds up a mountain? Its so hard that you can't even hardly break off a piece to bring home, let alone determine the grain size. So the technique... began to break down when you wanted to go to the lithified (sediments). (46)

A further quotation from another respondent displays the variety of positions on this issue quite succinctly:

.. if the average sedimentologist is confronted with, say, making an interpretation of a sandstone, most people would use everything else possible before he'd use grain size. Now for an ancient rock... that's true. Now for a modern sediment it depends on a person's, I guess, feeling.... For example, Prof. X would, he'd want to see the grain size analysis as one of the, say, two most important aspects. Most people would say "No. I'll put it down on the list as sixth or seventh". But for ancient rocks very few people would ... put much (or) any reliance on the grain size data. (47)

Another worker, involved in the study of offshore processes of sediment transport made it clear how this has worked out in practice for at least one laboratory:

.. we used to have laboratories devoted just to grain size analysis, but now they've all been converted over to using S(canning) E(lectron) M(icroscopy), ... microchemical analysis of sediments, X-ray diffraction, X-ray fluorescence. (48)

In this laboratory, they still do particle size analysis as a matter of routine, but it is only one of a wide battery of techniques. For those concerned with indurated sediments, frequently a qualitative rather than quantitative estimate of particle size is made - but this, as one of my respondents pointed

out, is a technique as old as the first distinction between shale and sandstone, ⁽⁴⁹⁾ and can hardly be seen as an aspect of particle size analysis of the kind we have been discussing here.

In this way, particle size analysis has become defined by sedimentologists as one of a variety of methods which may be used, preferably as a battery, to determine the environment of deposition. There are two more specific ways in which the relevance of the technique has been redefined. Many practitioners agree that the study of maximum pebble size reveals the direction of flow of an ancient stream. One respondent explained the approach to me like this:

... as a gravel is transported by streams ... the size of the fragments tend to diminish as you go downstream. Whatever the reason might be, its an observed fact. So if you look at an ancient conglomerate, and can see outcroppings of it in various places, if you observe a gradual diminution in pebble size, you will infer from that the direction of transport of the material, and you will know that in the up-current direction you are approaching the source land from which it came. ⁽⁵⁰⁾

This is a useful technique in the study of palaeogeography, but involves measuring only the very largest indurated pebbles in each outcrop - not a complete size analysis.

Another approach, advocated by one of the interviewees, was to use particle size analysis in the study of tectonic events. His line of argument may be summarised as follows: it is now fairly clear that the method does not on its own permit us unambiguous determination of the environment of deposition. It is not, however, clear that it could not be profitably used in the study of tectonic events. This is because coarse sediments are typically deposited in high energy environments which themselves

are usually created as a result of tectonic uplift. Again, well sorted sediments usually reflect a relatively high level of reworking and burial - more likely associated with tectonic stability. This respondent felt that in their concern with local environments, many other sedimentologists were failing to study the large scale tectonic events which form the more fundamental background to such local environmental conditions. Furthermore, particle size analysis was an ideal tool to be used in the development of such work. (51)

I have now discussed those who redefined the goal and techniques in such a way that the problem of particle size analysis was specified in a somewhat different way. In addition, immediately above, I have discussed those who redefined the relevance of the technique in such a way that either its uses were altered, or its importance downgraded to (or below) the level of sundry other techniques of environmental determination. It is now necessary to consider a third category - those who have continued to work on particle size analysis without such redefinition - those who have, in other words, continued the tradition of conformity that I discussed earlier. Here one must distinguish between those who are acknowledged by others to be first rate geologists, important although perhaps mistaken contributors to the field, and those who are perceived as ritualistically cranking the handle without making a significant intellectual contribution.

