THE ROLE OF SCIENCE IN POLICY MAKING

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Doctor of Philosophy

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This thesis aims to consider the role played by science in policy making. Firstly, two decision models are considered, synoptic rationality which depends heavily on formal information and comprehensive planning, and disjointed incrementalism, under which decisions are made in a fragmented and remedial manner via the interaction of interested partisans and with little necessity for formal information.

Secondly, different descriptions of scientific activity are discussed and a broadly Kuhnian view of science is supported with what is regarded as a 'fact' being heavily influenced by social factors. It is suggested that scientific controversies are more likely to occur in policy related science but for reasons that are intrinsic to science rather than due to some correctable aberration. A number of case studies, including two 'in-depth' studies into maternal deprivation and the relationship between hyperactivity and food additives support this contention, and also show that whilst scientific findings can raise issues they cannot aid in the resolution of these as the synoptic model suggests that they should. Instead information supports and legitimates value based policy views, with actual policy decisions arrived at via negotiation and aiming at a balancing of partisan pressures, as suggested by the incremental model. Not only does information not aid the resolution of policy disputes it cannot do so. When policy is disputed, scientific findings are also likely to be disputed and further research merely attracts more highly destructive criticism. This is termed the over critical model. When policy is decided then there is reduced impetus to critically test scientific ideas-this is termed the under critical model. Both of these situations act to the detriment of science.

The main conclusion drawn is that the belief that science is essential to decision making is misleading and may serve to mask rather than illuminate areas of dispute.

Decision making, Scientific controversies, Hyperactivity, Maternal deprivation.

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INTRODUCTION

1. INTRODUCTION

It has long been said that knowledge is power[1] and, certainly, scientific knowledge is widely regarded as essential to the power of decision making. Public policy has been seen, in large part, to be technical in character and scientific expertise has been frequently sought or offered to policy makers. The implicit model of decision making assumed is one under which goals are clear and agreed upon, scientific inputs are straightforward and capable of only one interpretation and the combination of these yields policy choices which are obvious and unquestionable. Over the past twenty years or so this picture has been increasingly questioned from three directions. Firstly, students of decision making have questioned the utility of these policy models. Secondly, case studies of science in policy areas have not supported this rather simplistic view and thirdly, sociologists of science have questioned the idea that science provides objective truth. A major disadvantage of these criticisms is a lack of cross fertilization. Decision theorists have paid little attention to sociological studies and have continued to treat scientific knowledge as unproblematic, whilst studies of science (particularly scientific controversies) have been restricted to 'pure' sciences or have failed to consider the ramifications of their conclusions for decision making. Where policy related scientific controversies have been considered this has generally been from a narrow, positivist view, with controversies not being explained but rather explained away as due to some bias or political influence. The few studies where this has not been the case have been limited in scope and provided few examples.[2]

At this point the question must be asked - Why study scientific controversies? Surely these are atypical and unrepresentative of the vast majority of scientific work? There are several views on this. Firstly, and most simply, acceptance of the view that controversies are atypical, they arise due to some bias or failure of science. Secondly, controversies are very much more common than they seem to be on a general inspection. "Our sense that science is ruled by consensus is to some degree a distortion, produced by our own theories of the nature of science, in which consensus is important"[3] and, thirdly, accepting that controversies may be rare phenomina they are worth studying for the insights they provide into day to day science. The inception, maintenance and resolution of a controversy to create a new consensus is precisely the same process as the creation of consensus in less contentious areas of science. In a controversy however, there processes are far more visible, the 'black box' of science is opened.[4] Thus, the first reason for studying scientific controversies is for the potential insights they provide into scientific practice, to answer the question - are controversies aberrant and atypical or are they merely more visible examples of what happens in science at all times? This question leads us to the second reason for studying controversies - as a means of studying the functioning of decision making models. If controversies are a result of some bias or aberration then decision models which require large amounts of unproblematic information may be supportable. The answer to any informational problems will be to correct these aberrations. If, on the other hand, controversies arise from normal processes within science then this implies that all scientific knowledge is, in principle, controversial. If this is the case then decision models should be able to cope with possible dubious and changeable information and, thus, if a desire exists to utilise scientific inputs the answer is to modify the model rather than scientific practices. Next, from this question, stems a further issue - is science used simply as information or, as has been suggested, is one of it's major functions to act as a rationalisation for the values of those in power in society?[5] These issues will be explored in this thesis via a critical examination of selected decision models, a review of recent ideas in the sociology of science and in the light of these, case studies of policy related controversies. One potential drawback of this case study approach is the way that controversies have frequently been studied as 'one offs', leading to problems of comparability of case studies and the degree to which generalisations may be drawn from them.[6] Certainly, this lack of comparability may be a problem for those looking at microscopic factors driving and influencing controversies, but this should not affect the utility of the case studies to the broader approach taken here. As well as these overviews of controversy, two policy related scientific controversies have been studied in depth. These are the controversy over the existence and effects of maternal deprivation and the controversy over the relationship between hyperactivity and food additives.

The layout of the thesis is as follows:
Part I will be devoted to consideration of some models of decision making and will be followed by a review of the development of modern ideas in the sociology of science including a review of 'pure' science case studies. In Part II some of the problems of research in policy areas will be considered followed by case studies of policy related controversies and finally, in Part III the issues raised in Parts I and II will be drawn together.

The main theme that will be followed in this thesis is that, contrary to received wisdom, science is not enormously useful to policy making. At best it can raise issues but solutions will tend to be arrived at politically. Any attempts to steer scientific research to policy useful areas neither helps science nor serves policy. If this is the case then models of decision making are required which can utilise scientific knowledge when it is available but can also function in it's absence or when ideas change.

P A R T I

C H A P T E R 2

TWO MODELS OF POLICY MAKING

2. TWO MODELS OF POLICY MAKING

2.1 Introduction

In this chapter I will be concentrating on two models of decision and policy making, synoptic rationality and disjointed incrementalism which are at opposing ends of the 'rational idealist' spectrum.[1] These two are among the most widely considered models particularly in Britain and America. Other models, such as the Marxist and the anarchist, will not be considered here since they neither put forward as descriptions of the actual situations in Western Society, nor remotely likely to be adopted as methods of decision making in these societies.[2] Some reference will be made to case study material to illustrate particular points but, by and large, this material will be considered in later chapters.

2.2 The Synoptic Model

A variety of names have been applied to this model, most notably the utopian-rationalist model[3], the rational deductivist ideal[4] and the rational action model.[5] The model has been elaborated in various forms but is most succinctly expressed as five steps.[6]

1) A problem requiring action is identified and goals and values relating to the problem are classified.

2) All important possible ways of solving the problem are listed.

3) The important consequences of each strategy are listed, along with the probability of each occurring.

4) Consequences are compared with desired goals and objectives.

5) Finally, a policy or strategy is selected in which the consequences most closely match goals and objectives, or the problem is most nearly solved, or most benefit gained at least cost.

The model has been described as goal directed since goals are set, and ways and means considered afterward.[7] To carry this out two types of information are required. Firstly, the decision maker must have access to some set of values and preferences (to set goals and acceptable means) and, secondly, a source of objective information, usually supplied by expert advisers. (In this chapter the words expert, adviser and scientist will be used interchangeably).

One of the main appeals of the model is it's apparent openness in terms of both scientific and value inputs. These are assumed to be clearly separable with the scientific facts verifiable by anyone with the requisite skills and knowledge and the ranked values of the decision maker open to question and discussion. (The degree to which these two are in fact achievable is discussed below). It has been suggested by several authors that the above model is unrealistic and is, in fact, a 'straw man' set up by critics to be easily demolished.[8] A less demanding and, hence, possibly more operable model is that of bounded rationality.

2.3 Bounded Rationality

In many ways the bounded rationality (or satisficing) model is akin to incrementalism[9] (see below), but it is goal directed as is the 'fully' rational model.[10] The bounded model is best described by Simon and was developed in response to the weaknesses he perceived in the rational model, notably that it almost completely ignores human cognitive limits.[11] To follow the rational model the decision maker:

> "... would have to have a complete description of the consequences following from each alternative strategy and would have to compare these consequences. He would have to know, in every single respect, how the world would be changed by his behaving one way instead of another and he would have to follow the consequences of behaviour through unlimited stretches of time, unlimited reaches of space and unlimited sets of values. Under such conditions even an approach to rationality in real behaviour would be inconceivable."...[12]

This problem is compounded (if that is possible) by the fact that the synoptic model is an optimising strategy. Thus, even if the above is carried out, the process would have to continue indefinitely under the assumption that a 'better' alternative might exist.[13] It was these problems which lead Simon to propose the idea of bounded rationality. Under this model the decision maker deals with a highly simplified model of the world.[14] This simplification is inevitable because of mans limited abilities of comprehension, computation and prediction.[15] Within this simplified scheme the decision maker seeks not to optimise but to 'satisfice'-to achieve a solution that is satisfactory.[16] Any search activity begins with some satisfactory solution in mind. During this search "repeated failure to discover 'acceptable' alternatives leads to a re-definition of 'acceptable'."[17] Thus, if solutions are hard to come by, then the level of aspiration will tend to drop ensuring that some solution is found.[18] The utility of this approach is that decisions can be made in a realistic time scale but this is lessened by a lack of knowledge of how and when aspiration levels change. As March and Simon note "The proposition is weak, since it simply asserts that some search will occur before adjustment of aspirations."[19] Despite this problem the model is apparently seen as both a description of what does happen and also a prescription 'when solutions are hard to come by, lower your aspirations'. Before considering the criticisms applied to these models it is worth noting that, at least in some cases, attempts are made to apply these model. Thus, the discussion does not relate to some out-moded or hypothetical situation. The example is drawn from a recent work on decision making and environmental lead,[20] and consists of the knowledge considered necessary before decisions on safe levels and methods of control of lead may be made.[21] These information requirements consist of several pages of research involving short and long term studies in epidemiology, toxicology, pharmacology and similar areas. Collingridge and Douglas suggest that these requirements are such that the likelihood of making any decision is remote.[22] Some of the reasons for this are considered below.

2.4 Criticisms applied to Rational Models.

The criticisms considered here are, in the main, those applied to the 'full blown' synoptic model but, in most cases, they are equally applicable to the more limited bounded model. Two broad categories may be identified, relating to the suppliers of inputs to the decision making process, and to the inputs themselves.

Suppliers of Inputs As stated above, the synoptic model requires decision makers to supply values and goals and experts to provide objective information. Neither of these has escaped critical comment. Firstly, inputs from decision makers. Two questions have been posed - Can values be listed and ranked independently of goals and policies?, and are values likely to remain stable for the lifetime of any project or policy? For the rational model to be workable an affirmative answer is necessary to both of these questions.[23] On the listing of values, Lindblom claims that decision makers are unable to

> "... formulate the relevant values first and then choose among policies to achieve them [and so] must choose directly amoung alternative policies that offer different marginal combinations of values. Somewhat paradoxically, the only practical way to disclose ones relevant marginal values, even to oneself, is to describe the policy one chooses to achieve them."...[24]

To put this another way, ends and means cannot be considered in isolation.[25] This has obvious implications for the openness of synoptic decisions, since as Altshuler notes, "Unless he can rank alternatives [the decision maker] is forced to use intuition. The greater the proportion of intuition, the less possible it is for a decision maker to allay all suspicion that his personal preference ruled."[26] Finally, agreement on ends does not necessarily bring with it agreement in the means to those ends.[27] Thus, explicit statements of values, trade offs, etc., may give rise to fruitless and paralysing disputes over the mix of trade offs to achieve these ends. The second value related problem is the stability of values over the lifetime of any decision. Rational decisions tend to be 'one off' solutions. There is no necessity for any flexibility to be built in to any decision since, by definition, no decision is taken until some satisfactory or optimum solution is known. Thus, if values change, either much of the information gathered to the time of that value change may be irrelevant (since goals have changed), or the decision made yesterday is, in the light of today's values, a mistaken one. Collingridge discusses these issues using the example of the nuclear breeder reactor and notes that this technology is highly inflexible leading to a situation where little can be done to correct decisions made in error or to accommodate changes in scientific knowledge or values.[28] Clearly, any decisions which cannot allow for some changes of preference over time will be prone to a judgement of error at some stage.

The second set of inputs come from expert advisors. The assumption of the rational models is that of a 'democratic paradigm' where questions of policy and technical questions are clearly separable and experts only provide information relating to the latter.[29] If experts are unable to provide objective and unbiased information (or at least make any bias plain) then the adviser is a provider of values as well as facts and to a certain extent the model is undemocratic. Several authors have commented with concern on this issue, for example, Macrae has expressed concern over the development of a technocratic elite,[30] and Bickerstaffe and Pearce warn of the risk of the disenfranchisement of the technologically illiterate,[31] i.e.

they see an increasing tendency for decisions to be taken by the technologically sophisticated at the expense of the democratically elected. The prime reason for this increased role of science and expertise seems to lie in the belief that advice is objective and value neutral, but is it? There is increasing scepticism in this issue. The amount of information and analysis required by the synoptic view would seem to rule out objectivity, some selectivity is inevitable.[32] The reason for this is that "... the number of facts conceivably relevant to virtually any policy issue is infinite".[33] The bounded model fares little better in this issue due to the uncertainty as to where any bounds should be set. This selectivity occurs for reasons of factual logistics, but selection may also take place due to bias (conscious or otherwise). Kantrowitz, for example, considers it unlikely that the scientist can "... have deeply held moral and political views about a question and simultaneously maintain complete objectivity concerning it s scientific components."[34] On this view failures of objectivity are due to human frailty, science is objective when carried out correctly. (This view of science as objective truth is under increasing attack and will be discussed in the next chapter. For the moment however, to give the synoptic view the benefit of the doubt, science will be treated as, at least potentially, objective.) If this lack of objectivity is due, as Kantrowitz suggests, to moral and political views, perhaps the scientist could make clear any obvious biasing factors, such as political and religious affiliations, pressure group membership, etc. What is less clear is what one does with this information. Does one ignore the opinions of scientists admitting bias? Compare views of those with differing biases? Should disagreements between scientists be attributed to scientific or extra scientific causes? (Methods such as the 'Science Court' have been suggested as a means of resolving these issues. These will be considered in Chapter 5). So far I have concentrated on overt and obvious biases. Presumably more subtle influences may also exist which the adviser may be unaware of but which may affect any advice given.[35] Mazur has argued that the separation of fact and value in it s entirety is not required as long as the more obvious sources of bias are considered.[36] He maintains that firstly, subtle values which cannot be removed or detected are too subtle to affect political decisions and secondly, that less subtle but shared values will not affect the issues. This first point is contentious in that it is always likely to be possible to detect some biasing influence if one looks hard enough. Collingridge gives an example of such a situation where an author alleging scientific bias is accused of bias himself. As Collingridge says "All that is now needed is someone to investigate [the second critics'] background and so on and so on."[37] On the second issue of shared values, shared with whom? Presumably, scientists are more likely to share certain values with other scientists and professionals than with the general public. This Marxist type analysis does not depend on acceptance of all of Marx's ideas for it s force.[38] As Friedson suggests: "Professional 'knowledge' cannot be a guide for social policy if it is a creation of the profession itself, expressing the commitments and perception of a special occupational class rather than that of the public as a whole."[39]

These above are some of the problems relating to the providers of information for the synoptic model. Substantial problems may also occur in relation to the informational requirements themselves.

Information The potential problems in this area arise from several sources, most notably the problems of bounding the system, the costs of obtaining information and the time taken to obtain information. The fully rational model requires that all potentially relevant information be collected. This, presumably, implies that no boundaries are set and thus infinite information is collected at infinite cost over infinite time before any decision can be made - hardly a useful recipe for decision making! Bounded rationality attempts to reduce these informational requirements by taking into account such factors as human limitations on information processing,[40] time pressure[41] and resource limitations.[42] This is carried out via the mechanism of changing aspiration level, that is, the definition of a 'satisfactory' solution changes as these limits are approached. The actual specifics of the mechanism is less clearly defined. As noted above, it is suggested only that 'some search' will occur before aspirations are adjusted.[43] One possible influence on aspiration level is the marginal cost of information. Information is collected until the cost of each new piece of information is equal to the savings or benefit achieved by obtaining that information. Thus, the amount of search is determined by the expected payback. This strategy may be applicable to industrial organisations (where Simon's work was initially aimed) and to governmental decisions which may be clearly defined in advance, but many government decisions are 'messy', pay-backs are not known, solutions are not clear (if indeed solutions exist) and hence one may merely be left with the advice to carry out 'some search' and then adjust aspirations. On occasion Simon has tried to make matters clearer, for example, advocating a remedial strategy. That is, waiting until a problem emerges and then dealing with it promptly,[44] but this reduces the bounded model to a poorer version of incrementalism (see below) rather than salvaging the bounded model itself.

Two further points are worth making. Firstly, the synoptic and bounded models are heavily dependent on formal (usually meaning scientific) information. When the models were first bruited science was viewed as 'the truth' and scientific disagreements were seen as rare, correctable aberrations. This view has in recent times been questioned both with regard to the truth status of science and on the nature of scientific disputes. These will be considered in the following chapter. Secondly, some authors have questioned the desirability and, indeed, the rationality of the rational approach.[45] This issue is well brought out in studies of risk, where experts tend to treat situations with equal risk of fatality as equivalent, whereas the public apparently does not.[46] Presumably, the assessment of the experts is the 'rational' one but the public, by viewing risk differently, are indicating the importance of 'extra rational' factors.

Survival of Rational Models

Given these above criticisms, it might be wondered why the rational model has not been abandoned long ago. Several authors have considered the relationship between scientific information and politics and in their work is the implication that, whilst politicians do not follow the precepts of the rational model they find it convenient and useful to promulgate the idea that they do. That is, they express support for the idea of rationality rather than it s substance. On this view the main uses suggested for science are legitimation, 'technologising' and delay.

Legitimation Science acts as 'the politician's helper'.[47] It is utilised not as an indicator of policy options but as a buttress for previously determined political views.

> "Politicians want their experts to serve some political ideology. Therefore, they expect or hope the analyst will see to it that the facts and figures serve the ideology. If the facts do not fit, or if they are embarrassing, they should, at the very least, be discretely omitted."[48]

Advisers may be chosen in the knowledge that their previously expressed views will fit in with those of the people they are advising,[49] a process made easier when expert disagreement exists since "Clients take advantage of the distribution of schools of thought within specialities to find sympathetic candidate-experts, raise them up and inject them into the policy process."[50] A further aspect of this role is the 'de-legitimation' of opposition, either by finding fault with the opposing experts,[51] or by allowing politicians to dismiss criticism as "... 'purely political' implying that the analysis is not 'scientific' and that it s contents need not be taken seriously."[52]

Technologising This strategy involves making political choices look apolitical by re-defining them in a tehnical way. "Policy makers find that it is efficient and comfortable to define decisions as technical rather than political..."[53] so that "the issues are ... debated in terms of technological fact rather than ultimate preferences in policy."[54] This may take place in order to either avoid confronting certain issues such as conflicting values,[55] or to avoid or share responsibility for an unpopular decision,[56] where it serves as a legitimation tactic.