Consider the former first. In my sample of eighteen American sedimentologists, there were two outstanding men who may be counted as current advocates of particle size analysis. Both of these have

made important contributions to more than one area in sedimentology. One has been President of two of the relevant professional associations. The other is widely felt to be one of the two or three most outstanding sedimentologists working in North America today. The latter, who has published several exemplary and relatively recent studies in the area described himself to me as a "strong advocate"⁽⁵²⁾ of particle size analysis, and later said of it:

I think its been a strong contributing tool to interpretation of sedimentary rocks, and its one of the many methods we have to use, and I think its a useful method when its used the right way.⁽⁵³⁾

The former, who has developed a new method to distinguish sedimentary environments by means of scatter diagrams which array pairs of statistical attributes took very much the same line in interview.⁽⁵⁴⁾ They are both very much aware of the fact that their faith in particle size analysis is not widely shared. One presented a paper on the subject in the early sixties which went down very badly. I asked him whether anyone, at the very large conference meeting had been persuaded by his presentation. His answer:

No one. It was a pretty lonely place at the time. No one was persuaded. ... Yeah. It was pretty lonely. You looked pretty damn foolish. You really did ... - in a sense it was a shock.⁽⁵⁵⁾

The other of these two respondents said of the method:

Many people got disappointed, and so this left a large group of people who scorned it completely, as being totally useless. In fact lets say three or four people in the department consider it a totally useless waste of time. So those of us who are working on it have had to become ... on the defensive about it.⁽⁵⁶⁾

These two writers, plus several others who have recently become well known for the development of new methods in particle size analysis must be distinguished from those who have continued to produce papers in the area in a ritualistic manner. Many of my respondents were agreed that there was a good deal of rather mindless utilisation of the technique. The same sedimentologist said of such scientists located in the petroleum companies:

They just produce great orders of data for the purpose of filling files and showing the boss that they are being busy, and this is my criticism of a lot of grain size work - it simply does that. (57)

He felt that only twenty to thirty per cent of the particle size analysis currently being published was of any real interest.

Another respondent, this time not someone whose name has been associated with the recent development of the technique, said of the approach:

I think its ... hopeless, but this is what people have been doing anyway, and it gives you a lot of numbers which - I'm perhaps being a little facetious here - but it gives you some sort of impressive tables to make up... (58)

Yet another respondent suggested that it was a good, quick, and easy way to get a publication, (59) which is perhaps now further from the truth than it would have been a decade ago. (60)

A final category which must be discussed includes those scientists who have in one way or another migrated out of the particle size area - although many of these have retained some measure of interest in the development of the field. Such migrants may be seen as falling into four categories. First, there are those who are primarily committed to the exploitation of certain techniques and

skills, and who seek to exploit these wherever they may be profitably used. The mathematically and statistically competent form the clearest cut cases of such mobility. One such scientist, talking of his career, said:

it focussed (on) ... the unifying thread of obtaining numbers, and then knowing what the hell to do with those numbers in order to get them into a shape where they can be presented, displayed if you like, where they can be then discussed, interpreted, and generalisations arrived therefrom. (61)

The interests of this man have led him to make important statistical and mathematical contributions to a number of other fields in geology. Another man, whose concerns have also been primarily statistical, was described to me by one of my respondents as having moved "totally away from geology into statistics per se", and the application of such statistics to economics. (62)

In addition to those who have moved away from particle size analysis in order to develop and exploit a skill, there are those who have retained a commitment to a particular aspect of geology for which the technique no longer seems relevant. I discussed one such commitment earlier - the attachment to indurated rocks rather than contemporary processes. (63) Those who are, for instance concerned with the study of lithified precambrian sediments will find particle size analysis less attractive than those who study the characteristics of current day beaches.