Delay This response has been described as 'study as an excuse for inaction'[57] and is effectively another tactic of legitimation. The policy makers' desire to take no action on an issue is legitimated and criticism mollified or deflected by the setting up of a study group or working party to look at the area in detail. The hope is that, by the time any report is produced, the pressure for action will have decreased. If it has not then a further tactic used is to keep the report confidential.[58]

To conclude this section it is worth considering the origins of the rational model. It has been suggested that it was 'pushed' by scientists after the First World War on the grounds that

> "... if science and scientists were to play a major part in American Society it was necessary that the values of science should be seen to correspond with those of society at large. These scientists formulated the ideology American democracy is the political version of the scientific method." [59]

This view is also taken by Price who suggests that science can "... help clarify our public values, define our policy options and assist responsible political leaders..."[60] Lindblom also supports the view that 'scientific' decision making is highly valued in Western Society but suggests that, in fact, attempts at synoptic decision making are more appropriate to centrally planned economies, with the more appropriate form of government for Western democracies being disjointed incrementalism.[61] It is to this model of decision making that I will now turn.

2.5 Disjointed Incrementalism

Incrementalism or 'muddling through' was developed mainly by Charles Lindblom and his co-workers as a response to the perceived normative and explanatory failures of the synoptic model. Earlier workers had approached the issue on similar lines, for example, Popper's 'piecemeal social engineering'[62] but it is in Lindblom's work that the model finds it's fullest articulation. The strategy is problem oriented[63] (rather than goal oriented as is the synoptic approach) and has been applied to both policy areas and budget setting.[64] It has been described as a mechanism for coping with problems which are too complex to solve.[65] The main elements of the incremental model are as follows:[66]

1) The only policies considered are those differing incrementally from present states and from each other. These decisions can, in large part, be informed by experience of past events. It is not necessary to compare different values explicitly, to say, for example,

that x units of liberty are worth y units of security. This weighting is made implicitly by the choice of incrementally differing policies.

2) Only a restricted number of the above policy alternatives are considered. Although only incremental changes are considered, there are, of course, an infinite number of these. These are reduced to more manageable proportions by such limiting factors as the 'lumping together' of similar policies, by dismissing obvious 'non starters' without analysis and by failures of imagination.

3) Only a restricted number of the consequences of any candidate policy are considered. Those not considered include not only those perceived unimportant (as would be ignored by bounded rationality) but also "... the uninteresting [to the analyst and sponsors] , the remote, the imponderable, the intangible and the poorly understood, no matter how important..."[67] Whilst this may entail ignoring important consequences, Braybrooke and Lindblom contend that it is better to do a 'good' job of a limited analysis than to do a bad job of everything attempted.[68]

4) Policy objectives are adjusted to feasible policy options. This may be self evident but is in contrast to the synoptic, goal directed strategy. The implication of this is that policy objectives are flexible rather than set. As means (resources, knowledge, values and priorities) change so do policies in an interactive and iterative fashion.

5) The adjustment of feasible options means that problems (as well as policies) are frequently reconstructed and restated. Policy 'redesign' is to be expected.

6) Analysis and evaluation is serial, proceeding via a series of steps. Problems are, in the main, not solved but are temporarily dealt with and returned to at a later stage when the need for further changes are perceived. In this serial treatment underlying continuities

may or may not be apparent, for example, a problem may be dealt with by 'more' of an earlier policy or by attack from a completely different angle as new knowledge, ideas and opportunities present themselves.

7) Analysis, evaluation and policy are remedial. Long term aspirations are not the dominant issue for the policy maker but rather immediate problems. Instead of moving towards some ideal, policy aims to move away from perceived ills. "Policy aims at suppressing vice even though virtue cannot be defined, let alone concretised as a goal."[69] An advantage of this remedial treatment is that agreement is more likely between disparate groups in what is 'bad' rather than on what is ideal. The utility of remedial decision making may be much enhanced by making highly corrigible decisions. These are decisions that, if mistaken, are easily corrected. For example, if a policy arrived at suppressing vice in fact encourages it, it is desirable that this failure is quickly discovered and that the policy can easily be reversed or modified. This strategy has been described by Collingridge as 'flexing'.[70] This involves trying for the best decision whilst accepting that the worst possible outcome might occur and being ready for that worst. The essence of these corrigible decisions lies in their flexibility (hence flexing). When a decision is taken, it is not seen as the definitive last word, preparation is made to monitor outcomes and to change rapidly to an alternative strategy if necessary. Collingridge contrasts this with the commonly used strategy of 'hedging'. This entails planning for the worst possible outcome which can be considered and basing decisions on the avoidance of that outcome. Little monitoring or flexibility is required in such situations since each decision has already made allowance for the worst forseable eventuality. This lack of monitoring and flexibility may, however, have serious drawbacks since any policy failures

are likely to be discovered late and any required changes in policy direction may be very difficult to achieve. It is worth emphasising here that incrementalism is a strategy which aims to avoid major errors. The penalty paid for this is that major policy 'advances' are also unlikely. The synoptic view is the antithesis of this since whilst major improvements are possible, the potential for major errors is also much enhanced. If incremental decisions result in a worsening of a current situation this is likely to be minor and since only a small change has occurred this is (or should be) easily correctable.

8) Analysis and evaluation is carried out in a fragmented manner at a large number of points in society. There is not a single decision maker (or agency) working on an issue but a large number of agencies and interests, each concentrating their efforts on the fact and value aspects of the issue with which they have particular concern. There is no central co-ordination but none-the-less this (unplanned) fragmentation is claimed to ensure that all important aspects of an issue are dealt with. Essential to the functioning of this aspect of the incremental model is partisan mutual adjustment.

Partisan Mutual Adjustment Partisan mutual adjustment (PMA) is the mechanism by which co-ordination of the fragmented incremental process is achieved. This adjustment has been called 'co-ordination without a co-ordinator'[71] and occurs between a plurality of groups and actors by means of bargains, negotiations and adaptations of policy and action. It may include elements of compromise, mutual aid ('you support my policy and I'll support yours'), compensation for agencies sacrificing some part of their interests and authoritative prescription when one agency or group is in a powerful position relative to another.[72] This method of arriving at policy decisions is an explicit acknowledgement of the fact that, regardless of their technical nature, most policy decisions are political. This is not to say that information and expertise do not play important roles in negotiation and debate, indeed, to support and legitimate a position, partisans may be encouraged to carry out research (which may modify the position taken) but 'facts' alone cannot settle evaluative issues and it is here that negotiation and adjustment between interested parties comes in. It is worth briefly digressing at this point to discuss the role of pressure groups.

Pressure Groups as Partisans[73] Under the synoptic model the role of the pressure group is, presumably, as in indicator to the decision maker of the correctness or otherwise of chosen goals and values. The only real influence which the pressure group can exert is the threat of lost votes at subsequent elections. For several reasons this threat is rather a hollow one. Firstly, pressure groups deal primarily with single issues whilst voting concerns a variety of issues. Dissatisfaction over a single issue is unlikely to give rise to major shifts of allegiance. Secondly, many pressure groups are too small to be influential and those which are not (for example trades unions) are likely to have members with varying political interests. Thirdly, interest groups leaders seek to achieve a lasting relationship with decision makers, consequently they cannot threaten decision makers and at the same time maintain this relationship.[74] It has been suggested that pressure groups may be a source of problems for a rational decision making process by providing a constituency for disagreeing experts,[75] and that by injecting ideology and politics into the process they make rational decision making difficult.[76] This emphasis on 'scientific' decision making means that, from the rationalist viewpoint, pressure groups are at best ineffective, and at worst a nuisance to decision making. For the incrementalist, pressure groups play a more positive role and it is claimed by Lindblom that "Pressure groups are ... major participants in partisan mutual adjustment."[77] This relationship is, however, not as straightforward as would first be thought, since to be a partisan in PMA requires not only the desire to participate but also some power in the process. In certain circumstances pressure groups will have that power, for example, they may boycott goods and services but, in general, power lies with those directly involved in formulating and operating policy. (I am here considering 'lawful' pressure rather than terrorism). If pressure groups do not have direct power and the threat of voting sanctions is, in general, an empty one (as noted above), then where does their influence lie? Two closely linked areas may be identified. Firstly, a pressure group attempts to gain publicity for a cause and place it on the political agenda. As Gustafsson and Richardson note, politicians are very concerned with agenda management,[78] that is, in influencing what issues are formally recognised as being worthy of discussion. If a pressure group can make a cause sufficiently visible then political consideration is likely to follow. Reeve suggests that in the controversy over the health effects of smoking in the late 1950's, one of the main battles was between the medical profession and the tobacco companies over whether or not smoking should be on the political agenda.[79] The second role of the pressure group is as information providers to those influential in the decision process.[80] It is likely that, unless a pressure group has very extreme or unusual views, one or more influential persons may be found who are sympathetic to those views. By providing information, the pressure group aids these influential individuals in fighting for their views, that is, in being a more effective partisan in attempts to sway those uncommitted in an issue.[81]

Thus, pressure groups do not, in general, have a direct role in PMA but act by providing support for the decision maker who has interests in common with them. These comments on interest groups lead to the first criticism which has been applied to the incremental model, that is, not all citizens have an equal voice.

2.6 Criticisms Applied to Incrementalism.

Partisanship and Power. Numerous authors have commented on the unequal distribution of power between different individuals in society.[82] For example, Forrester suggests that "Pluralistic assumptions of equality of effective voice ... seem significantly unrealistic."[83] On this view, incrementalism, via the mechanism of PMA, legitimises the power of certain sections of society at the expense of others. Margolis presents an example of this in a case study on American regulation of auto emissions. The main actors in the debate over regulatory standards were the government and the auto manufacturers, with little input from the general public. Thus, Margolis claims, important issues were left out of the debate.[84]

Lindblom is not unaware of this area of criticism and has offered several replies. Firstly, "It is not ... a persuasive objection to partisan mutual adjustment unless it can be shown that more centralized political decision making represents a fuller array of interests and does so more consistently with principles of democratic equality."[85] That is, it is not enough to show that PMA is not perfect, for any criticism to be telling a more adequate method must be available. Secondly, greater weight is given to elected authorities than to pressure groups.[86] Thus, PMA is not simply a free for all but is biased in favour of elected officials. Issue may be taken with methods of election and potential bias of government but any arguments here will equally apply to any decision making model rather than to PMA alone. Thirdly, and perhaps most importantly, the weight given to different actors in PMA are not 'God given' and immutable but can be changed.[87] An example of this change might be in the increase in environmental litigation in the United States following the 1970 National Environmental Protection Act.[88] Citizens going to court under the act are deemed to be acting in 'the public interest'. Of course, those resorting to these means are likely to be the more affluent and better educated and there is certainly resistance to these increases in public involvement,[89] but the arguments of unequal distribution of influence can only be applied to PMA as presently constituted rather than it s principle.

<u>The Conservative Nature of Incrementalism</u>. Several authors have commented upon this facet of incrementalism. These comments are epitomised by Dror[90] who claims that for the incremental model to be applicable, firstly, present policies must be broadly satisfactory so that marginal changes can bring about an acceptable rate of improvement in policy results. Secondly, there must be a high degree of continuity in the nature of problems and, thirdly, there must be a high degree of continuity of means in dealing with problems. He goes on to claim that the incremental model is an "... ideological reinforcement of the pre-inertia and anti-innovatory forces prevalent in all human organisations, administrative and policy making."[91] In reply to this, it is suggested that "... the incremental method may produce both large and small increments"[92] and Lindblom has commented that the incremental model may be less conservative than other models.

> "Logically speaking, one can make changes in the social structure as rapidly through a sequence of incremental steps as through drastic - hence less frequent - alterations. Psychologically and sociologically speaking, decision makers can, sometimes, bring themselves to make changes quickly and easily only because the changes are incremental and are not fraught with great risk of error or of political conflict."[93]

Elsewhere, Braybrooke and Lindblom ask rhetorically, "Is not the strategy prepared for a world of unremitting change...?"[94]

They note that whether rate and amount of change are regarded as large depend on the society in which that change is occurring. To a rapidly changing society incrementalism might seem slow, whilst to a stable society (such as those in the West) this change may be seen as 'rapid enough.'[95] The next series of criticisms relate to situations where, it is claimed, incrementalism cannot be applied.

Non Incremental Decisions. Two main areas have been cited where the incremental approach is inappropriate. These are in relation to fundamental decisions and non-fragmentable decisions. The importance of fundamental decisions has been noted by, amongst others, Etzioni who suggests that whilst incremental decisions are the most numerous class, their context is set by (less frequent) fundamental decisions,[96] and Dror suggests that "When there are no past policies in respect to a discrete policy issue incremental change is, in fact, impossible."[97] (See also below). In reply to this Collingridge and Douglas point out that incrementalism has never been claimed as a 'universal model' applicable to all decisions and for the point to be a telling one it would be necessary not only to show that there is a class of decisions to which incrementalism cannot be applied but that some other model can deal with these decisions effectively. Except for trivial examples this has not been done.[98] It is difficult to conceive of a strategy of decision making equally applicable to, say, declaring war and local traffic planning and, whilst the importance of fundamental decisions cannot be disputed, it is easy to concur with the view that these are outside the practice of 'standard' planning.[99] The argument that some decisions and policies are non-fragmentable is essentially the same as that over fundamental decisions. Schulman, in a case study of the growth and development of the North American Space Agency (N.A.S.A.) notes that the agency could not have developed via incremental policies because a large start up investment was required.[100] Thus, the decision was, in fact, a fundamental one and, hence, outside the claimed remit of the incremental strategy.

Incrementalism and Improvements Two similar issues may be noted under this heading. Firstly, that incremental change may not bring about any improvements and, secondly, that these changes may actually bring about a worsening of the situation. On this first point Etzioni claims that there is no way to guide the steps of incremental change "... the steps may be circular - leading back to where they started or dispersed - leading in many directions at once but leading nowhere."[101] This situation is described by Boulding in his oft quoted "... We do stagger through history like a drunk putting one disjointed incremental foot after another."[102] The essence of this criticism is, presumably, the difficulty of recognising whether or not an incremental change has brought about an improvement, though, since a policy change is brought about with some aim in view, some form of monitoring is likely to note the degree of achievement of that aim. Possibly, a more important problem relates to how long a decision maker should wait before deciding that a policy has not achieved the desired result.[103] However, since this criticism is equally applicable to any decision model, it cannot be regarded as a criticism of incrementalism per se.

On the second point, any worsening of the situation may be temporary or permanent. These temporary failures are noted by Etzioni (above) when he warns of the risk of circularity of incremental decisions. This argument hinges on the assumption that a decision maker, perceiving an erroneous policy decision, will attempt to return to square one. It seems far more likely that a further incremental change would be applied using knowledge gleaned from the current failure. Baram gives an example of a case where fragmented and incremental decisions might cause a permanent worsening of the situation.[104] It relates to the release of potentially carcinogenic substances in the environment. Each single use may add very little to total exposure levels but the summation of these may exceed safe exposure thresholds. This criticism, however, can only be applied to very high levels of fragmentation of authority. There is nothing in incremental theory that forbids a single body (or indeed several bodies) from overseeing an issue of this nature with a view to ensuring that the situation postulated by Baram does not arise. Thus, whilst it is undeniable that incremental fragmentation could give rise to such problems, there is nothing in the incremental strategy that makes these problems inevitable. Of importance here is the size of an incremental change. Braybrooke and Lindblom suggest that 'large' and 'small' are relative notions depending on the importance attached by different actors to different aims and values but that these ideas tend to converge due to broad agreement on what factors are, or are not, important, at any one time.[105] They describe a small change as "... a change in a relatively unimportant variable or [a] relatively unimportant change in an important variable."[106] To some extent this begs the question, in that, whilst there may be certain changes that are clearly incremental and other changes that are clearly non incremental, there is likely to be a 'grey' area between these. It is here, presumably, that PMA plays a role in attempting to modify or resist any change which is, for that particular partisan, non incremental.

Incrementalism and Information. Goodin and Waldner have criticised incrementalism as 'unreflective action'.[107] This criticism is contained in a paper discussing different views of incrementalism and is, apparently, a 'straw model' set up for easy demolition, since nowhere do Braybrooke and Lindblom deny that information should be used in decision making. They claim that the strategy is:

> "... a way of getting along without theory when necessary... [but this] ... should not be mistaken for an attack upon systematic formal theories in social science. The strategy is not a rival of scientific theories. It shows what to do when scientific theories are not available, yet effective use of information is required."[108]

It could even be claimed that the incremental model is particularly fitted to the utilisation of scientific inputs, since as Friedmann points out "... scientific knowledge, no less than the power to act, is fragmented and comes to us sequentially."[109] Thus, the strategy is able to use the information that is available rather than the information we would like to be available and further changes can take place as more (or better) information comes in. Two other informational criticisms which have been applied to the incremental strategy are, firstly, that fragmentation leads to duplication of effort and, secondly, that partisanship leads to secrecy.

Duplication of Effort. One of the claimed 'plusses' of incrementalism is that a problem naturally fragments as each partisan actor looks at their particular area of interest, thus ensuring that no important area is overlooked. It has, however, been suggested that this is only advantageous when organisations have a redundancy of resources and that, when this is not the case, there may be moves towards a more central overview[110] and Bozeman, et al, have suggested that public agencies be designed in order to optimise informational exchange.[111] Whilst it is undeniable that better communication of information would be desirable, it may also be said that if an area is important enough for two or more partisans to devote resources to it, then it is important enough that extra study may well be beneficial. Furthermore, since different partisans are likely to be supporting different values or aims, they will look at an area differently and combined research might miss out aspects important to individual values and aims. Thus, the possibility of duplication is undeniable but this is not automatically a bad thing.

Secrecy One potential problem of partisanship is that partisans 'fighting their corner' might fail to communicate relevant information to rivals if that information is inimical to the interests of that partisan.[112] Whilst this may occur, the duplication discussed above should overcome this, since as Lindblom points out, partisans are motivated to discover information to protect their own interests.[113] The discovery of information by one partisan, of information useful to a second partisan (which that partisan has missed), seems likely only if the second partisan group is neglecting it s interests or through an act of serendipity that no decision making model can either expect or allow for. Furthermore, as Benveniste notes "Since it is a political commodity, information can be exchanged ..."[114] and PMA with it s bargaining and negotiation may well encourage this exchange.

There, then, are the major criticisms which have been applied to the disjointed incrementalist model. Some authors have considered these to be severe enough to warrant the development of alternative models. Two of these will be briefly considered.

2.7 Alternative Models

Several 'rationalist' alternatives to the above models have been developed. The best developed of these are Etzioni's 'mixed scanning'[115] and Dror's 'normative optimum' model.[116]

Mixed Scanning Mixed scanning is effectively a combination of the synoptic and incrementalist views. It entails taking an overview at an all encompassing level (so that no major options are left uncovered), followed by selection of options to be studied at a more detailed level.[117] It is essential to the strategy to differentiate between fundamental decisions and lower level incremental ones.[118] The strategy has been summarised as follows.[119] Policymakers should carry out a comprehensive but general consideration of alternatives. Policy options are then selected for more detailed consideration on the basis of the values of the policy makers and which benefit groups with values differing from the policy maker when the policymaker is indifferent. Decision priorities are set by taking one single goal as paramount and decisions are evaluated by the degree to which the primary goal is achieved.[120] This (very brief) sketch of mixed scanning is sufficient to indicate some of it s problematic aspects. In several ways it is close to the synoptic model in it s assumption that decision makers have predictive powers that they do not, in reality, possess[121] and that some means of ranking values exists.[122] Collingridge and Douglas note that Etzioni ignores the fact that decisions are nested, that is, they may be fundamental from one point of view but incremental from another. They give the example of lead levels in petrol, which seem to have been set incrementally but are clearly fundamental from the point of view of petrol suppliers.[123] Finally, as Smith and May point out "The important point is that rationalism and incrementalism embody diametrically opposed principles which are not reconciled by 'mixed scannings' sampling of either side."[124]

<u>The Normative Optimum Model</u> This model, developed by Dror, has several similarities to mixed scanning. It s essential elements are as follows.[125] First, carry out some clarification of values, objectives and decision criteria. Second, identify alternatives and make a conscious effort to consider new and novel alternatives. Third, conduct a preliminary estimation of expected pay-off of various alternatives and decide whether a strategy of minimal risk or innovation is preferable. Fourth, these alternatives are compared. The test of an optimum policy is that it is agreed by various analysts after these steps have been carried out. The elements involved in alternative consideration include theory and

experience, rationality and extra rationality, intuition and creativity in various mixes. This emphasis on rationality and extra rationality has lead to the claim that "Taken together the assumptions and the elements of the model seem to call upon the decision maker to face in two directions at the same time,"[126] and factors such as 'intuition' and 'experience' have been described as "disconcertingly vague variables and hardly more than residual categories for non rational sources of information. The whole model borders on the tautologous with it s commitment to both rational and non rational elements."[127] Finally, Lindblom claims that Dror's model is "... simply a series of discrete observations and prescriptions on decision making which, taken as a group are not tightly interlocked and which, taken one by one, are not generally valid or acceptable."[128]

Overview of Alternatives. Arguably, these above models have not been done justice in these brief comments but neither have been extensively considered in the decision making literature which is perhaps a comment in itself. It is possible that given the amount of consideration and critical appraisal which have been applied to the synoptic model and incrementalism, they might emerge as viable alternatives but, as they stand, they (and bounded rationality) share several drawbacks. Firstly, where they approximate the synoptic model they share it s failings. Secondly, where they advocate incremental type strategies they become merely poor imitations, subtracting rather than adding to it s utility and, thirdly, where they differ from these above (for example Dror's intuition), they are woolly and poorly defined, providing little guidance for action. For these reasons, these models will not be considered further and discussion will concentrate on the synoptic and incremental models.

2.8 Discussion

It will have been noted that nowhere in this chapter have such techniques as cost-benefit analysis and systems analysis been considered. Certain authors have described these as 'rationalist' and, as such, they have been criticised in application.[129] However, neither these, nor similar techniques, are decision models in their own right, they are tools used in attempts to improve decision making and, as such, may be applied to both synoptic and incremental models, with their utility, or otherwise, determined to the extent that the models they are applied to are workable. This workability hinges on the informational demands of the different models and the uses to which that information is put. Clearly, the synoptic model can only function with vast amounts of information. This information serves to determine policy options within a framework of pre-set goals. The model may be described as 'informationally deterministic' in the sense that scientific and informational inputs are seen as objective and true and can thus legitimately be interpreted in only one way, with alternative interpretations explicable only in terms of bias, error or irrationality. Aside from the specific criticisms applied to the model in this chapter, the suggestion that information is used to legitimate as well as inform decision making implies that this model is significantly unrealistic. The incremental model explicitly recognises that information is used politically via the mechanism of partisan mutual adjustment. PMA recognises that for different actors and groups different policies are rational and that the role of scientific knowledge and information is to provide guidance, support and legitimation for policies arrived at politically. This formal information is regarded as useful rather than essential and is not seen as having superior importance to political beliefs. The suggestion that information may be used for legitimation or 'technologising' of political aims provides implicit support for the incremental model since this 'political' use of information is one of it s major roles in PMA, whilst for the synoptic model use of information in this explicitly political way can only be regarded as a failure of the model. Of the criticisms applied to incrementalism, those which are specific can be seen not to hold and those which are general are equally applicable to any decision model rather than to incrementalism alone.

In the following chapters I will be evaluating the degree to which these models are workable, especially under the telling conditions of scientific controversy. It has been suggested that comparisons of these two models are artificial since each contains different normative and explanatory content. Thus, the rational model is seen as an attempt to prescribe how decisions ought to be made whilst the incremental model is primarily descriptive.[130] Implicit in the prescription that decisions should be made in a synoptic manner is the assumption that decisions can be made in this way, that the model is applicable to 'real life' situations, whilst implicit in the categorisation of the incremental model as descriptive is that other decision models exist which could produce better decisions. Thus, dismissing any disagreements as artificial merely begs the question, or rather several questions, Firstly, Can decisions be made synoptically? Secondly, Are decisions made in the manner described by incrementalists and, if not, how are they made? and thirdly, in the light of these questions, What role does science play in the decision process? In the following chapters I will attempt to provide some answers to these questions.

CHAPTER 3

THEORIES OF SCIENTIFIC KNOWLEDGE

3. THEORIES OF SCIENTIFIC KNOWLEDGE

3.1 Introduction

In this chapter I will look at some of the sociological ideas which have been put forward about the practice of science and the implications of these ideas for the generation of scientific controversies. The ideas considered here may be divided into two categories. Firstly, scientific theories are accurate reflections of nature, they are 'isomorphic with reality' and secondly, scientific theories are changeable models, at any time they merely represent the best available description of reality. Thus, in the former case, there is an external authority (nature) by which theories may be judged and to which theories may be compared, whilst, in the latter case, theories are created rather than given by nature, with their authority stemming from the extent to which they are agreed upon. The potential for social influences exists in both of these categories but each will view this in a different way. For the 'scientific theories are truth' school, the influence of any outside factors can only bring bias and hence error into science. For the view that scientific theories are created, social influences are an inevitable part of these creative processes. These two views will be explained below and will be followed by a review of some case studies of scientific practice in what may be termed 'pure' or basic science, such as high energy physics where no clear or direct policy applications exist.[1] The utility of the case studies is in providing a control for the policy related controversies to be considered in later chapters. In these policy controversies it could easily be argued that extra-scientific influences will almost inevitably be found but, if social influences can also be discovered in what are nominally 'pure' sciences, then this will strongly suggest that social influences are an inevitable part of the scientific process rather than some pathological influence which may in some way be removed.

3.2 Scientific Knowledge as Truth

This view of science has been called the 'standard view'[2] and the 'storybook image of science.'[3] The scientist is seen as a dispassionate truth seeker engaged in value free observation to build laws descriptive of regularities in nature. The role of the scientist is basically that of a conduit for these observations which are repeatable at will. Thus, knowledge is shorn of all subjective factors and can be guaranteed with a high degree of confidence. Of course, not all knowledge is empirical, theories may exist prior to observation but these remain speculative until their truth content has been assessed by comparison with the 'real world'. On this view "... the social origin of scientific knowledge is almost completely irrelevant to it s content, for the latter is determined by the nature of the physical world itself."[4] A scientist is seen as having loyalty and interest only in the truth and this is such "... that he would rather cut off his right arm than suppress ... new data."[5] Whilst few philosophers and sociologists of science would support this view, these ideas are clearly still prevalent amongst the general public, politicians and, to a lesser extent, practicing scientists and they have certainly been very important in the past in shaping ideas about the limitations and capabilities of science. These 'popular' views of science will be considered through the work of the very influential sociologist Robert Merton. He put forward the idea that the activities of scientists are governed, primarily, by four institutional imperatives or norms - universalism, communism, disinteredness and organised scepticism, commonly known by the acronym CUDOS.[6] They are defined as follows:-

Universalism - only the scientific attributes of the contribution, rather than those of the contributor, should be used in judging any contribution - judgement criteria should be impersonal.[7]

Communism - science is common property, the only right of any discoverer is the right of recognition for having made the discovery.[8]

Disinteredness - the belief that the advancement of scientific knowledge is the prime goal of the scientist.[9]

Organised Scepticism - judgement is detached and is suspended until all the facts are in.[10]

More recently, other authors have extended this list of norms[11] but CUDOS have remained at the core. For Merton these norms are both prescriptive and descriptive, science is successful because these norms are adhered to and adherence to these norms is the best way to ensure success in science. At first sight little scope is left for controversy since judgments are formed dispassionately in terms of scientific truth content with "scientific consensus form[ing] automatically as scientists ally themselves to the theories that are demonstrably 'truer' than their rivals."[12] However, Merton had noted the existence of controversies throughout the history of science. He concluded that these were explicable as priority disputes. Since the aim of science is to produce new knowledge, originality is highly valued and thus, "Recognition for originality becomes socially validated testimony that one has successfully lived up to the most exacting requirements of one's role as a scientist."[13] Recognition here is seen as the currency by which scientists are paid for their original contributions to science, though Merton is at pains to point out that it is not egotism which leads to these disputes[14] but rather social pressures.[15] To explain the relative frequency of priority disputes Merton invoked the concept of multiple discovery, which is likely when researchers study the same area.[16] The risk that this pressure for originality might lead to plagiarism and fraud was also recognised by Merton[17] but the incidence of these (it is claimed) is small and is controlled by the moral integrity of the scientist and by the policing actions of organised scepticism and experimental replication. A second (potential) social influence on science stems from Merton's belief that academic freedom and autonomy are required for science to flourish (and related to this, the belief that science is at it's best in a democracy).[18] Thus, outside influences will warp the direction of scientific research,[19] possibly introducing "... partiality, self interest, intellectual prejudice and secrecy."[20] Under this formulation, controversies in policy related areas may be explained by political influences leading to 'bad' science potentially subject to biased findings, interpretations and conclusions. The relevance of these ideas (at least to modern science) seems questionable. It is germane that Merton's original paper on scientific norms was published in 1942[21] and drew on earlier historic data more relevant to Victorian 'little science' than to today's government funded research which must compete for central research funds with other 'big science' disciplines.[22] Thus, even if the Mertonian norms once held true, their modern utility and relevance is called into doubt. To give just one example of this, experimental replication depends on not only relevant experimental skills but also access to the requisite equipment. In today's 'big science' with it s many specialities this replication can be seen as an ideal rather than the reality.[23]

Attempts to test Merton's ideas have, in the main, come from case studies of scientific practice looking for evidence of the existence and influence of scientific norms. One of the best known of these was carried out by Mitroff who interviewed scientists concerned with lunar geology at the time of the Apollo moon landings.[24] Two main conclusions emerge from his work. Firstly, none of the scientists interviewed believed that their work was governed by the Mertonian norms and, secondly, the norms were rejected as a prescriptive ideal. Instead, strong commitment to different hypotheses was suggested as being an essential sustaining force in the development of scientific theories.[25] These results led Mitroff to postulate complementary pairs of norms and counter-norms[26] (which Merton had also considered in earlier work where he suggested that the influence of these pairings produced an 'ambivalence' in scientists.)[27] These counter-norms included secrecy and emotional commitment and Mitroff further suggested that science, as we know it, could not have arisen if it were governed only by the norms of 'CUDOS'.[28] On first inspection this argument for counter-norms seems plausible but, rather than supporting the existence of counter-norms, it would seem to demolish the support for the original norms. If any behaviour can be explained on a continuum from, for example, 'maintain secrecy' to 'share knowledge' then the existence, or at least the relevance of these norms, must be called into question. Whilst a unitary set of norms is insufficient to explain scientists' activities, a complementary set can explain all possible behaviours but for this very reason, has little explanatory utility. Some authors have suggested that Merton's evidence for these norms was adduced from practicing scientists[29] who use the postulated norms as rhetorical devices. These "... provide a repetoire or vocabulary which scientists can use flexibly to categorise professional actions differently in various social contexts."[30] Studies in both astronomy[31] and biochemistry[32] support this view and on the grounds of this Mulkay has suggested that the norms can best be viewed as an ideology[33] consisting of a flexibly used vocabulary rather than a series of social obligations.[34]

Another approach to the study of Merton's norms has been via the 'collection' of situations where they can be seen not to hold. One of the most comprehensive of these was carried out by Mahoney where he questions the existence of scientific objectivity, rationality, integrity and communality amongst others.[35] On open-mindedness and objectivity Mahoney considers experimental replication and notes that experiments carried out by scientists with shared biases may merely replicate biased results and he also questions the extent to which experimental replications are actually carried out.[36] It might be argued that experiments need only be replicable in principle but, if this is the case, replication would seem to be more of a remedy for fraud than error. This apparent lack of objectivity in science has led one author to suggest that if it is genuinely desired to inculcate a norm of objectivity in students then 'the history of science should be rated X.'[37] Mahoney also casts doubt on the open mindedness of the scientist and certainly the history of science is replete with examples of scientists resisting new ideas.[38] On non-open-mindedness Mahoney quotes Einstein who is reported to have said that he would reject discrepant data rather than reject relativity.[39] One of the most public examples of scientific non open-mindedness was the 'Velikovsky affair'. In 1950 Velikovsky published a book 'Worlds in Collision'[40] which called into question many of the central tenets of physics, geology and historical biology. The furore which this book created has been extensively considered elsewhere[41] but briefly the book was denounced by scientists who had not read it, the publishing company was threatened with an academic boycott and scientific tests of the ideas were refused. It is not my intention to argue how those perceived as 'cranks' should be treated but clearly the norms of science did not operate in this case. If it is suggested that the norm of organised scepticism did not operate and should have, then it's force, if it can be ignored at will, is called into question. If, on the other hand, it is argued that the norm did not operate and should not have then it becomes a device to be deployed when required rather than any sort of governor of scientific activity.

On the question of scientific integrity it will be remembered from earlier in this chapter that the scientist would rather cut off his right arm than suppress data. Presumably, even more heinous is the invention of data but examples of this abound[42], from Newton, who has been described as 'the master of the fudge factor,[43] and guilty of deliberate fraud,[44] up to the present day with a 1985 meeting of the American Association for the Advancement of Science reporting that on average two accusations of major scientific fraud are made each month.[45] Even assuming that not all of these are upheld the report would seem to indicate that fraud, if not common, is at least not too unusual.

Enough has been said to indicate that Merton's norms have come under severe attack and that their existence is, to say the least, open to doubt. It might be wondered why this normative structure could be e rected almost without opposition in the 1940's and be so severely attacked in the 1960's and 70's. One of the major reasons for this is the work of Thomas Kuhn which will be discussed below.

3.3 Scientific Knowledge as Consensus

By the above title I do not wish to imply that for Merton science is non consensual, but that the Mertonian consensus is imposed by nature, science is consensual because it is true. For Kuhn the consensus is not imposed from without but created from within by scientists. Kuhn's ideas on this subject were first expressed in 1962 in 'The Structure of Scientific Revolutions' and re-issued with some changes in 1970.[46] His views are well known and so will only be considered briefly here. Scientific activity is divided into two categories - normal and revolutionary science. Normal science is governed by the paradigm. This concept has attracted some criticism relating to the numerous senses in which it is used[47] but it s two main elements are a set of shared beliefs, values and techniques and a set of models or exemplars of expected results.[48] The paradigm is not a 'recipe' for science which can be taught to prospective scientists but is inculcated by training, teaching, example and study of applications.[49] This knowledge is 'tacit' and can only be learned by doing science rather than by acquiring rules for doing it.[50] Broadly speaking, the paradigm defines what areas are interesting, what results may be expected and why and how these results may be achieved. Without this guidance the only activity which can take place is random fact gathering since all facts may be equally relevant.[51] Normal science, within the paradigm, has been described by Kuhn as a 'mopping up' operation; an attempt to force nature into a 'relatively inflexible box' supplied by the paradigm.[52] There is no intention to produce surprises and Kuhn likens the activity to jigsaw puzzle solving, where a solution is known to exist and the challenge is to manipulate apparatus and variables to achieve this solution. Since the solution is known (or at least believed) to exist, failure to achieve that solution is regarded as a failure of the scientist rather than of the paradigm. Kuhn suggests that normal science is progressive, at least for scientific practitioners, since, over time, more and more puzzles are solved and the paradigm becomes better and better articulated.[53] This success owes itself "... to the ability of scientists regularly to select problems that can be solved with conceptual and instrumental techniques close to those already in existence."[54] During the practice of normal science unexpected 'facts' are likely to emerge and, indeed, normal science is extremely efficient at producing the unexpected (though it does not aim to do so) since "... novelty ordinarily emerges only for the man, who knowing with precision what he should expect, is able to recognise that something has gone wrong. Anomaly appears only against the background provided by the paradigm."[55] The initial response to anomaly may simply be to 'wait and see' in the hope that further work will clear up the problem. Usually this response is justified and anomalies are eventually explained in terms of the paradigm.[56] Rarely, however, the anomaly may call into question some fundamental aspect of the paradigm[57] and, if severe enough, may lead to a 'scientific revolution'. This can only occur when an alternative paradigm exists and the rejection of one paradigm is always simultaneous with the acceptance of another,[58] this change being likened by Kuhn to a gestalt shift, with the difference that once things are seen in a new way it is not possible to return to the old world view.[59] Kuhn gives rather contradictory reasons for shifts of paradigm. He states that paradigms are incommensurable, thus they cannot be compared.[60] Elsewhere, however, he lists some of the reasons for accepting a new paradigm as the ability of the new paradigm to solve problems which the old cannot, aesthetic reasons such as neatness and simplicity and extra scientific reasons such as the new paradigm fitting in with theological or other beliefs.[61] This implies that paradigms cannot be completely incommensurable since how could, for example, greater neatness be identified except by comparison? This leads to the idea that degrees of incommensurability exist[62] and in a later work Kuhn appears to take this view.[63]

When a science possesses a unitary paradigm it is 'mature'. Prior to this time a number of 'pre-paradigmatic' schools may exist within a discipline. These share many of the elements of the mature paradigm[64] and may give rise to disputes over what problems are interesting, what observations are important and how these problems and observations may be best explained. Those observations which the school cannot adequately explain may be dealt with by ad-hoc means or left as problems for future research.[65] Several other authors have expressed ideas with some similarity to those of Kuhn. For example, Holton has considered the role of 'themata' in science.[66] Two types of themata are postulated relating to ways of expressing laws and to the practice of research.[67] The main difference between themata and paradigms is that themata may change during 'normal science' and persist through 'scientific revolutions'.[68] Other authors have suggested the idea of models as individual paradigms[69] and considered scientific change by the exploration of new areas,[70] but none of these have been developed and discussed to the same extent that Kuhn's ideas have. Before considering some of the critical comment on Kuhn's work two further issues are worth considering. Firstly, what role do social influences play in this model? It will be remembered that for Merton extra scientific influences were seen as a distorting factor. For Kuhn this is not the case. One of the reasons given for choice of a new paradigm was extra scientific influences. Since the paradigm is the governor of scientific activity, it follows that non scientific factors may be highly influential within science.[71] Secondly, what are the implications of these ideas for scientific controversies? Nowotny suggests that "Taking Kuhn literally would mean that controversies are utterly futile if they are conducted between paradigms, because they are unsolvable, while they should not really occur within a paradigm."[72] This, however, is not strictly correct. Firstly, controversy (or at least competition) between paradigms is not necessarily futile, at least for the supporters of the paradigm which becomes dominant, since this will be seen as progress. Secondly, 'pre-paradigmatic' conflicts between schools are not futile if they result in a unitary paradigm, and, thirdly, controversies within the paradigm are not necessarily disallowed. Normal science aims to 'flesh out' the paradigm. During this activity, the anomalies which arise may allow the entry of alternative explanations of these and hence controversy. It is possible that the types of controversy in these categories might differ in character. Controversies between paradigms, or candidates for paradigm, are likely to have wider ramifications with criticism aimed at methodology and techniques as well as findings. Controversies within the paradigm are likely to be milder and 'superstructural'. There will exist an agreed background of methods and results and disputants are unlikely to attack these since to do so would be to attack their own roots also. These suggestions also have implications for policy related disputes. Science in policy areas tends to be multidisciplinary and thus disputes may be deeper and more bitterly fought than disputes over anomalies in unidisciplinary sciences would be. These possibilities will be considered at greater length in later chapters.