A third category of migrants are those who have become interested in a different area of geology which has no real connection with particle size analysis. One of my respondents, for instance, who had done extensive work on particle size analysis at a somewhat

earlier stage in his career had, when I spoke to him, ceased work in the area, and was doing work on the rate of growth of the continents. This man did not abandon particle size analysis because he felt that it was a dead end:

I don't think (I moved on because there was) ... any lack of interest in the topic, or any feeling that it had played out, or anything of that sort, because there are obviously still very fascinating things to do. (64)

His shift of interest coincided with the arrival in his Department of several new faculty members who were interested in sedimentology, and consequently relieved him of much of his previous teaching in that area. (65)

There is a fourth category of scientists who have moved to new areas that may be called pragmatic migrants - those who have moved out of the area for essentially non-scientific reasons. Thus, there are those who work for large organisations - business or government - where organisational objectives have been altered. In my sample there was one man who worked for the U.S. Geological Survey who had moved on to other work for this reason. (66) There are those who have been promoted within organisations where such promotion has led to administrative rather than scientific responsibilities. There were two such scientists in my sample, one in a University, and one in an independent research institution. (67) Finally there are those who have been unable to obtain positions which permitted the development of research interests. I spoke to one such man, now an independent geological consultant, who is widely recognised as having made really major contributions to the field in the 1930s. (68)

Returning now to the general, if the initial response to first order anomie is the development of what I have called conforming innovation, then the various responses to first order anomie (which together go to make up the cultural fragmentation I have called second order anomie) can also be arrayed in terms of Merton's modes of adaptation. Some of those who attempt to redefine goals and means, or indeed the relevance of such goals, must be seen as rebels. These are scientists who have rejected existing goals and standards, fail to recognise their legitimacy, and have established their own alternatives. Others would perhaps be more accurately displayed as reformers who may be defined as those who have replaced or supplemented the goals and methods with closely related alternatives. There is thus no outright rejection as in the case of rebellion yet neither is there complete withdrawal as in the case of ritualism. Those who continue to develop particle size analysis are clearly innovatory conformists, whilst those who crank the handle may, perhaps, be seen as ritualists, though we need to know more about the extent to which they receive honourific rewards before we can confirm this diagnosis. Finally, those who have migrated to a new area, may perhaps, be seen as retreatists, although there must be severe doubts about this designation, for Merton depicts it as a simple rejection of dominant goals and norms, a rejection of society, without any alternative commitments to put in their place. Such a description may perhaps be analytically appropriate for those who have withdrawn from science and its reward systems altogether into (for instance, industry). It certainly will not do for those who have simply migrated elsewhere

in science - which term, of course, suggests that we now require a category for migrants.

Notes on Anomie and Normal Science

This paper represents research in progress, and the theoretical form that its conclusions should take is still very much open to question. Broadly, I take the view that the concept of anomie in the sense used by Merton is a useful orienting concept. It allows us with some modification to organise a description and analysis of the rather diffuse development of ideas that has occurred in this particular intellectual tradition. Merton it will be recalled used the concept to show that certain societies have a tendency to produce deviant behaviour⁽⁶⁹⁾ which is thus amenable to a distinctively sociological analysis. He also used it to suggest the likely form that deviance might take for social groups who experienced different socialisation and differential access to the means of legitimate goal attainment. He finally used the approach to show that patterns of behaviour dysfunctional to one social system might lead to social change instrumental to the development of an alternative social system, and, therefore, that middle range functionalism was an approach capable of dealing with social change.

This paper uses the Mertonian categories in a quite different context for we now take for granted that social structures can produce patterned deviance. In addition, unlike Merton, I have not here presented data that would allow us to explain the different modes of adaptation taken by the different groups that I described above. It should also be clear that there are also important disanalogies between the situation analysed by Merton and that here.

First, Merton's analysis is designed for the situation where there is a single paramount preferred goal in the culture.

I have suggested that such a goal - the determination of sedimentary environment - is to be found in this instance, but I readily concede that it may be felt that there was commitment to a plurality of goals at several levels. This, perhaps, raises the more fundamental question about how the analyst should attempt to determine the goals of the individual or group - whether, indeed, it is ever possible to identify a paramount cultural goal. Secondly, Merton's analysis in part uses an analytical distinction between culture and structure which I have not made here. Thirdly I have suggested that in this case there is no very obvious way in which access to the means of achieving those goals was differentiated structurally between the different groups examined,⁽⁷⁰⁾ a point which was of course crucial to Merton's approach.