Criticism of Kuhn's ideas falls into several categories. Firstly, he has been taken to task for his imprecision, for example, using the term paradigm in several different senses, so that the ideas are so vague as to have little explanatory power.[73] Secondly, Kuhn's work was criticised when it first appeared for it's lack of empirical support. This led to the suggestion that Kuhn used evidence from a set of rare situations to form over-generalised conclusions.[74] Case studies exploring this suggestion are considered below. The third area of criticism has been from philosophers of science, particularly those supporting ideas close to those of Popper.[75] These criticisms are very well expressed in an appraisal of Kuhn's work published in 1970 and the following paragraphs will be based on that appraisal.[76] Firstly, Popper accepts that normal science exists but characterises it as the work of an uncritical scientist who accepts the ruling dogma,[77] and further as "... a danger to science and, indeed, to our civilisation."[78] To this Kuhn replies that he is merely explaining how he believes science is carried out rather than expressing approval or disapproval.[79] The essence of the dispute is neatly summed up by Barnes, "Kuhn ... is read as a moralist, and criticised for advocating the wrong things."[80] Secondly, a major source of disagreement would seem to be over the frequency of scientific revolutions. For Kuhn these are infrequent events, whereas Popper sees a potential for overthrow of theories at any time. Kuhn suggests that here Popper has been mislead by his study of science concentrating on revolutionary periods and using these to characterise the whole of scientific activity.[81]

Attempts have been made to suggest that the differences between Popperian and Kuhnian ideas are linked to Kuhn's ideas being largely descriptive whilst Popper's ideas are mainly normative.[82] Whether or not this is the case for the philosophical dispute, it seems likely that Kuhn's support from the sociology of science may be explained by this distinction. Popperian theories with a large normative content provide little scope for work within the sociology of science, whilst Kuhn emphasises the need to study the activities of scientists[83] and provides either puzzles to solve (from a Kuhnian perspective) or a testable set of theories (from a Popperian angle). One of the main sociological research areas given impetus by Kuhn's work is the 'Strong programme'. It's basic tenets are as follows:[84]

1) It is causal, concerned with the conditions bringing about beliefs or states of knowledge.
2) It seeks to explain beliefs seen as true and false, rational and irrational. Both sides require explanation.
3) It should seek symmetrical explanations of both true and false beliefs.
4) It should be reflexive. It s explanations should also be applicable to sociology.

It can be seen that the strong programme is in contrast to Merton's work which divided science into true and false, good and bad, and only felt the need to explain the bad. Much of the work of sociologists of science in the 1970's has been given impetus by the Strong programme and by Kuhn's work, with sociologists 'getting their hands dirty', that is becoming involved in research at the laboratory level as observers or even participants. In the next section I will review some of these studies.

3.4 Case Studies

Much of the work in this area has involved the study of scientific controversies. Collins suggests that the study of controversies provides an insight into the processes of social negotiation which are hidden in 'ordinary' science.[85] Two main criticisms may be raised with regard to these studies. Firstly, it is suggested that controversies are rare and atypical events, thus, they provide poor guides to science in general. Collins replies that "... resolution of controversies is precisely the establishment of a new consensus."[86] That is, the mechanisms of consensus achievement are similar throughout science. Secondly, studies of controversy are 'one offs', they are non-comparable.[87] Certainly, a variety of methodologies have been utilised but studies are broadly comparable and whilst 'micro-comparison' may be problematic, broad generalisations may be drawn out. These are considered below.

In a study of a dispute over the detection of gravity waves in physics Collins introduces two views of knowledge transmission - the encultural and algorithmic models.[88] The algorithmic model assumes that an experimenter can provide a 'recipe' for an experiment such that it may be replicated at will, whilst the encultural model suggests that knowledge is transmitted more or less unconsciously via a shared culture. This concept is clearly akin to the concept of tacit knowledge considered above. Support for the existence of the encultural model has three important corollaries.[89] Firstly, it may appear that knowledge is transmitted via the algorithmic model if a shared culture is present since 'recipes' appear to work. Secondly, the only way individuals can know whether or not their knowledge is sufficient to replicate an experiment is by trying to do so and seeking the agreement of colleagues that the experiment is valid. Thirdly, in new situations where no shared culture exists, arguments about the comparability and validity of experiments will be negotiations about what is to count as a working experiment. Collins conducted his study via interviews with practicing physicists most of whom agreed that gravity waves were an expected phenomenon within Einstein's 'relativistic paradigm.' Following a report of detection of gravity waves other experimenters attempted to replicate this detection using different apparatus. Obviously, this different apparatus meant that these follow up experiments were not genuine replications. Collins explains this via the encultural model where the lack of a shared culture for the new phenom na meant that what was a working detector was still questionable. For example, there was much debate over what variables were relevant and, in several cases a highly important variable for one scientist was more or less irrelevant for another. Thus, no agreed criteria of replication and experimental judgement existed and these criteria could only be arrived at via negotiations of the culture of the field and the character of the phenomenon. Until this process was completed the categorisation of experiments as good or bad remained open to dispute. Some years later Collins returned to the controversy. By this time the original positive experiment had been rejected as incorrect though the reasons for reaching this conclusion seem to have varied from scientist to scientist.[90] One of the most influential 'destroyers' of the phenomina was 'Quest' (not his real name). Collins suggests that Quest and his group carried out their experiment specifically with the intention of developing a position from which they could most effectively destroy the phenomenon which they realised could not be done by experiment alone. Essentially, Quest, via social and political means,managed to define the shared culture such that gravity wave detection, using the currently available equipment, was not possible.

Collins provides further support for the encultural transmission of information in a study of the building of the 'Transversely Excited Atmospheric Pressure Carbon Dioxide Laser' - the TEA Laser.[91] He found that transfer of knowledge of building techniques did not (and, indeed, could not) take place via published information alone but only by transfer of individuals holding the knowledge. It was not possible to provide an algorithm for laser building because even those who succeeded in building working models were unsure as to which were important variables and techniques.[92] Several other authors have produced studies which may be taken to illustrate the importance of the shared culture in achieving and maintaining scientific consensus. A good example of this occurred in psychology in the final decade of the last century. The dispute took place between two psychological schools, primarily represented by Professors Baldwin and Titchener and concerned the measurement of human reaction times.[93] Titchener's group tried to make the test groups used as objective and standardised as possible whilst Baldwin was interested in 'natural' responses.[94] Thus, whilst Titchener's interest was in the general phenomena with individual differences removed, for Baldwin the main area of interest lay in these differences. The controversy over whose results were 'correct' proceeded for several years with scientific debate gradually being replaced by polemic and personal attack with resolution only achieved when other experimenters found that both sets of results held under different circumstances.[95] The obvious question here is why did the protagonists not arrive at this solution? Krantz suggests that the results themselves were not important, what was important was the relevance of the data for psychology and "... facts could only be important when the disputants had agreed on a common ground of discussion",[96] i.e. a shared culture. Krantz is at pains to avoid the dismissal of this episode as an 'unscientific aberration', for him it is "... a heightened level of the very conditions which are necessary for normal scientific functioning and growth."[97]

A less prosaic study of experimental replication is that of 'worm running' by Travis.[98] The study is explicitly based on the encultural model and Travis suggests that since the number of potential variables in any experiment is infinite, the decision to regard two experiments as 'the same' must involve a shared culture[99] (containing the elements of the Kuhnian paradigm). The controversy began in 1955 when two psychologists, Thompson and McConnell published a claim to have trained a flatworm in a conditioning task. Flatworms are able to regenerate into viable worms if cut into pieces and, even more startling, was their claim that both regenerated tail and head sections retained much of their previous training and, furthermore, when trained worms were chopped up and fed to untrained worms, these tended to learn more quickly than would otherwise be the case. "Knowledge, it seemed, was edible."[100] Attempted replications failed and alternative explanations were offered. These failures were, in turn, dismissed as incompetent replications. Eventually, the 'successful' experimenters were challenged to provide full details of how learning might be achieved, i.e. they were asked to provide an algorithm.[101] This they could not do, they were unsure as to exactly what variables were relevant.[102] It seems that past successes had occured by experimenters getting things right as a matter of course, that is, by the development of a body of tacit knowledge. Travis further argues that the arguments which took place were, in fact, negotiations of what variables might be considered relevant, what experiments successful and so on.[103] If this is the case then the question arises, how are scientific disputes brought to a close? For the Mertonian this would occur via further experimentation, data collection and so on, which eventually compels agreement. However, if both theory and perceived reality are negotiated and negotiable, then there exists no outside factor to compel agreement and any dispute could continue indefinitely. Under these circumstances, it is suggested, disputes end via "... a collective decision to stop arguing."[104] There may be several reasons for taking this decision. Pickering, in a discussion over the possible detection of magnetic monopoles, suggests that the limiting factor was the decision of the participants to maintain earlier agreements, that is, all previously accepted experiments and explanations were upheld. This in turn constrained the possible explanations of the experimental results and led to the abandonment of the monopole explanation by it's discoverers in order to avoid jeopardising agreements on earlier phenomina.[105] This may be seen as an example of shared culture constraining the controversy. It was in the interests of neither group to attack the roots on which both their work was based. Group interests may also influence the closure of controversies in other ways. This idea is developed in a study of two different theories, charm and colour, as explanations of new particles found in physics.[106] Initially, three groups of scientists existed with interests in the area, one pro-charm, one pro colour and one ostensibly neutral group consisting of hadrodynamacists and guage theorists. However, charm theory supported the ideas of these neutrals and provided them with further work whilst colour did not intersect with their interest at all. Thus, in Kuhnian terms, charm provided new puzzles to solve whereas colour did not.[107] Pickering explains the support for charm in terms of interests. Charm provided support for work already undertaken by the physics community and areas of further work whilst colour did not intersect with these areas. Eventually, even the originators of colour ceased to support their ideas, primarily because charm had been accepted by the majority. To continue to talk to other physicists and to do physics those who had supported colour had to switch to charm.[108] Where groups are more insulated from one another this closure mechanism may not operate. This is illustrated by Dean in a discussion of a debate over systems of taxonomy in biology.[109] The debate has been going on for over fifty years with no achievement of consensus. Each 'side' has different skills, social networks, funding areas, research communities and so on, and, thus, can maintain any dispute without professional penalty, an option which was not open to the colour theorists. What links these controversies is the role played by interests. In the example from physics these aided the closure of the dispute whilst in the taxonomy example these tended to maintain the dispute. The role of interests is summed up by Shapin:

> "... Skills and technical competences ... represent a set of vested social skills within the scientific community. There is every reason why a scientist should wish to display the value and scope of what he can do, even to the extent of criticizing the value and scope of others' acquired skills and competences."[110]

Thus, consensus may be achieved via mechanisms which are rational in an individual sense but less so in the traditional scientific sense. A further example of the apparent involvement of less than fully rational criteria is the case of Barkla and the 'J Phenomenon'.[111] Barkla was a professor of physics at Edinburgh University from 1913 until his death in 1944 and had won the Nobel Prize for his work on X ray scattering and his discovery of the K and L Electron Shell series. In 1916 he announced the discovery of a new set of radiations, the J radiations associated with the J Electron Shell. By the 1920's opposition to these ideas was growing and in 1923 Barkla rejected the idea of a J Shell but re-emphasised his belief in the 'J Phenomina'.[112] The most interesting aspect of the controversy is the way in which critics 'refuted' Barkla's ideas. Firstly, all these refutations referred to an earlier mistake of Barkla's as though this were evidence that he were mistaken again. Secondly, what was seen as the 'definitive refutation' of Barkla's ideas appeared in 1928 five years before Barkla finished publishing results.[113] Thirdly, Barkla was criticised for not publishing a complete account of his experimental techniques and, finally, he was criticised for his lack of an explanatory model of the results.[114] Clearly, these first two points show that Barkla's critics did not consider all the evidence and only the evidence, as the rational scientific model suggests that they should. This contention is further supported by an earlier critique of Barkla's work which used a mistake by two of Barkla's research students to question the credibility of the whole of his work at the same time as suggesting that his was the only support for his ideas when, in fact, the work had not been used in support of the J Phenomina.[115] Mulkay ties in the latter criticisms with the TEA laser study (above) which shows that "... It is literally impossible to provide a complete discription of experimental procedures and this is not usually required... Nor are observers always expected to give a causal explanation of their results before the latter are accepted as competent."[116] On the basis of these issues Wynne suggests that the reasons given for the rejection of Barkla's ideas were, in fact, post-hoc rationalisations which may be traced to the different interests of Barkla and his 'opponents'. Each group was committed to different types of experiments and techniques and were thus aiming to show the utility of their research skills and activities. As the gulf grew wider, the shared culture became less, tacit knowledge was not 'common knowledge' reducing the probability of consensus still further,[117] until eventually, as with the colour theorists, Barkla was frozen out of the mainstream of research. Other studies support similar conclusions to these[118] but I will now turn to a further influence in the conduct of controversy - the role of authority. This authority may come from outside science, from inside a single science or from one science over another and may generate as well as close controversies. The idea of authority is closely linked to that of prestige - an individual who has carried out well regarded work in the past is likely to be granted greater prestige and authority than one who is less well known or who has made errors. Though this is understandable it does not accord with the 'rational' view of the independence of the discovery and the discoverer.[119] Several examples exist of the exercise of authority both from within and outside science. Ford discusses the authoritative role of physics in the nineteenth century.[120] His first example relates to Darwin's theory of evolution and, in geology, Lyell's theory of uniformitarianism both of which required greater age for the earth than allowed for under the then current religious schema.[121] Lord Kelvin, the physicist, took up the challenge of arriving at a 'scientific' age for the earth. He considered a variety of known physical processes giving a range of ages of up to 400 million years (later reduced to 24 million years).[122] This time scale was insufficient to support Lyell's ideas but rather than generating a controversy the response of the geological community was to accept these estimates. "They were either convinced or cowed by Kelvin's mathematical treatment of known physical phenomena and adjusted their speculations accordingly."[123]

Ford's second example relates to the theory of continental drift. The originator of the theory, Alfred Wegner, could suggest no mechanism for the phenomina which physicists could not demolish and, such was the authority of physics, that the geological, botanical and palaeontological evidence was dismissed as mistaken.[124] Both of these examples demonstrate the authority with which physics was endowed in defining what was acceptable as 'truth' within science. It is possible today that with the existence of more or less independent research schools, these examples could not recur. However, any group or individual disputing all or any of the ideas of modern science is still likely to be regarded (perhaps correctly) as a 'crank', as witnessed by the 'Velikovsky affair'. It is possible that Kelvin's religious beliefs motivated his involvements in the area but even if this was the case, his actions took place within science. Creationism is a religious belief which tries, from outside science, to constrain and control scientific theories. Arguably, the creation /evolution dispute is policy related since it intersects with such issues as teaching but, as an example of the role of authority, it fits in more neatly within the current discussions. The debate is not a new one and has been occurring with varying intensity since Darwin's theory was put forward. Rather than repeat the well known arguments[125] I will instead only consider what lessons may be drawn from the debate. Firstly, the roots of the current exacerbation of the controversy are as much in the political world as the scientific and may be a good example of Nowotny's 'conflict by proxy',[126] that is, a dispute that is conducted 'scientifically' but related in large part to non scientific issues. Yoxen identifies the current resurgence of creationism with the rise of the 'new right' in the United States, this being related to the perceived economic and moral decline of society with the answer to this seen as a moral crusade and a reassertion of 'The American Way'.[127] The beliefs include reference to the Bible as an absolute basis for living and a belief in the truth of the ideas expressed therein. From here it is only a short step to the view that if creation is 'true', then evolution is 'false' and thus the teaching of evolutionary theory is at best misleading and at worst atheistic and leading to moral decay.[128] Thus, attempts are being made to use the authority of the Bible to determine truth status within science.

As a final example here I will consider one of the few case studies which attempt to consider the relation between pure science controversies and policy related science. The study, by Pinch, concerns discrepancies in the predicted and actually detected numbers of solar neutrinos.[129] Four main specialities have been involved in this area, radiochemistry, nuclear physics, astrophysics and neutrino-physics. These fields are highly specialised and expertise tends to be confined to one area only. Pinch suggests that scientists are likely to place the blame for the discrepancy in fields other than their own and express certainty in their own.[130] This, he claims, can be explained by the craft practices and tacit knowledge incorporated in science with the practices being taken for granted within the speciality but seen as sources of doubt to outsiders.[131] To quote a phrase used earlier, the lack of shared culture is a cause of doubt. At the same time as expressing doubts in other specialities, the scientists reaffirmed their beliefs in the solidness and certainty of their own though on further 'off the record' questioning, scientists did express uncertainties in their own practices. Pinch suggests that the perceived audience for scientific comments influences the degree to which uncertainties are expressed[132] and then brings in the notion of the core set,[133] which consists of those scientists actively involved in any controversy such that their work may influence the outcome.[134] It seems likely that these core set scientists are more likely to be aware of uncertainties whilst scientists more peripheral to the specific area may believe that the issues are more or less settled. Finally, Pinch relates the dispute to public science debates such as those over nuclear power. He makes two main points. Firstly, since scientific certainty is contentious in basic science, it is hardly surprising that it is also problematic in public science. Secondly, in both public and basic science it may be useful to consider the relevant audience for comments[135] since publicly expressed confidence may mask private doubts (at least for the core set). These issues will be considered at greater length in the following chapters but, finally, here I will discuss some of the issues raised above.