Having entered these caveats and disclaimers, the following observations are in order.

- (1) Use of the notion of anomie does not in and of itself allow us to account for the form or content of the various adaptations. Indeed, it was not designed so to do. Much more detailed work on culture, reference groups and metaphorical transfer is thus required if this job is to be completed.
- (2) We must in future be clear that under certain empirical circumstances anomie is a phenomenon limited to certain narrow fields. This is implicit in Merton's formulation, but it should be made explicit. These sedimentologists did not experience or suffer from anomie in some overwhelming Durkheimian sense. The fragmentation arose in a particular narrow role segment. Other segments were not

disorganised in the same way, and indeed were used as resources in attempts to reorganise the anomie area.

- (3) It is important to be clear how and by what mechanism the goals and means are enforced. Merton's analysis implied that culture is transmitted widely in a process over which actors have little control. Goals and means are thus a "fact of life" for many incumbents of a social system, a constitutive part of that system, unless (a) they are altered by successful rebellion or (b) rejected in a rebellious subculture.⁽⁷¹⁾ He notes in addition that some social groups are less exposed to such cultural values than others - perhaps, for instance, lower status groups develop alternative, not necessarily deviant, subcultures⁽⁷²⁾ which insulate their members from the general cultural imperatives. In the case examined here, however, the goals and institutionalised means are predominantly built up and sustained within the specialty. There is in other words, no external elite or ruling group which stabilises and sustains them, no group in whose interests it is to perpetuate them. This has another consequence:
- (4) which is that to speak of deviance in a case like this is unhelpful. Rules, means, and goals are sustained by action and affirmation. They may also be undermined by action, just as they may be undermined by inaction. I suggest that we are not so much looking at deviant behaviour as at negotiating behaviour. There are continuing negotiations (which given limitations of space I have been unable to detail) about what rules are to be followed, and what action should count as following those rules. Given the current fragmented state of the field,

the outcome of those negotiations is inconclusive at the present time.

- (5) The fifth conclusion to be drawn refers to the modes of adaptation. It is merely necessary to say here that in different empirical cases it may be appropriate to designate alternatives which differ from those outlined by Merton.

I have added two categories: those of reformer and migrant. Doubtless there are others. It is also important to note, as I have already done, that confirmity does not necessarily imply stagnation.

Finally and briefly, there are three points to be made about normal science in the context of the present discussion:

- (1) The development of a puzzle-solving tradition in science is a highly contingent outcome. "Normal science" is the possible outcome of a set of perilous negotiations about the value of goals, and techniques to achieve those goals. In other words it can be seen as a constant struggle to avoid lapse into anomie - a struggle greatly aided by an effective programme of shared professional socialisation, but one not necessarily thereby won.
- (2) The mechanical notion that there is a necessary progression inside any branch of science through paradigm development-articulation-anomaly-extraordinary research-crisis-revolution, is misplaced. Of course, Kuhn does not actually assert this. He notes carefully⁽⁷³⁾ that crisis may be resolved in one of three ways: by the assimilation of anomaly to the existing paradigm; by the conclusion that there is no solution either within the ambit of the current, or any other alternative conceivable paradigm; and only thirdly, by the development of a new paradigm and the consequential scientific revolution.

The present study suggests that a puzzle solving tradition may fragment because of simple failure to develop agreed puzzle solutions. Thus, there is no basic novelty-anomaly exists only in a most attenuated sense.

It may be argued against this that the example studied does not constitute a paradigm or a puzzle solving tradition. While there is perhaps some measure of truth in this,⁽⁷⁴⁾ this in no way undermines my fundamental point: that unified scientific practice is a contingent outcome, and that a mechanical reading of Kuhn's text is neither warranted nor acceptable.

(3) Finally, let me draw attention to the striking similarity between Kuhn's notion of crisis, or extraordinary science, and what I have called here second order anomie. Kuhn writes that all crises

begin with the blurring of a paradigm and the consequent loosening of the rules for normal research⁽⁷⁵⁾

A proliferation of possible paradigmatic articulations develops⁽⁷⁶⁾ with scientists offering partial, often incompatible and competing puzzle solutions. Notice that the development of what will become the new paradigm is not immediately recognisable as such to all the practitioners. Some are sooner or later persuaded. Others are never converted.⁽⁷⁷⁾

Kuhn writes at some length about why conclusive proof of paradigmatic superiority is not possible. His essential claim is that there is no such thing as a neutral observation language: the import of findings, methods, and theoretical expressions may all have been altered. It is not possible to discuss this in detail here. Let us simply note that Kuhn's description is appropriate in the case of particle size analysis: there is continual argument about the significance

of findings and their relationship to theory; there is argument about methods. By the time second order anomie has developed, views about the area are so divergent that new "facts" do not speak in the same way to different practitioners. While I would hesitate to call the situation under study one of extraordinary science - for there is no clear anomaly unless the failure to puzzle solve is an anomaly - it is reasonable to suggest that extraordinary science might be seen as one particular form of scientific anomie.

One last, but basic point. Paradigm change involves a fundamental reorganisation of observation language. The case I have studied here like extraordinary science, involves failed negotiations about observation languages: about what are to be taken as data, and their significance. This suggests that anomie should be seen as referring, not simply to normative breakdown, but also to the collapse of interpretive consensus. It is possible, after all, to fight over the interpretation of rules, as fundamentalists of all persuasions know well. If my suggestion is correct, then further elaboration of anomie theory is both possible and desirable. It is, for instance, possible to understand the work of the ethnomethodologists as the study of members' methods for avoiding interpretive anomie. A profitable link between macro and micro-sociology is thereby opened up.

Footnotes and References

1. EDGE, D.O. & MULKAY M.J., "Astronomy Transformed: the Emergence of Radioastronomy in Britain, Wiley, N.Y., 1976; MULLINS N.C., "The Development of a Scientific Specialty: the Phage Group and the Origins of Molecular Biology", Minerva, 10 (1972), 51 - 82; LAW J., "The Development of Specialties in Science: the Case of Protein X-ray Crystallography", Science Studies, 3 (1973), 275 - 303.
2. MERTON R.K., Social Theory and Social Structure, F.P., 1957; Sections IV & V.
3. HAGSTROM W.O., The Scientific Community, Basic Books, N.Y., 1965; DE SOLLA PRICE D.J., Little Science, Big Science. Columbia, 1963.
4. LAW J., "Theory and Methods in the Sociology of Science: an Interpretive Approach", Social Science Information, 13 (1974), 163 - 172.
5. See MERTON R.K., "Priorities in Scientific Discovery" in MERTON R.K., The Sociology of Science, Chicago, 1973; HAGSTROM W.O., op.cit., note 3.
6. KUHN T.S., The Structure of Scientific Revolutions, Chicago, 1970.
7. Indeed, Kuhn suggests that in certain cases paradigms may be shared without agreement about rules: that paradigms have, as he puts it, "priority". This issue can be avoided in this empirical instance, because during much of the period under study there were explicit rules of scientific conduct. When breakdown actually occurs, it seems to be simply a definitional matter as to whether a paradigm should be said to exist or not.
8. BARNES S.B., Scientific Knowledge and Sociological Theory, R.K.P., 1974.
9. KUHN T.S., op.cit. note 6, p. 38.
10. For reasons of confidentiality, I am obliged to use pseudonyms and disguise the names of institutions throughout. I am obviously likewise unable to give bibliographic references.
11. See LAW J., op.cit., note 1, on technique based specialties.
12. TABLE: PARTICLE SIZE ANALYSIS IN THE JOURNAL OF SEDIMENTARY PETROLOGY