3.5 Discussion

It is clear that the above studies provide little support for the Mertonian idea of science as truth and are far more supportive of Kuhn's ideas, at least in broad outline. Several common features emerge from the case studies. The most important of these is the non conclusive nature of experiment as an arbiter of 'the facts'. Collins describes this phenomina as the experimentors' regress.[136] Since an experimental replication can never be an exact copy of the original experiment, the only way to assess whether or not this replication is acceptable is by replicating it and that replication must, in turn, be replicated and so on. If this is the case then decisions as to the validity of experiments and replications, cannot be coerced by science but only negotiated and agreed socially. This negotiation will obviously apply not only to controversial areas but to all scientific activities and the limited study carried out to date based on laboratory work supports the contention that in day-to-day as well as controversial science, 'scientific' factors are insufficient to compel agreement.[137] Thus, in the following discussion I will be referring to the controversies considered above but the issues raised will be relevant to all areas of scientific activity.

If factors from within a single science are insufficient to create consensus, how is this consensus arrived at? The case studies offer several, not incompatible, answers. Firstly, the acceptance of the authority of one discipline or group over another with regard to what is, or is not, possible. Thus, physics was able to act as a constraint on potential controversy relating to continental drift. It seems likely that this exercise of authority is less probable in well differentiated modern science but an analogy may be found in Collins' core sets. Members of the core set are those who, having researched in a particular area, may issue authoritative comments upon that area. In the gravity wave controversy this role was exemplified by Quest, who reportedly conducted an experiment not as an attempted replication but as a means of joining those with a right to be heard.[138] Reeve, emphasising the role of the experimental result as authority suggests that if Quest's group had produced a positive result then their attack would have been untenable.[139] This seems to ignore the ease with which ad-hoc explanations of unexpected results may be arrived at. Of course, it is possible that a positive result might have converted Quest's group to gravity wave supporters but it is equally likely that the results would have been explained as artefact and used as an example of the problems inherent in the area. Overall, it seems likely that one of the main ways of judging whether or not an experiment is 'good' or 'bad', competent or incompetent, is by the results it produces, though that is not to say that experimental results are not used in support of particular positions, especially by those outside the core set who are less aware of the inherent problems and uncertainties of the area.

Associated with authority as a means of closure is a second means 'the decision to maintain prior agreements.' These agreements serve as a constraining authority as to what explanations are acceptalbe. In the case of the magnetic monopole it would not have been socially acceptable for those claiming discovery of the monopole to have attempted to criticise earlier agreed experiments. In principle, this criticism could have taken place,but to do so protagonists (within a single discipline) would also be attacking the bases of their own ideas and work to that point (i.e. the shared culture). Under normal conditions this is inconceivable, though, if Kuhn's ideas are correct, when sufficient anomalies are present this occurs as a scientific revolution.

A third mechanism of closure is provided by the interest model. In the disputes over charm and colour, and the J phenomenon, the losing protagonists beliefs caused them to occupy marginal positions and victory for them would have meant a loss for the majority of physicists. Thus, the colour theorists and Barkla were 'frozen out' of mainstream research. Even if, for them, the controversy continued, for the majority it did not and the issues they raised were not worth studying or discussing. Instead, there was a positive impetus to deny the validity of their ideas since to say that Barkla might be right would have been tantamount to a confession of possible error by those utilising different research methods.

To summarise what has been suggested above. Controversies are not aberrant phenomena. 'Facts' in science are agreed by social and technical mechanisms. Where these are slow to operate controversies may develop and these are brought to a close by precisely the same means that non controversial facts are agreed. These include the acceptance of some authority, the 'power' of majority interests and the importance of shared culture in making some ideas, not least in the short term, more or less sacrosanct. Much of this shared culture is on the tacit level and thus experimental skills can only be judged by performance, that is by the ability to produce acceptable experimental results.

What are the implications of these ideas for policy related sciences? Firstly, it is clear that controversies in these areas are unlikely to be merely 'conflict by proxy'. That is not to say that policy relevance does not influence conduct but since disputes in pure sciences are socially influenced, it seems highly likely that these influences will be found throughout science. Secondly, some of the limiting factors on controversy discussed above, may illuminate the reasons for the longevity of policy related disputes. In pure sciences, problems are generally chosen by scientists, in Kuhnian terms they are puzzles where both the solution and the method of achieving that solution are defined. In policy areas problems may be taken on with little idea how these should be attempted, what variables are relevant and where solutions may lie. Thus there is no obvious means of judging what is a 'good' or 'bad' experiment. This problem is exacerbated by the generally multidisciplinary nature of these areas. Thus, there is no unitary shared culture (or paradigm) to act as a fixed reference point. This has two main implications. Firstly, doubts may be felt with regard to the practices and interpretations of other disciplines and, secondly, articulation of these doubts need not be restricted to immediate results. Attacks may take place in the general as well as the specific disciplinary practices without harming one's own discipline or it s experimental practices and, on a more personal level, without affecting one's prospects of obtaining grants, promotion, etc, as would be the case if a similar attack were launched closer to home. If these assertions are correct, it would be expected that (at least some) scientific controversies in policy related areas would be long lived, have a high critical temperature, may accumulate a mass of experimental data without achieving consensus and, given the still widely held view that science equals truth, attempts may be made to explain the activities of one's opponents in terms of bias, irrationality and bad science. These ideas will be tested in later chapters but, firstly, in the next chapter I will consider some of the problems of research in policy related areas.

PART II

C H A P T E R 4

PROBLEMS OF RESEARCH IN POLICY AREAS

4. PROBLEMS OF RESEARCH IN POLICY AREAS

4.1 Introduction

In the previous chapter several factors have been suggested which may influence the closure of scientific controversies. If controversies are 'natural' rather than aberrant phenomena and closure factors are weak or not present, then long lived controversies may occur. In this chapter I will be considering some of the factors which may support continuing existence of controversy. In the style of detective fiction these factors will be considered under three headings - means, motive and opportunity.

4.2 Means - Science is Multidisciplinary

It seems to be a general rule, with few exceptions, that policy relevant research involves several disciplines. This may give rise to problems with regard to the comparability of research produced by different methodologies, experimental techniques and so on. There is no shared culture (or in Kuhn's term paradigm) to form a common basis for discussion. Each discipline is likely to have it s own authorities, publications, research institutions, sources of funds, etc, which mean that the closure mechanisms considered in the previous chapter are unlikely to operate and may, in fact, increase polarisation of views as disputes come to be seen as 'our' results and methods versus 'theirs', with in-depth criticism taking place.

Several authors have considered policy related controversies in the light of these issues, either from an explicitly Kuhnian perspective or in more general terms. One of the best known of the former is an examination of the debate over the health effects of lead in petrol carried out by Robbins and Johnston.[1] They specifically reject the idea of Merton's 'disinterested scientist'[2] and instead suggest that "The controversy bears all the marks of a conflict between self contained systems of belief; lacking concepts and terminologies in common, the protagonists tend to 'talk through' each other."[3] The main emphasis of their paper revolves around the debate as to what are normal blood and environmental lead levels and involves very acrimonious comments by certain chemists, occupational toxicologists and geochemists as to the competence of other disciplines to comment on these levels.[4] Robbins and Johnston conclude,

> "It thus appears that the difference in technical, cognitive and professional standards and the way they are mixed between the rival sets of experts provides an explanation for the conduct of the controversy and for their inability, at least in the short term, to reach any form of consensus"[5]

though in the longer term this may be possible.[6]

Gray reports rather comparable findings in a study of the propensity of U.S. defense analysts to be 'hawkish' or 'dovish'.[7] He finds that it is possible to differentiate these by institution rather than discipline, though these may well be connected, since a specialist is more likely to seek out and be accepted by institutions with coinciding views. Under these conditions also there is a lack of direct pressure and authority on individuals to compromise and form a consensual view. Nelkin, in studying a controversy over the siting of a nuclear power station notes that opposing scientists could not agree on issues such as water sampling intervals and techniques and that different issues were viewed as important. She suggests that such findings are typical of expert disputes and that other relevant issues included the political use of information and genuine uncertainties.[8] (These issues will be discussed below). Several other authors have described controversies in which a complex mix of social and disciplinary differences have exacerbated or maintained disputes.[9] Nowotny and Hirsch claim that these disputes do not arise because of a mix of the scientific and the political but merely make explicit the relationships which already exist.[10] This interlinkage makes the division of explanation between 'scientific' and 'political' factors an artificial one, as witnessed by Calabrese's comments on different methods of standard setting for industrial chemicals in the U.S.A. and the U.S.S.R. In Russia the maximum allowable concentration of a chemical is one which will not permit the development of any deviation from normal physiologic parameters, whilst in America minor adaptive changes are permissible.[11] Leaving aside the possible disagreements over normal levels, these differences are clearly a mix of the political and the scientific, with an undesirable change in the U.S.S.R. seen as a normal adaptive response in the U.S.A. Similar international differences in judgement have been noted with regard to U.S. and British examination of evidence relating to the carcinogenicity of the pesticides Aldrin and Dieldrin. Although the same evidence was examined by scientists in both countries only in the U.S.A. were the pesticides viewed as carcinogenic, apparently because (amongst other reasons) certain U.S. scientists used a 'trigger' model of cancer (such that a few molecules of a substance may trigger tumour growth), whilst their British counterparts utilised a more permissive 'threshold' model (at which low levels of a substance are harmless).[12]

These few examples, coupled with the discussion in the previous chapter, should be enough to provide support for the idea that 'genuine' scientific differences can exist, that is disputes may be explained without the necessity to invoke claims of fraud, bias and so on (though clearly these are not disallowed). If these provide the means for scientific controversies, what provides the motivation?

4.3 Motives - Political Relevance

As has been emphasised at several points, I do not wish to suggest that without political 'interference' all would be well in science,but clearly the policy relevance of some scientific findings and pronouncements gives them a visibility which they would otherwise not have and this may increase the temperature of disputes. Thus, in this very limited sense political relevance may be seen as a potential cause of more heated controversy but not in any way that may be 'corrected'.

What are the reasons for utilising science in these areas? A very important one is the widely held view that science is a means of discovering objective truth and can, therefore, be used to inform decision makers and the public (the recipients of information) of the bounds of the possible. Here scientific information performs the legitimating function discussed in Chapter 2. This word is not used in any perjorative sense, but to mean that scientific information is used in support of one political view of the legitimate and best way to proceed. Of course problems arise here when scientific conflict is present because then scientific findings may be used in support of several political viewpoints and it is under these circumstances that Nowotny's phrase 'conflict by proxy' is particularly apposite.[13] It should not be forgotten here that political conflict may support, as well as be supported by, scientific conflict, providing funding, publicity and so on for opposing scientific views hence aiding controversy to continue. One example of this was given in Chapter 3 - the creationist versus evolutionary debate.

When this legitimating role of science was considered in Chapter 2 it was noted that advisors may be expected to find information which supports policy and reduce the import of any information which goes against policy. Lipsky and Olson identify this as one of the major functions of riot commissions in the U.S.A.[14] Following the riots in several North American cities in the 1960's, various riot commissions which were set up to find the causes of these and earlier riots. Lipsky and Olson suggest that two of the commissions' main functions were to gain legitimacy for proposals already arrived at and to defer action whilst appearing to act.[15] Furthermore, they suggest that on several occasions, the commission opted to secure political legitimacy at the expense of scientific legitimacy by rejecting scientific work which arrived at unpalatable conclusions.[16] Hadden notes the existence of a similar phenomenon with regard to the possible carcinogenicity of di-ethyl stilbestrol, a growth promoting drug. She suggests that "... the policy makers used the scientific uncertainty to choose interpretations which supported their own policy preferences."[17] As well as legitimising one's own case, science may also be used in attempts to 'de-legitimise' an opponent's case. On the view that science is rational and true, any disagreement with a policy maker's scientifically supported preferences can be discredited as 'irrational', 'biased', 'emotional', 'value laden', and so on. Examples of these responses are often found where scientific findings are being used to enforce or promote certain regulations and here the disputes tend to consist of closely intermixed facts and values. For example, Peterson and Markle have considered the controversy over the efficacy of laetrile as a treatment for cancer. They note that problems existed with the comparability of data collected by the several disciplines (multidisciplinary problems as considered above) and also that opponents of laetrile attempted to categorise the dispute as merely one of freedom of choice over treatment.[18] Hence, in many people's eyes, the dispute came down to 'scientific truth' ('laetrile is ineffective') versus 'irrational values' ('freedom of choice'). Though clearly these issues did play a part in the debate it seems unlikely that those favouring laetrile and taking it for the treatment of cancer did so simply in the belief that it was ineffective but that they were asserting their rights. Similarly, the recent debate over the utility of wearing automobile seat belts in part consists of a debate over freedom of choice and, in part, a scientific debate over the interpretation of data on the question of whether occupant restraints actually do reduce, or merely redistribute, road deaths.[19]

I have not here explored the extent to which either science influences policy choices, or policy preferences influence the directions of scientific research. It seems likely that the policy relevance of certain areas encourages the development of controversy by persuading scientists to begin research which would otherwise be left for the future ('puzzles' which have no obvious approaches or solutions). It also seems likely that a variety of scientific findings may influence policy disputes by providing legitimation for these views. If this is the case then some form of positive feedback mechanism might exist such that a dispute in either science or policy may increase the possibility of a dispute in the other area which increases the temperature of the original dispute, and so on. The corollary of this is that if agreement exists in either of these, dispute is less likely in the other since, for example, disputing scientists will not find a political platform for their views and disputing politicians will find it difficult to find scientific support to legitimate their views and 'de legitimate' their opponents. Thus, as well as a positive feedback loop fueling dispute, a negative loop may exist which damps down disagreement. These postulates imply that disagreement in science is more likely where there is disagreement over policy and vica versa. If this is the case then the utility of science may be called into question, at least for the synoptic policy maker, since it suggests that when policy makers actually require advice to aid the choice between several policy options, scientific disagreement is likely and this disagreement is only quelled when advice is no longer required. As a final point, I have presented the above models as static ones but clearly this cannot be the case. There are likely to be times when agreement exists in one sphere but not in another. The suggestion is that the above options are 'stable configurations', whilst times of dispute in only one area are transient phases between these two and will tend to move toward one or other of the above models.

Next I will turn to the problems of data collection and utilisation. Though scientific findings may be used politically, the debate is, in large part, conducted in terms of scientific 'facts' and data and it is here that major opportunities for dispute exist.

4.4 Opportunity

It was noted in the previous chapter that experimental results alone are insufficient to enforce agreement in science and that this agreement is influenced by social factors such as shared culture. Surely, these provide sufficient opportunity for disagreement? Whilst the answer to this is yes, there are two major reasons why opportunities for disagreement are worth studying separately. The first reason relates to the core set and the audience for scientific expressions of uncertainty. It will be remembered that the core set consists of those scientists who are actively involved in any controversy.[20] When a dispute takes place between two disciplines and particularly if areas of dispute are obvious to all practicioners in a discipline, then most or all practicioners may serve both as participants and audience for expressions of uncertainty.[21] Secondly, there may be those who, despite the discussions in the previous chapters, still hold the view that, with a little tinkering, a modified synoptic view might be achieved. The following should be sufficient to indicate that numerous problems exist in research which has relevance to policy. Discussion will focus on health related topics and be based on the widely considered area of drug assessment. Many scientific controversies in the policy arena have health related aspects, for example, fluoridation of water, lead in petrol, smoking and so on, and the issues raised have much in common with those found in drug research. Two major area may be identified. Firstly, opportunities for disagreement in arriving at results and, secondly, opportunities for disagreement over whether or not these results justify action. Though these have closely overlapping aspects they will be considered separately.

4.41 Opportunity 1 - Experimental Interpretation

There are three main methods by which data on potential hazards may be gathered. These are, animal testing, controlled human exposure and epidemiological studies. First, a brief note about these.

Animal testing has been utilised in the assessment of potential hazards since the early nineteenth century.[22] This use has it s roots in both ethics and science. It is considered to be less unethical to expose animals to potential hazards than to expose humans to the same hazards[23] and it is assumed that, since humans and animals are phylogenetically related, their responses to physical and chemical insults will be similar[24] and, thus, animal data may be used to assess human hazards. Though these ethical issues are clearly of major importance in this chapter I will concentrate on the scientific aspects of animal testing.[25]

Controlled human exposures usually take place once drugs and chemicals have passed the hurdle of animal testing. Debate exists as to the amount of animal testing which should take place before human study occurs with discussion ranging from the risks of too early human exposure to the advantages of humans as subjects and the losses of wrongly rejecting a useful human drug which fails animal testing procedures.[26] It is worth bearing in mind that ethical issues exist in this area, for example, is it ethically justifiable to expose an individual (even a volunteer) to a potentially toxic substance? Is the answer to this the same when considering a drug (for which some health benefits might be expected) and an industrial compound (where no benefits would be expected)? Again these ethical issues will not be further considered but they should be borne in mind when considering this area as a 'scientific' one.[27]

Epidemiology is the study of disease patterns of large groups. It may be divided into two categories.[28] Firstly, searching for causes of diseases known to affect a definable group or sub-population. Secondly, searching for the effects of exposures to potentially injurious substances. Clearly epidemiological studies can only detect existing disease processes and this detection depends on such factors as the frequency and form of responses. For example, the rare cancer angiosarcoma of the liver was connected with the manufacture of polyvinylchloride from an incidence of only fifty cases worldwide,[29] whilst fifty additional cases of a common disease such as lung cancer would obviously go unnoticed.

All of these methods of hazard assessment have common problems and uncertainties with regard to data collection. These fall into two areas, experimental control and extrapolation of data.

Experimental Control. It will be remembered that in Chapter 3 the problems of experimental replication were touched upon. Since one experiment can never be a direct copy of another, it is always possible to claim that an attempted replication is deficient in some respect. Clearly this claim is more likely to be widely accepted the greater the potential variability in either the subjects or the experimental conditions.

Hewitt suggests that a major advantage of animal tests lies in the opportunity to use genetically uniform stocks and thus variations in response can only be due to variations in treatment.[30] Goldman supports this view and also suggests that animal experimentation allows fairly rigorous control of influential variables.[31] Given the numerous potentially influential variables this may be rather optimistic. Some of these are listed in Table 4.1. Some of these variables may be easily controlled but others, considered less relevant or unknown, may be uncontrolled or uncontrollable. One researcher in this area has commented upon these problems by quoting the 'Harvard Law of Animal Behaviour'. "Animals under the most precisely controlled laboratory conditions still do as they ... please."[34] An example of these unexpected responses is described by Barnes and Denz where animals apparently did not like the taste of a compound added to their food (food intake increased as amount of additive decreased). Thus, such factors as weight loss, deficiency diseases and so on could occur not directly related to the compound under study.[35] Some authors have also reported that handling animals in different ways can result in differences in learning responses,[36] and this observed response is clearly analogous to the experimenter effects found in clinical drug trials which has led to the practice of conducting these 'double blind'.[37]

TABLE 4.1 FACTORS AFFECTING ANIMAL REACTIONS AND INTERPRETATIONS OF DATA.
(From Hurni [32] and Giovacchini [33])

Physiological Status	- Strain, species, genetic factors, age, weight, maturity, sex, oestrous cycle, pregnancy, lactation, health.
Environment	- Season, temperature, humidity, barometric pressure, atmospheric constituents, air circulation, light intensity and spectrum, light cycle, noise, commotion.
Diet	- Constituents, quantity, mode of administration.
Water	- Quality, quantity, mode of administration.
Caging	- Size, material, shape, single animal or group, crowding, hygiene.
Bedding	- Source, quantity, frequency of changing.
Handling	- Physical contact, personal qualities of staff, e.g. training, temperament, replacement, etc.
Compound Administration	- Route, dose, fasting or non fasting, number of doses, frequency of administration, continuous or bolus dose.
Results	- What to examine? e.g. behaviour, tissues, cells, etc. After how long?