Type of paper	<u>1950</u>	<u>1951</u>	<u>1955</u>	<u>1956</u>	<u>1960</u>	<u>1961</u>
Substantive:						
(Use of grain size)						
Total	2	1	0	1	0	1
Major	1	1	1	0	0	2
Important	3	5	2	1	4	5
<u>Elubtotal</u>	<u>6</u>	<u>7</u>	<u>3</u>	<u>2</u>	<u>4</u>	<u>8</u>
Minor	2	1	2	1	7	6
None	9	13	17	17	27	28
<u>Subtotal</u>	<u>11</u>	<u>14</u>	<u>19</u>	<u>18</u>	<u>34</u>	<u>34</u>
Methods:						
(On grain size?)						
Yes	1	2	4	5	1	5
No	1	1	2	8	5	6

Theory:
(On Grain Size?)

Yes	3	2	0	2	1	4
No	0	0	4	2	3	3
Total Papers on Grain Size	10	11	7	9	6	17
Total Papers Published	22	26	32	37	48	60
Percentage of Papers on Grain Size	45	42	21	24	12.5	28

13. See, for instance KUHN T.S., op.cit., note 6, p.35.
14. See MERTON R.K., op.cit., note 2.
15. Ibid., p.162.
16. Ibid., p.139.
17. Ibid., p.139.
18. An analogous situation might arise in science where there was differential access to expensive but necessary equipment.
19. There is no space here to enter into a discussion of innovation and conformity. It may be noted that behaviour can be both novel and conformist. Although there are various ways in which which such a possibility may be conceptualised. A Wittgensteinian notion of rule following is convenient. See: LAW J. & BARNES S.B., "Areas of Ignorance in Normal Science: a Note on Mulkay's 'Three Models of Scientific Development'", Sociological Review, 24 (1976), 115 - 124; and S.B. BARNES & LAW J., "Whatever Should Be Done with Indexical Expressions?", Theory and Society, 3 (1976), 223 - 237.
20. For Ease of Exposition, I shall use the term in the more conventional non Mertonian manner.
21. See A.
22. See B.
23. See C.
24. See D.
25. See MERTON R.K. op.cit., note 2, p.141.
26. See E.
27. See F.
28. See WOOLGAR S., "Writing an Intellectual History of Scientific Development: the Use of Discovery Accounts". Social Studies of

29. See G.
30. See H.
31. See I.
32. See J.
33. See K.
34. See L.
35. See M.
36. See N.
37. See O.
38. See P.
39. See Q.
40. See R.
41. See S.
42. See T.
43. See U.
44. See MURKAY M.J., GILBERT G.N., & WOOLGAR S., "Problem areas Research Networks in Science", Sociology, 9 (1975), 187-193.
45. See V.
46. See W.
47. See X.
- See Y.
48. See Z.
49. See AA.
50. See AB.
- See AC.
51. See AD.
52. See AE.
53. See AF.
54. See AG.

57. See AH.
58. See AI.
59. See AJ.
60. One of my respondents -- an editor of a journal -- suggested that he would not now normally accept for publication papers that simply reported the results of particle size analysis. Even those papers that included particle size analysis would not typically report the data in detail. See AK.
61. See AL.
62. See AM.
63. I suspect that this is one manifestation of a deep dichotomy that exists in sedimentology between those whose primary interest lies in the structure of rocks, and those who are first and foremost concerned with the analysis of sedimentary processes. Of course, most sedimentologists would (rightly) claim that they are interested in both to some extent.
64. See AN.
65. See AO.
66. See AP.
67. See AQ.
68. See AR.
69. See MERTON R.K., op.cit., p. 121.
70. If the availability of certain skills and resources is excluded from consideration.
71. See MERTON R.K., op.cit., p. 191.
72. Ibid., pp. 170 - 176.
73. See KUHN T.S., op.cit., note 6, p.84.
74. See above, where I have noted that it is only a weak puzzle.
75. See KUHN T.S., op.cit., note 6, p.85.
76. Ibid., p. 71.