Human subjects are likely to be as variable in their responses as animals and many of the animal variables listed in Table 4.1 have their human analogues. In some ways humans may be even more variable than animals since lesser control can generally be applied to their activities, diet, etc.[38] Epidemiologists face even more horrendous problems in assessing influential variables under non-controlled situations. These problems include assessment of dose or exposure level which is usually uncertain and often unknown, exposure patterns - in peaks and troughs or at a steady level,[39] synergistic effects of exposure to multiple chemicals,[40] and the existence and effects of pre-existing disease patterns. Before any of these questions can even be asked, the relevant population or sub-population must be identified. Here too, problems exist, for example, have exposed persons been lost to the study? and was this loss due to geographical mobility or death? If individuals have moved away was it because they were unable to tolerate exposure to the hazard under study and hence, is the remaining population hypo-sensitive? If death is the reason for participant loss, was this connected to the hazard in question? Theoretically, data on this issue should be obtainable from death certificates but these are not always reliable or comparable over time or geographical area. An example of this problem may be found in the London Bills of Death for 1665. (Table 4.2).

Table 4.2. Causes of Death in London in 1665.[41]

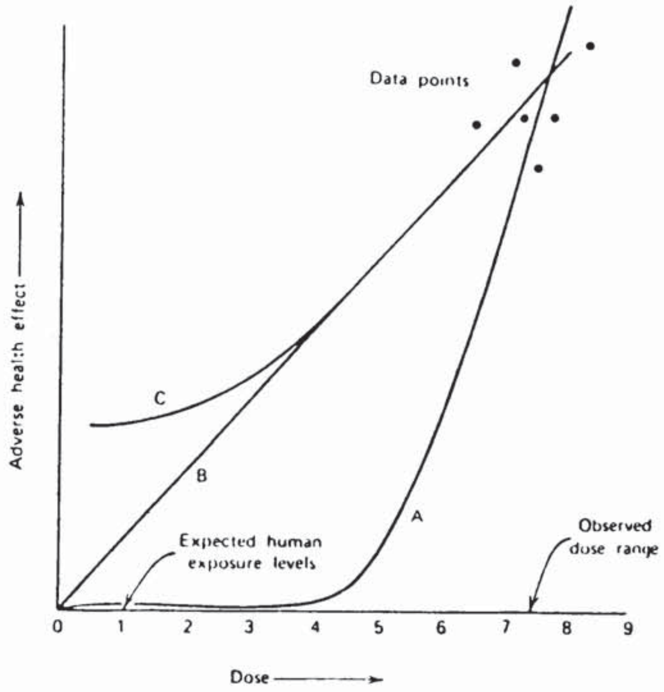
Executed	21
Flox and Small Pox	655
French Pox	86
Frightened	23
Overlaid and Starved	45
Plague	68596
Rising of the Lights	397
Scurvy	105

Some of these causes of death such as plague and scurvy might be recognisable to the modern epidemiologist but others, such as 'rising of the lights' might cause some debate amongst modern diagnosticians! To give a more recent example, Harrington notes that "For years, North American physicians diagnosed emphysema where British physicians presented with the same case would have diagnosed chronic bronchitis."[42] Other problems have emerged when clinical causes of death, as recorded on death certificates, have been compared with post mortem findings. Burch reports on a study finding that death certificates overestimated lung cancer fatalities by a factor of two.[43] The occurrence of this would clearly cause major problems for an epidemiologist carrying out a retrospective study.[44]

All of these above difficulties relate to the collection of data and this may or may not involve extrapolation or interpolation. However, once data have been collected, some form of extrapolation is inevitable if findings are to have any meaning beyond the population the original data relates to. It is here that the next set of problems may arise.

Extrapolation of Data. There are several reasons why data extrapolation may be required and may be problematic. These reasons include the translation of data from animal to human responses, the transfer of data from one human group to another and estimates of effects at exposure levels other than that to which the data relates. These questions may, in principle, be informed by further experimentation but this may, in fact, not be possible. To describe situations such as this Weinberg has coined the phrase 'trans-science'. These are "... questions which can be asked of science and yet which cannot be answered by science,"[45] - they transcend science. Examples of these include the estimation of very low dose effects and the probability of extremely improbable events such as nuclear reactor accidents.[46] In situations such as this where answers are required and experiment cannot help, answers can only be offered by extrapolation and 'best guess'. Even where experimentation can be carried out severe problems may exist. For example, Rall notes that there are five steps in the ultimate action of any chemical.[47] These are absorption, distribution, metabolism, excretion and mechanism of action. Even if, in each of these steps, there is a correlation of 0.9 between the response of one species and another, then the overall correlation is only 0.9 to the power 5 or 0.59. If four of the steps have the very high correlation of 0.95 but the fifth only 0.5 then the final correlation is reduced to 0.41. As Rall says, 'This is not good predictability'. Animal responses may differ both qualitatively and quantitatively from those of humans. For example, morphine produces a depressant effect in man, rats and dogs, but a stimulant effect in cats, goats and horses and phenylbutazone (an antiarthritic drug) is metabolised in man at about 15 per cent per day whilst in many animal species it is excreted in only a few hours.[48] Further problems may arise where no analogous response exists between animals and humans or where any response cannot be communicated, for example, nausea, headache, etc.[49]

When using the phrase 'extrapolation to the human' the question, Which human? is raised. This has been called the 'median mouse to median man' issue.[50] Under most conditions, "Experimentalists tend to select vigorous, well fed, healthy animals to extrapolate to a population which contains sub-populations that have all varieties of illness, weakness and disease."[51] This issue is also highly relevant with regard to controlled human exposures where similar extrapolations occur. Paget illustrates the problems of this area by consideration of the effects of steroids. The responses these produce are predictable in both humans and animals but they may also exacerbate latent and quiescent infections such as tuberculosis.[52] Similar differences in response may also occur due to genetic variations.[53] As a means of overcoming these problems, two main approaches are used, the study of an animal species which is more sensitive to any effects than the human[54] (such as canaries in coal mines) and the use of conservative standards permitting human exposures of only a fraction of those producing responses in animals.[55] This approach entails several assumptions. Firstly, all human responses must have animal analogues, since those that do not cannot be monitored. Secondly, a more sensitive animal species must be known. These may be well known for chemicals in long term use but as new industrial compounds are produced it may take some time for any candidate species to be identified.[56] Thirdly, the humans 'at risk' must be known. This may be influenced by age, sex, concurrent disease, habits (such as smoking), diet, synergistic occupational or environmental exposures, etc., or any combination of some or all of these. Fourthly, the model assumes that low dose, long term effects, may be identified from acute, high dose studies. It is worth noting here that the interspecies comparison of even acute effects is open to question with no more than fair comparability being achieved, that is, some responses are comparable and predictable, others are not.[57] The most widely used method of assessing long term, low dose, effects is by acute toxicity tests, on the principle that, if effects are not found at high levels, they will not be found at lower ones. Clearly this is not the case for diseases of insidious onset such as cancer or heart disease and a further problem here is the life span of laboratory animals (particularly the commonly used rabbits, rats and mice), whose lifespan is such that detection of long term problems is physically impossible. Accepting, for the moment, that these acute studies have value in predicting qualitative effects, neither they, nor human acute exposures, can predict what is a no-effect level. Calabrese illustrates this problem by describing three different dose-response curves which may be fitted to the same set of data. (Figure 4.1)



<u>Figure 4.1</u> <u>Possible dose-response relationships at low level exposures.</u>[58]

Curve A represents the traditionally used threshold dose-response relationship. On this curve there exists a dose below which no effects would be expected (at about dose 3). Curve B is a linear dose-response curve. Reducing the dose reduces any effects but any dose produces some effects. Curve C shows a relatively higher risk of health effects at low doses (which may be found with some exposures to radiation).[59] Calabrese makes the point that, since any of these curves fit the data, lower dose effects cannot be predicted with any certainty and depending on the model of dose-response accepted, each of them are equally plausible. Thus, not only is extrapolation required but this extrapolation depends on the model held by the scientist. When several disciplines are involved in the area this may lead to dispute over prediction and hence controversy. It is possible (though perhaps, unlikely) that these differences of opinion could be solved by further experimentation. In practice the issue seems to be one falling into the area of Weinberg's 'trans science'. Saffiotti describes why this may be so:

> "In order to detect possible low incidences of tumours, such a study would use large numbers of mice, of the order of magnitude of 100,000 mice per experiment ... [this] would cost about 15 million dollars ... [and] would still leave most of our problems in the evaluation of carcinogenesis hazards unanswered."[60]

Situations similar to this above also arise in both epidemiology and controlled human exposures where studies do not produce results which are statistically significant. For example, an epidemiologist may study several workers exposed to a radiation hazard. If one, or a very few of them, subsequently die and this number is not statistically significant, what can the researcher do? At best, the findings may be taken as indicative of a potential problem and attempts may be made to find and study other accidental exposures, which may give rise to debate over comparability of age of subject, length of exposure, and so on. A similar situation is the study of a new drug for a rare disease. If this drug is only a few percent better than alternative treatments it may save a few lives but not enough to reach a statistically significant level. Since the disease is a rare one it may not be possible to ever reach this significance level.[61]

I am not here interested so much in statistics per se, (although it is worth noting that, in recent years, there has been some controversy over significance testing),[62] but rather in the role of statistical significance in different disciplines. The prime issue here is the differentiation between what is statistically significant and what is important. Though this is clearly a value judgement it is also closely related to the tenets of different disciplines. In the exact sciences (such as physics) statistically significant differences or similarities are also regarded as important differences or similarities and whilst this may also be the case in life sciences (such as medicine), there are also a wide range of situations where statistically significant differences are unimportant and where non statistically significant differences are important. Meehl describes this first as a 'methodological paradox' since, in physics, the highly exact techniques, methodologies and so on, make statistics a very powerful tool in distinguishing between different predictions; whilst in less exact sciences, the variability of subjects and conditions is such that powerful statistical tests increase the liklihood of spurious differences being detected.[63] On the second issue, it is important to distinguish between statistical significance and the medical concept of clinical significance. Clinical significance will vary according to the type of disease or treatment being considered. For example, a six percent difference in the response rate to a drug and a placebo in the treatment of say, headache, is far less clinically significant than a similar difference in response rate in a fatal disease. Clinical significance relates to the value placed on a therapy and in this context, a treatment whose effects are statistically significant may be clinically insignificant.[64] Finally, it should be remembered that a statistically significant association does not necessarily imply a causal association and, here again, scope for multidisciplinary disputes may exist.

One of the major reasons for this difference in statistical views is the use to which any end results are to be put. As in the earlier discussion, when experiments, extrapolation and so on have been carried out, some decision may be made - to use a drug or to ban it?, to enforce exposure limits on some hazard? or to act or not to act? For the synoptic model this judgement is purely a political one and the researcher would merely be expected to present the results as they stand with no recommendations or advice. Even ignoring the likelihood of implicit values and judgements this idea seems to take rather a naive view of the politician, who is likely to ask for advice, and of the scientist who, as a human being, will clearly have views on what, if any, action should be taken. Even if this advice is not presented directly to the policy maker it may surface via other channels, for example, publications in newspapers and journals, pressure groups, and so on. Thus, a further source of disagreement between scientists is over whether or not action is justified.

4.42 Opportunity 2 - Justification for Action?

This issue is another of the possible consequences of the multidisciplinary nature of policy related scientific research. Different scientific actors are likely to be influenced by some shared culture not only with regard to the meanings of results but also with regard to the use to which results may be put. The central notion here is that of error cost. This cost will vary depending on the aim of the discipline. Three groupings may be distinguished. Firstly, the theoretical scientist who aims to 'advance' knowledge. The most costly error here is in the acceptance of a theory based on weak foundations since this will lead to further research based on dubious premises. Delay in reaching conclusions, on the other hand, is not as serious as premature closure, any problems are seen as solvable in the fullness of time via the collection of more and better data obtained by more rigorous and better thought out experiments. The second grouping, who take a similar position to this are applied scientists concerned with some product or process, such as engineers or industrial chemists. Here, the most costly error is acting too soon, in building a bridge that collapses or a chemical plant which explodes. In most circumstances, the cost of delay for further study will be minor in comparison to the potential catastrophe of precipitate action. In contrast to these two groups are those concerned with relatively immediate action such as physicians (and to a lesser extent, psychologists). Their main aim is to prevent suffering or even death, only in a limited set of circumstances will they be able to conduct a full set of exhaustive tests and, particularly in life threatening situations, action will be urgently required. This viewpoint is nicely illustrated by the definition of diagnosis as 'an estimate based on observed facts.'[65] Hence, physicians, by and large, are expected to make diagnoses on limited information and, furthermore, to act on that limited information.

Two broad groupings may be identified in this above discussion. Firstly, a group who have no need or desire to either formulate ideas based on limited information or any desire for rapid action based on that information and a second group who aim to act rapidly based on what knowledge is available. It is recognised that this grouping is simplistic since 'crossovers' may occur, for example, an engineer faced with a hazardous situation such as a bridge about to collapse will act rather like a physician, on an informed guess for rapid action. Here the error cost has changed so that it is more costly not to act than to act on limited information. Nor am I suggesting that this enforcement of 'action versus inaction' is via some form of Mertonian normative structure. What I am suggesting is that some form of shared culture inculcated into embryonic practicioners tends to encourage particular actions such that for example the scientist who frequently bases research on theories regarded as dubious (as did Barkla in the previous chapter) is not likely to be well regarded and the physician who refuses to treat a patient on the grounds that further research is needed, is likely to face censure from both patient and colleagues.

Is there any evidence for these suggestions? Several authors have pointed to rather similar distinctions, for example, Chein distinguishes between 'scientism' and 'clinicism'[66] whilst Kris has discussed the differences between 'pure research' and 'action research.'[67] Friedson suggests that the physician:

> "... Whose work requires practical application to concrete cases simply cannot maintain the same frame of mind as the scholar or scientist : he cannot suspend action in the absence of incontrovertible evidence or be sceptical of himself, his experience, his work and it's fruit. In emergencies he cannot wait for the discoveries of the future."[68]

Goldman concurs with this view and contrasts the 'intellectual nihilism' of the scientist with the need to act the practicing physician.[69] This need to act tends to be in one direction, towards diagnosis of illness and thence, treatment. There are four possibilities of diagnosis; a correct diagnosis of illness, a correct diagnosis of no illness, a mistaken diagnosis of health and a mistaken diagnosis of illness. Clearly these first two involve no errors whilst the latter two involve error and therefore an error cost.[70] Of these, the most costly is to dismiss illness when it is present since then the patient passes out of medical care[71] and this is characterised as the clinical maxim 'When in doubt treat. When in doubt intervene.'[72] Scheff gives two examples of this maxim in operation. In the first, 1000 children were examined with regard to the advisability of tonsilectomy. One group of physicians recommended that 611 of these had their tonsils removed. The remaining 389 were examined by a second group who recommended 174 further operations. The 215 left were examined by a third group of doctors who judged 99 to need tonsilectomy. The remaining 116 were examined by a further group of doctors who recommended almost half of them for operations. Thus, eventually, well over 90% of the original group were judged to be suffering sufficiently to need operations. The second example refers to a sample of 14,867 chest x rays for tuberculosis. Of these, 1216 positive readings turned out to be clinically negative whilst only 24 negative readings turned out to be clinically positive.[73] Even allowing for errors of diagnosis, these findings would seem to suggest a bias towards finding illness.

The suggestion put forward here then, is that in a policy related controversy involving several disciplines, two groupings may emerge, one group advocating early action and a second group advocating further research. (See also following chapters). Clearly, the issue is not as simple as this since, for example, interpretations of evidence will exert a major influence on opinions as to whether or not action is justified and, in Pinch's suggestion is correct, the audience for any comments influences the expression of certainty or otherwise (see Chapter 3); thus, the views 'further research is needed' and 'we must act now' may be selectively advanced, possibly in attempts to legitimate or 'de legitimate' various policy preferences.

4.5 Discussion

In this chapter I have attempted to indicate some of the issues which may arise when attempts are made to utilise science in the policy process. Firstly, the multidisciplinary nature of most research means that a variety of different methodologies, interpretations and so on are applied to the area. It might be thought that a variety of approaches would enhance the possibility of solution but instead, the clash of several 'shared cultures' merely gives rise to acrimonious criticism and a mass of non comparable results. Secondly, the motivation for research in this area is provided by political 'push'. Scientists are encouraged to begin research which they would otherwise tend to avoid and any results are given great visibility either to support certain policies, or to be demolished because they do not support certain policies. Yet again there is an impetus towards high temperature criticism. Thirdly, the opportunity for this criticism, for disparate interpretations of results and for the support of a wide variety of policy alternatives, may be found in the many problems of research in these areas, accompanied by the possibility of disputes as to what these results actually mean in terms of policies, actions and theories. Overall, the major problems are the multidisciplinary nature of the problems faced and the political relevance of the areas both of which mean that criticism is likely to be extensive and wide ranging since none of the moderating factors found in less policy-relevant sciences are able to operate. These ideas are very similar to those put forward by Collingridge and Reeve in what they term the Over Critical Model. (Figure 4.2).

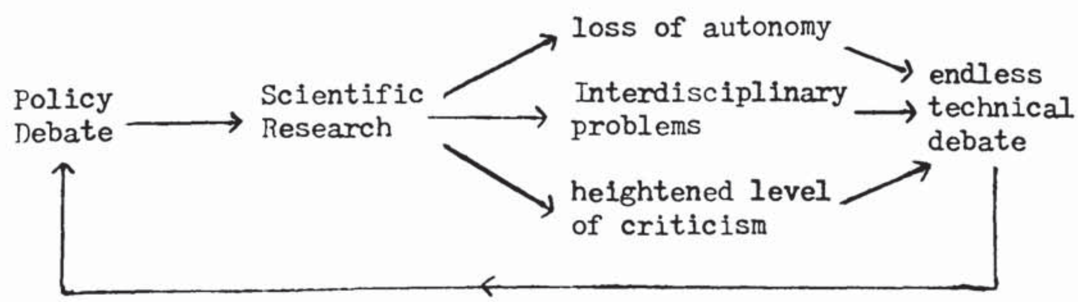


Figure 4.2 The Over Critical Model.[74]

The model shows all the features discussed above including the positive feedback loop considered in Section 4.3. The policy relevance of the research and the disputes over the 'right' policy provide impetus for scientific research which, due to the inherent problems of the policy area, can only give rise to debatable results. These in turn provide support for a variety of policy initiatives stimulating further scientific research and so on. This model (and the above chapter) support the incrementalist rather than the synoptic view. The implication is that the main role of science is not to provide a 'true' bedrock on which to build policy but to provide weopons with which political interests may fight for legitimacy. If some form of approximate policy consensus is achieved however, then scientific and technical findings become much less relevant and hence much of the motivation for technical dispute is removed and what dispute does take place receives little publicity. This leads to the situation described above as a negative feedback loop which Collingridge and Reeve call the Under Critical Model. (Figure 4.3).

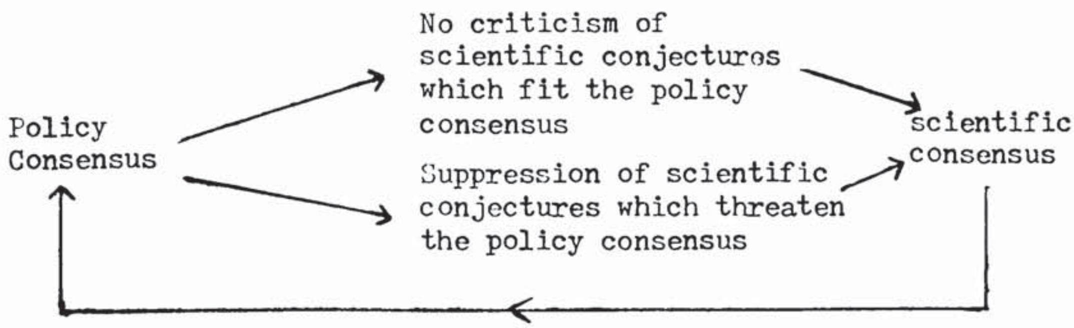


Figure 4.3 The Under Critical Model.[75]

Under these conditions the motivation and opportunity to engage in technical dispute are much lessened which in turn reduce the possibility of policy disputes developing. The 'suppression' of scientific conjectures in this model need not be sinister in nature but may simply relate to the non funding of certain research areas 'because we know the answer', or to the lack of visibility of research deemed to be false or trivial. Essentially, the idea of this chapter and of Collingridge and Reeve, is that there is a fundamental antagonism between the needs of science and the needs of policy such that, when policy makers wish to know particular answers this distorts scientific research and, when answers are not required, again the research process is distorted. If this is the case then attempts, at even a limited synoptic view, are doomed to failure and, even for the incremental model, any great reliance on scientific findings may cause problems. These ideas have now been developed to a point where they may be tested by comparison with controversies in policy related areas. This will be done in the following chapters.

CHAPTER 5

SCIENTIFIC CONTROVERSIES IN THE POLICY ARENA

5. SCIENTIFIC CONTROVERSIES IN THE POLICY ARENA

5.1 Introduction

Before considering policy related controversies in this and later chapters it is worth reiterating the aims of this thesis. As noted in Chapter 1, the reasons for studying the areas are, firstly, to test different ideas of the status of scientific controversies. Are these abnormal or part and parcel of science? Secondly, in the light of the answer to this question, what role does science serve, and can science serve, in decision making? Some tentative answers have been arrived at in earlier chapters. Controversies, once regarded as aberrant phenomena, are now widely seen as 'natural'. Even students of science and policy not accepting this view recognise that no scientific claim is beyond dispute, ambiguities and grounds for questioning knowledge claims are always present. Thus, decision models cannot depend on a bedrock of solid scientific truth but must, instead, be able to accommodate the shifting sands of changing ideas. In the following chapters I will attempt to illustrate the role of science in certain policy controversies. These examples have been chosen to illustrate some of the issues considered in earlier chapters and I will concentrate on these specific issues rather than on the controversies in detail.

5.2 The Fluoridation Controversy

Like many disputes in the policy area, the dispute over the health effects of water fluoridation has a lengthy pedigree and is still going on after some sixty years of research.[1] For those advocating the fluoridation of water the issue is a simple one. The presence of fluoride in water at a concentration of around one part per million reduces the incidence of dental caries in children, this is not harmful to health and, where fluoride is not present in drinking water it may be added cheaply and easily.[2] Those opposed to this measure state that fluoride has not been proved to be harmless at low doses[3], that it merely delays the onset of dental caries,[4] that it is not cost effective to treat all water whether it is drunk or not[5] and that mass medication is a breach of individual freedom.[6]

In the U.S.A. in the 1950's and 60's decisions on fluoridation of local water supplies were taken by local referenda. In these, only 773 of 1899 communities of over 10000 people voted for fluoridation.[7] Most interesting here is not the controversy per-se but the responses made by medical and social scientists to these results. By and large, medical and dental practicioners were content to remain in the background and 'let science speak for itself.'[8] When they did get involved their main action seems to have been ad-hominem attacks on anti fluoridationists describing them as 'irrational'[9] and 'misinformed and stupid'.[10] The Journal of the American Dental Association devoted a large part of one issue to comments about anti fluoridationists[11] including disparagement of their qualifications to discuss the issues,[12] association of opponents with strange schemes and dietary fads[13] and the suggestion that opponents see fluoridation as a communist or Jewish plot.[14] In spite of these comments it is asserted that the issue is a purely scientific one with opponents acting unscientifically or being anti science.[15] The evaluative issue of freedom of choice received little attention with the debate seen as scientifically validated policy versus irrational attitudes.

Social scientists studying the referenda took a similar view to the above and approached the area with the implicit and even explicit assumption that fluoridation is a 'good thing', a pro fluoridation vote was a rational vote and only 'anti' votes needed explanation.[16]

The most commonly utilised explanations for these votes were anti science attitudes,[17] alienation of voters and authority[18] and lack of education.[19] Each of these has received some empirical support but have not escaped critical comment.[20] The main means of overcoming this perceived failure of voters to make the 'right' choice was an education campaign,'if only they understood the issues they would vote correctly.'[21] It was, however, suggested that misinformation was equally prevalent amongst pro and anti fluoridation voters and, because of this, an information campaign ran the risk of transforming uninformed propenents into ill informed opponents and that the task should be seen as a political one requiring not education but propoganda.[22] Other writers have questioned the role of the referendum with the suggestion that whilst education is desirable, decisions should be taken by administrators who are better able to select experts to consult.[23] Implicit in these ideas is the belief that fluoridation of water is a good thing. Alternative routes of administration such as via milk, toothpastes and so on, whilst effective, have been criticised on the grounds of cost, though this has been disputed.[24] Though cost is clearly relevant, one of the main reasons for rejecting these alternative routes seems to be the implicit assumption that fluoride should be widely administered and this is most reliably achieved via water supplies. Green questions this view and asks "Why should anyone be distressed that the public, for whatever reason, rejects technological benefits,"[25] and Mazur also questions the apparent urgency with which measures such as this are pursued.[26]

What conclusions may be drawn from this brief foray into the fluoridation controversy? Firstly, the word 'scientific' was seen as a euphemism for the word 'true' so that support for fluoridation was seen as rational, whilst rejection was seen as needing explanation as some form of abnormality such as irrationality, anti science and the like. Secondly, the dispute was seen (by the pro fluoridationists) to lie only in the realm of science. Evaluative issues were either ignored or brushed aside leaving only technical issues where science could provide the answers. Because of this view, only the best technical answer was considered. It seems likely that a more incremental approach via provision of individual supplies of tablets and so on might have advanced the fluoridation case further and faster. Not only would this have bypassed and avoided anti mass medication arguments, but emphasis on these alternatives would have allowed those who wished to utilise fluoride to do so immediately. If this is the case then 'rational' insistence on this best technical answer tended to increase opposition which would otherwise have been muted. In this example a model of science was utilised which emphasised the role of science in determining policy. A similar model may be seen in the next example.

5.3 The Windscale Inquiry

The Inquiry was held in 1977 to consider an application by British Nuclear Fuels Limited for planning permission to build a nuclear fuel reprocessing plant. This was described as a thermal oxide reprocessing plant, known by the acronym THORP. The inquiry report, produced by Mr. Justice Parker in 1978, was in favour of the plant.[27] The conduct of the inquiry and the writing of the report has been criticised by several authors, not on the grounds that the conclusions were necessarily wrong, but that arguments and issues were misunderstood, misrepresented and ignored. Wynne explains this by suggesting that Parker utilised a model of 'judicial rationality'[28] representing a belief in the separability and the separateness of facts and values, the belief that values are secondary to facts and the belief that disputes over facts can only be due to bias or ignorance since the meaning of facts and data is apparent and is the same to all competent observers.[29] Objectors to THORP who suggested that social factors might influence scientific viewpoints, were heavily criticised by Parker who could only see this as an attack on the integrity of expert witnesses.[30] Similarly, a suggestion that the inquiry might carry less weight because those aiding Parker to assess the evidence were associated with the nuclear industry, was reported merely because it "... reveals the state of suspicion which exists in certain people".[31] For Parker the normal state of science is total agreement and "It was apparently inconceivable to him that there could be legitimate differences in interpretation ..." relating to methodology, research traditions and so on.[32] Public hostility was also seen in this light, as a result of either ignorance or malevolence, in a response closely akin to that of pro-fluoridationists. This has been described as "... the characteristic rationalist response to conflict."[33] When value issues such as the desirability of economic growth were raised by some witnesses these were ignored in the final report.[34] It is clear from these few comments that Parker's 'rational' approach was likely to favour the nuclear industry and either misunderstand or misrepresent the views of the opponents of THORP. Leaving aside issues of democracy, what are the dangers of this approach? Firstly, assuming that inquiries are not merely legitimating devices, then they are held in recognition that two or more valid (scientific and political) viewpoints exist. If these are then misrepresented the procedure is not an efficient one and, whatever it s intent, becomes only a legitimation of a single viewpoint. To use incremental terminology, partisan mutual adjustment is at it s most efficient when all partisans can play a full role. Secondly, given the realisation that taking part in inquiries is fruitless, those who feel that they are not being properly represented may take more direct action. This may be as mild as protest marches but may extend to civil disobedience or even violence.[35]

The following points emerge from this short discussion. The issue was seen as a value free one with the implicit value assumptions of the nuclear industry seen as the only ones possible. Parker's view of science, if it did not prejudge the issue, certainly acted in a prejudicial manner. He saw the issue as a purely technical one with technical ideas being seen as objective, separate and separable from values which took second place in the debate. In this respect, the Inquiry had a great deal in common with the ideas put forward for a 'science court'.

5.4 Science Courts and Mediation

The science court was proposed by Kantrowitz as a means of dealing with 'mixed decisions',[36] relating to questions with both scientific and value aspects and where scientific uncertainties are present so that advice is likely to be influenced by the political and moral aspects of the issue.[37] These proposals were considered by a U.S. Government Task Force which came up with specific ideas for a trial of the idea.[38] They proposed that, at least initially, only issues with easily separable facts and values should be selected, that funding should be provided for opposing groups and that judges of the issues should be 'unusually capable scientists' who could be removed if suspected of bias.[39] It was suggested that the court should only deal with questions capable of answer by experiment or observation.[40] Following the court hearings three types of statements should emerge.

Firstly, those agreed by both sides, secondly, disputed statements where the judges have given their opinion and, thirdly, areas where further research is required. Several criticisms have been made of the science court proposals. Firstly, selection of issues for consideration is, itself, an evaluative issue,[41] and conveys power into the hands of those selecting issues.[42] Secondly, policy questions tend to be broad in scope and highlighting differences may merely exacerbate these. Thirdly, there is an implicit assumption that issues naturally polarise into easily identifiable 'fors' and 'againsts.'[43] This polarity may be more apparent than real with views instead tending to fall along a continuum for both political and scientific reasons. Thus, the court, depending on two opposing viewpoints, may allow more (or less) extreme views to fall by default. Fourthly, the 'unusually capable scientist' with expertise in an area but no views on it seems to be a creature of fiction not fact and depends on the belief (dealt with in Chapter 3) that the scientist is a dispassionate 'truth seeker'.[44] The science court is based very closely on the legal court where the aim is to win a case not arrive at truth. It seems unlikely that it s scientific analogue could act any differently and this may raise problems of varying skills in advocacy, speech making and so on. Fifthly, a central tenet of the science court is that facts and values are separable. Nelkin suggests that a 'Catch 22' situation may exist such that if separation is possible then controversies would not develop and if it is not, then a science court cannot help.[45] Here a crucial question is, what should be regarded as facts? These have been defined as issues which can be addressed experimentally and which can be quantified[46] but into this category comes 'trans-scientific' questions (see Chapter 4) where answers can only be arrived at by expert opinion and here a science court can merely add to the list of opinions.

Concern has been expressed that the views of the science court would be imbued with unwarranted authority, leading to pressure on scientists who do not toe the 'official' line[47] and this may be enhanced by the power of the court to order research which may create a biased consensus. Concern has also been expressed that this authority could lead to neglect of the political aspects of any dispute and concentration on the technical to the extent that narrow 'technical fixes' are applied.[48] In fact, the science court is likely to find itself in a paradoxical situation. If action is urgent then the science court is likely to be of little use in settling issues, whilst if action if not urgent, then issues will not have sufficient visibility to cause referral to the science court. A modified version of this proposal designed to overcome some of these problems is mediation which aims to highlight areas of dispute.[49] The claimed advantages of mediation are that both values and facts can be considered and that more than two groups may take part. Furthermore, the aim is not to 'win' but to clarify matters, so no group or viewpoint need be discredited as may happen with the science court. The following pages describe two controversies where these techniques were attempted. The first of these is over the deployment of anti ballistic missiles (ABM) in the U.S.A. in the 1960's and early 70's and an attempted mediation by the Operations Research Society of America (ORSA) and the second deals with the siting of a high voltage power line, also in the U.S.A. Neither of these are perfect examples of these conflict resolution techniques but these are the best documented examples available.

5.41 The ABM Controversy : Mediation by Professional Body

In 1967 the United States announced that it was to build a light, country wide ABM screen known as Sentinel, designed to perform three functions. Firstly, to serve as a defence against an attack by China, secondly, as a defence against an accidental missile launch by the U.S.S.R. and, thirdly, to act as a limited defence of U.S. land based intercontinental ballistic missiles.[50] An ABM screen to defend population centres was specifically rejected since it was feared that this could lead to a Soviet arms build up.[51] When President Nixon came to power, the decision was reviewed and a modified system, Safeguard, was proposed, aimed primarily at protecting America's offensive missiles with the secondary role of protecting the population against a small scale nuclear attack. A proposal for twelve missile sites was accepted by the Senate in 1969 and the missiles were deployed shortly afterward.[52] Following this, talks began with the U.S.S.R. and an ABM Treaty was signed in May 1972 limiting each side to two ABM sites, one to defend the national capital and one to defend a missile site.[53] In 1974 this number was reduced to one and in 1975 the Senate and the House of Representatives voted to dismantle the remaining U.S. installation.[54]

Opposition to ABM deployment had begun to build up in the 1960's, initially amongst scientists and later in the general public. It has been suggested that this opposition related to wider social issues such as opposition to the Vietnam war[55] and that it is explicable in political terms,[56] but the debate was conducted by practicing scientists, in terms of science and facts and so cannot be dismissed as mere 'conflict by proxy'. What seems more likely is that dispute in one sphere fed dispute in the other raising both the political and scientific temperature.

The role of science in this dispute seems to have been as a political resource, with experts cited in attempts to legitimate political views.[57] Cahn describes this as a 'fig leaf' function "... scientists were influential when their views corresponded to those of the decision makers who were at the critical points in the policy making process."[58] Some scientists were also influential less directly via the mobilisation of public opinion[59] and this may be one of the major ways in which scientific influence is exerted in public policy. On this view scientists are not advisers outside the policy process but partisans within it.

ORSA was 'called into' the dispute by one of the proponents of ABM deployment and a member of ORSA, Professor Albert Wohlsetter. ORSA responded by setting up a committee of 12 operations researchers to mediate and adjudicate between the arguments for and against deployment. They concluded that the arguments used (primarily by opponents of deployment) were "... often inappropriate, misleading or factually in error."[60] Responses to the report varied from favourable, praising the report for it's even-handedness[61] and suggesting that all scientific disciplines should adopt such an approach,[62] to the highly critical on the grounds that the investigation should not have been carried out at all, and the way in which it was conducted. This point was made not only by one of those investigated by ORSA but also by five of the committee members.[63] On the conduct of the investigation, Doty suggests that ORSA concentrated on a limited set of issues to the detriment of the anti-deployment case[64] and he characterises the report as an attempt by the members of ORSA to judge those who were not, on matters not requiring expertise in operations research.[65] Clearly, as a mediation attempt, the report failed and, instead, became a judge giving praise and laying blame. Could it have succeeded? In principle

ORSA could have brought out the 'facts' and their attached values, however, in multidisciplinary areas, the practicality of this is open to doubt. If the idea of 'shared culture' considered in chapter three, is true to life, then disputes as to what is a fact?, what is relevant?, what is an acceptable interpretation? and so on, will be difficult to reconcile and the best which can be hoped for may be a concise list of areas of dissensus. Advocates of the science court have suggested that this may provide a means to consensus. Although the idea was criticised above, it is worth considering an attempt to apply the science court idea to see how it worked in practice. The example relates to the siting of an electric power line.

5.42 The Powerline and the Science Court.

The powerline dispute began in 1973 when two Minnesota electric utilities ('the Co-ops') announced plans to build a powerline across Minnesota.[66] When local farmers were informed of these plans they initiated a series of hearings and protests.[67] For them, the most important issues were political ones, such as eminent domain (the right of the Co-ops to purchase required land) with technical issues such as health and safety being seen as far less important. The farmers failed to make headway in these hearings and some of them turned to direct action such as obstruction of survey vehicles, in an attempt to delay construction.[68] Against this background the science court idea was mooted.

In 1976 the newly elected Governor of Minnesota, Rudy Perpich, brought in a professional mediator who advocated a science court to deal with health and safety issues. The farmers would only agree to this if it was accompanied by a moratorium on construction and at this point talks broke down. Following a series of unsuccessful legal challenges by the farmers, the idea was raised again and, once more, a moratorium was demanded. The Co-ops questioned the need of the court since the issue had already been decided and the idea was dropped once more.[69] Shortly after this, the Co-ops took legal action against the farmers for their obstructive activities and the Minnesota Department of Health produced a report stating that there was insufficient evidence to claim any health hazard.[70] The farmers response was to demand a science court to investigate the area but the Co-ops rejected the idea. This rejection brought them much 'bad press' and they later agreed to participate provided that discussions were restricted to health and safety and there was no moratorium on building. This was seen by the farmers merely as an attempt to divert them from direct action and, once again, the idea was shelved.[71]

Shortly afterward, the farmers proposed a court dealing with both technical and non technical issues, to be judged by the Governor. This was rejected on the grounds that the science court was designed only to deal with technical issues and, at this impasse, the idea was finally dropped.[72] The line has since been completed but acts of vandalism against installations continue and the issues remain unresolved.[73]

It is fairly clear that each of the actors in the dispute saw the court differently. For the Governor, it was a means of conflict resolution not of dealing with scientific uncertainty[74] and a means of 'technologising' the issue to avoid hard political choices "... in that respect, the science court is a politicians dream - it focuses public attention in peripheral technical issues and delegates the power to the 'experts'."[75] The Co-ops' opposition was based partly on the untried nature of the idea, partly on a concern that participation would confer credibility on the farmers' case and, perhaps most importantly, they had already 'won' and could not improve their situation. Later, this perception may have changed, with the court seen as a means of diverting the farmers' attention away from direct action against building.[76] For the farmers, the prime issues were evaluative and thus their efforts concentrated on widening discussion to include their concerns. If these suggestions are correct then decisions as to participation, format and timing were made after consideration of individual costs and benefits and under these circumstances, it is hardly surprising that each actor viewed the court differently. The important point here is that the science court is predicated on a commitment to scientific truth whilst in real life, concerns are likely to revolve around partisanship and winning one's case.

Finally, could a science court restricted to scientists have been more successful? Mazur acted as a 'referee' between scientists who had taken different views on the health issues relating to the powerline and eventually produced a series of empirical propositions relating to time limits of any health effects (within five years), and psysiological parameters (such as growth) which could be affected by living near the line.[77] This process is clearly very close to Popper's method of bold conjecture and attempted refutation.[78] Leaving aside the utility of this method for 'pure' science, there are several arguments against it in the policy area. Firstly, on ethical grounds. Presumably, if the issue is a contentious one, the possibility of a health hazard exists. If this is so, then however testable the ideas, the decisions can only be evaluative rather than technical. Secondly, is this criteria helpful in making policy decisions? In the previous chapter the methodological paradox between physics and psychology was considered where attempts to apply inappropriate standards of exactitude might result in erroneous rejection of hypotheses. Similar criteria apply in this case where, if no health effects are found within five years, then the only conclusion which can be drawn is that no health effects were noted within that time, not 'no health effects exist.' Furthermore, it seems likely that health effects in six or ten years are just as important to any decision, making highly specific criteria unhelpful. Of course, this problem is a common one in policy areas and the point is not that decisions should not be made but that these are essentially political and attempts such as Mazur's may, at best, bring disagreement into the open and, at worst, may be used to shield political decisions from the light. The political uses of science will be further discussed in the next section, using the example of the American dispute over the building of a super-sonic transport. (SST).

5.5 The SST : Science and Partisan Politics.

The idea of building a United States SST was raised in the 1950's and the programme was begun in 1961. By the mid 60's the programme had attracted widespread criticism both within and outside government.[79] The main reasons put forward for the SST related to national prestige, defence, support for the aircraft industry, the need for rapid transport and possible economic benefits.[80] Against these were arguments of cost, feasibility and environmental issues such as noise and risks to the ozone layer.[81] Many of these are clearly science linked and the debate was conducted using scientific arguments but not in the simplistic way predicted by the synoptic model. Firstly, arguments were selected to support political values. For example, federal agencies supporting the SST chose experts who were either 'enthusiastic aircraft visionaries' or who derived a large part of their income from aircraft industries. Other tactics included the choice of analytic techniques giving results closest to those desired and subdivision of analysis so that 'correct' synthesis could give the desired results.[82] This last shades into the second political technique, control of sources of information, which was carried out by refusal to release critical studies and release of biased summaries.[83] Thus, expertise was used, not to answer questions, but to support (or at least not damage) political positions. How influential was this expertise? It seems that, for several reasons, expert influence was minimal. Firstly, advice was frequently not understood by politicians, secondly, advice did not provide answers to many of the important questions, so that questions asked early in the debate, with regard to economic and environmental impacts, were still unanswered ten years later and, most importantly, the principals in the debate were motivated by political consideration such as support for the aircraft industry and so expert advice caused few changes of mind.[84] Advice was apparently of equally little influence on the less committed. From 1966 to 1969, when environmental concerns amongst the public and scientists were growing, the number of senators supporting the SST actually rose. This may be explained by the lobbying activities of two SST proponents, Senators Magnuson and Jackson, both representing Washington - the home of the project's major contributor, Boeing.[85] By 1970, however, eighteen Senators who had voted for the project in 1969, voted against it. Rosenbloom suggests that this was a response to public pressure linked to the fact that six of the senators were seeking re-election in 1970 and eight more in 1972.[86] Thus, it would seem that any influence of expert advice was indirect via the creation of public pressure and that politicians utilised advice in an attempt to legitimate politically arrived at views. This was also noted in the ABM dispute and seems explicable more in terms of partisan mutual adjustment than by attempts at a synoptic view. The next example deals with a further example of the political use of scientific advice. It deals with the dispute between the United States Environmental Protection Agency (EPA) and the Ethyl Corporation (EC) over the removal of lead in petrol.

5.6 The EPA versus the Ethyl Corporation : Selecting the Evidence.[87]

Lead compounds were first added to petrol in the 1920's to prevent pre-ignition (knocking) and to increase power output. By the 1960's concern was being expressed that this lead, emitted in exhausts, could be a health hazard and, in 1972, the EPA proposed a set of regulations to reduce lead in petrol. There were two grounds for this. Firstly, that lead was a health hazard and, secondly, that catalytic converters designed to reduce the emission of nitrous oxides and carbon monoxide, were inactivated by the presence of lead. The Ethyl Corporation, a manufacturer of additives, opposed this proposal on the basis that removal of lead would decrease fuel efficiency and would, therefore, be costly in economic and resource terms; that alternative means of cleaning up exhaust gases could be developed and that lead was not a health hazard. Clearly this was a crucial issue and the debate centred on this, via a series of publications and replies based entirely on scientific research. There were three main areas of contention. Firstly, the relationship between air and blood lead. Both parties accepted that over 90% of airborne lead comes from automobile emissions. The EPA suggested that there were both empirical and theoretical grounds for suggesting a relationship between air and blood lead and they were critical of studies not supporting this relationship. EC criticised this conclusion on three grounds. Firstly, they quoted studies not supporting any relationship and were critical of the methodology of those which did. Interestingly, the main criticism was that no account was taken of alternative sources of lead intake, which was the criticism that the EPA applied to studies not finding any relationship. Secondly, when relationships were found, they were not statistically significant (at a 95% level) and thirdly, blood lead levels were all within limits considered to be normal (up to 40 micrograms per 100 mls of whole blood).[88] It seems that, for the EPA, action was justified on the grounds that a hazard could exist, whilst for EC 'on average' all was well and that studies and evidence were interpreted to support these views.

The second argument related to the contribution of airborne lead to dust lead and, thence, to increased lead body burden in children.[89] The EPA supported their case for a relationship primarily by circumstantial evidence. Some children are known to eat dust and dirt, dust samples have been found containing up to 1% lead and lead poisoning in children has been found following ingestion of substances with 1% lead. The EC reply suggested that lead paint is the sole cause of childhood lead poisoning. They supported this with a large number of studies, some of which, on detailed reading, cite cases of lead poisoning attributable to dust and dirt.[90] Only four sources are quoted by both parties. One of these refers to a comparative study of rural and urban childhood lead poisoning. In the rural children 18 out of 19 cases were already associated with lead paint whilst this was so in only 60% of the urban children, i.e. some other source of lead may have been responsible. Whilst the EPA report this study in full, EC only refer to the rural children, presumably since this part of the study supported their case. Overall, it seems that both the EPA and EC are in agreement, that dustfall lead may be a minor source of lead poisoning but, whilst for the EPA this is grounds for policy action, for E.C. it is not.

The third issue relates to safe body burdens of lead. Both parties agree that at low blood levels lead inhibits certain enzyme systems but dispute the significance of this. The EPA further suggest that blood lead levels hitherto regarded as safe do not, in fact, protect all persons, whilst EC dispute this.[91] Essentially, the issue seems to be one of definition - what is a health effect and what a biochemical effect? and the EPA interpret this more cautiously than do the EC. What conclusions may be drawn from this dispute? It seems that both the EPA and EC had policy positions which they utilised scientific studies to defend. These studies were quoted or criticised depending on their findings. Science was used not in an attempt to form a synoptic view but in an attempt to legitimate certain partisan positions. This is not to say that scientific findings were not influential in placing the issue on the political agenda, but science could not aid the solution of the dispute. A similar situation was found in the dispute over the health effects of smoking.

5.7 Smoking and Health

The dispute over the health effects of smoking revolved around two rival theories. On the one side were the medical profession, especially epidemiologists, who suggested that statistical evidence supported a causal relationship between smoking and lung cancer, and on the other side, were some geneticists and psychologists with an interest in genetics who agreed that the propensity to smoke and the propensity to suffer from lung cancer were genetically linked. The research of the former group was supported largely by the Medical Research Council and the latter by the Tobacco Research Council (funded by the tobacco industry.)[92] Obviously, if it could be convincingly shown that tobacco was causally linked to lung cancer, there would be great pressure on the government to act to reduce smoking. To this end the medical profession made great efforts to get smoking onto the political agenda and the tobacco industry tried equally hard to keep it off by promoting alternative (primarily genetic) explanations, by emphasising the statistical nature of the link and by conducting research to present an image of responsible awareness.[93] Collingridge and Reeve suggest several reasons for the medical profession's support of a causal link. Firstly, they had a vested interest in demonstrating the success of a medical discovery, secondly, the link fitted medical beliefs of disease as an abnormal state caused by an outside agency, thirdly, the genetic theory challenged these ideas not only explicitly in this particular case but also implicitly throughout medicine and, fourthly, the model fitted the medical bias towards action and intervention.[94] Thus the scene was apparently set for a lengthy scientific controversy and the dispute has continued, though with very little public visibility.[95] This continuation casts some doubt on the conventional view that government acceptance of the causal theory was a triumph of 'true' science in informing policy. Was the apparent success of the causal link a major blow for the tobacco industry? Collingridge and Reeve suggest not and claim that the decisions made by the government were the result of political not scientific considerations[96] with the tobacco industry being extensively involved in negotiation of controls and 'calling in favours' to prevent stringent legislation.[97] In this they were aided by the Treasury which, because of the income derived from tobacco sales, opposed major efforts to reduce smoking. If these suggestions are accepted it becomes easy to see why the controversy died down. The government could be seen to be taking action whilst the tobacco industry could continue in business and diversify their activities against the day when more stringent action might be taken. It was arguably in the interests of the industry to damp down the controversy since, the less visible the dispute, the less likely were attempts to take major action on smoking. Thus, on this analysis, despite appearances to the contrary, science did not directly influence policy, though, as in earlier cases, science was influential in raising the issue in the first place. In this example, Collingridge and Reeve find evidence for their undercritical model, described in the previous chapter as the model of negative feedback. The agreement on policy reduced both the impetus for, and the visibility of, the scientific dispute and this may have been to the detriment of science reducing study of the genetic component of disease processes.[98] The final example in this chapter also supports the negative feedback model.

5.8 I.Q. and Education in Britain - The Early Years.

It might seem strange to discuss the I.Q. debate as undercritical since the very heated 'nature versus nurture' debate is well known.[99] In the U.S.A. the debate was a 'typical' multidisciplinary controversy with scientific findings being used as a legitimating device for political decisions.[100] In Britain however, prior to the 1940's there was little or no scientific debate. In these early years the close match between educational policies such as 'streaming' and educational findings has led observers to suggest a major scientific influence on policy.[101] If this is the case it would be a blow to the ideas being developed in this thesis, that policy is primarily based on partisan negotiation. Fortunately (for this thesis at least), closer examination reveals an alternative explanation. The whole issue has been covered at great length elsewhere[102] and discussion here will be limited to a few observations. Sutherland notes that the interest of educational authorities in the identification of children regarded as subnormal considerably pre-dated the development of I.Q. tests, so that mental testing merely reinforced and extended an already existent trend towards classification.[103] On this view the main role of psychometric testing was in the legitimation of a previously arrived at policy. The tests selected much the same children for secondary education as had been chosen by the earlier teacher selection and formal testing.[104] Suggestions such as these have led Stepan to characterise British psychometricians as "... the sorcerers apprentice rather than the sorcerer ... following rather than leading British education in a selective direction."[105] This suggestion explains why challenges to scientific ideas did not come from the policy area, as far as policy makers were concerned, scientific findings were perfectly acceptable and provided no motivation to look for alternatives. Furthermore, these alternatives were not readily available in Britain where schools of psychology (such as behaviourism) which had challenged the heriditarian position in the U.S.A. had not taken hold enough to become of political concern.[106] The challenge, when it did come, originated in the political arena, particularly in the 1950's. Possible reasons for this are firstly, the moves towards comprehensive education which meant that formal 11+ type streaming was no longer required, secondly, the post war baby boom which prevented many children of middle class parents receiving the grammar school education which their parents expected, and thirdly, the examination successes of many children in secondary modern schools which showed that they had been wrongly placed by the 11+ examinations.[107] These factors combined to bring education policy onto the political agenda. At about this time, psychologists began to raise arguments in opposition to testing. These arguments were not new but the changing political atmosphere increased the visibility of these and led to a collapse of support for psychometric testing.[108] This may be explained by the negative feedback undercritical model. In the early years policy had no need of scientific guidance, when scientific support came along it was accepted as useful justification of pre-existent policies. At this time there was no political platform for scientists disputing the findings of psychometric tests. The situation changed after the second word war when policy changed (for political reasons). This made psychometric findings less acceptable and provided a platform from which scientific disquiet could be expressed. In neither case were scientific findings helpful in formulating policy and neither was policy relevance helpful to science since it distorted the way in which these findings were considered.

5.9 Discussion

In the following chapters two case studies will be considered at length so it would be premature to draw too many conclusions at this point. There are however, some common strands in the case studies which are worth drawing out. Whilst not all of the examples show identical features, several generalisations are possible, especially when these case studies are considered in conjunction with the comments of earlier chapters. Though the issue has not been explicitly raised, many of the case studies are obviously multidisciplinary in nature, with all the (previously considered) implications this has for the conduct and maintainence of scientific controversies. Under these conditions mediation and science court procedures can only serve to highlight scientific differences and, if used to allow one discipline to judge another, may even worsen these differences. The synoptic decision maker in the situations raised in this chapter, would expect to delay any decision until fact and value were separated and all the relevant facts are in. In the fluoridation controversy and the Windscale Inquiry it was implicitly assumed that these were achievable and that the issues were purely technical ones. This belief caused certain evaluative aspects of the disputes to be dismissed as not relevant. In several of the case studies science was used as a means of hiding political and value preferences - Cahns 'fig leaf' function, with science as a political resource and evidence chosen and selectively deployed in an attempt to legitimate certain views, whilst at the same time, not exerting a major influence on these. The second main role performed by scientific findings in some of the case studies (notably the ABM, SST, smoking and lead debates), was to raise issues onto the political agenda. In this way science may be seen as a partisan involved in partisan mutual adjustment. Alongside other partisans, such as politicians and pressure groups, it provides a framework for discussion even though it cannot determine the outcome of any discussion, which depends for it s resolution on negotiated political solutions. In this sense the disputes may be called conflict by proxy since they were mainly carried out within science but were not only about scientific issues. The term is however, misleading in that it implies that a separability and a separateness of fact and value is possible, when, in fact, the two are inextricably linked in two ways. Firstly, there is no such thing as a 'pure' fact. As has been shown in earlier chapters 'facts' have evaluative aspects and secondly, nominal facts and political values interact closely together so that disputes in one area tend to feed disputes in the other. This is the essence of the positive feedback (overcritical) and the negative feedback (undercritical) models both of which are supported by the case studies.

It will have been noted that no distinction has been made between scientists' pronouncements and scientific pronouncements. This difference was seen as important in the ABM and fluoride disputes where attempts were made to define certain opinions as non expert and, therefore not worthy of consideration. This separation is difficult to support in multidisciplinary areas where there is no single group identifiable as 'the' experts. Furthermore, many politicians and members of the public appear to hold a Mertonian view of the scientist with science seen as an attitude of mind as well as specific expertise so that statements by scientists have a validity because of the credentials supplied by science.[109] Thus, for consideration of the role of science in policy issues the distinction may not be important though clearly it may add another layer of acrimony to a scientific controversy where individuals question the credentials and expertise of those who disagree with them. The next chapter deals with a controversy with major policy implications - that over maternal deprivation.

CHAPTER 6

THE MATERNAL DEPRIVATION CONTROVERSY

6. THE MATERNAL DEPRIVATION CONTROVERSY

6.1. Introduction

In this chapter I will be describing the development of the concept of maternal deprivation from it's beginnings through to it s more controversial and visible manifestations in the 1950's and 1960's. Rather than giving a rigid definition of the term it is probably better to give a broad definition here and expand upon that in the rest of the chapter via the various studies which will be reviewed below. The idea of maternal deprivation may be defined as a belief in the paramount importance of early life for 'normal' physical and psychological development and, furthermore, that the person most responsible for this development is the mother or substitute acting in her place. The concept is a fairly recent one but ideas with regard to the persistence of early childhood influences have a very lengthy pedigree. For example, Clarke and Clarke quote Plato (428 - 384 BC)

> "... the first step, as you know, is always what matters, most particularly when we are dealing with those who are young and tender. That is the time when they are taking shape and when any impression we choose to make leaves a permanent mark."[1]

Quintilian (c. 35 - 100 AD)

> "We are by nature most tenacious of what we have inbibed in our infant years, as the flavour with which you scent vessels when new, remains in them..."[2]

and John Locke (1632 - 1704)

> "... Try it in a dog or a horse or any other creature and see whether the ill and resty tricks they have learned when young are easily to be mended when they are knit, and yet none of these creatures are half so wilful and proud, or half so desirous to be masters of themselves and others, as man... Whatever [children] ... do leaves some impression on that tender age and from thence they receive a tendency for good or evil."[3]

These early statements, whilst stressing the paramouncy of early experience do not, however, contain any references to maternal influences. Dally suggests that although the word 'mother' has been in existence for millenia, the word 'motherhood' is much more recent, and quotes to earliest entry into the Oxford English Dictionary as 1547 when it was defined merely as 'the fact of being a mother'.[4] Thus, the modern concept of motherhood comes from no earlier than the seventeenth century. Badinter makes the claim that prior to the nineteenth century maternal (and paternal) indifference was the rule, citing as an example of this the wet nursing or 'farming out' of the infants of the well to do.[5] (This practice does not necessarily mean that the infants could be claimed to be maternally deprived since wet nurses could well be seen as 'mother substitutes'). This practice was common but by no means universal and was found in both the peasant and upper classes and was, Badinter suggests, primarily due to the very high infant mortality rate. This suggestion is illustrated by numerous contemporary quotes, for example, a comment on a mother's sorrow over the death of her little girl: "She is very much upset and says that she will never have another as pretty",[6] and a father writing in 1806 stated: "I lost two or three children during their stay with a wet nurse - not without regret mind you, but without great vexation."[7] (For further examples in this vein see Shorter[8]). If this indifference was commonplace, then why and when did the modern concept of motherhood appear? Badminter traces this, in part, to a decrease in infant mortality around the time of the industrial revolution which 'allowed' parents to become attached to infants in the confidence that most would survive into childhood. This decrease in mortality was, in turn, related to improvements in hygiene and diet and changes in attitude to breast feeding, initially amongst what Badinter calls the 'middle - middle class.'[9] Here the work of Rousseau (especially 'Émile' published in 1762[10])may have been influential in encouraging mothers to care for their own children, stating for example: "One respects the mother less who does not see her children,"[11] (for elaboration of this theme see Badinter[12]). Shorter is more doubtful that Rousseau had a major influence and claims that by the time Emile was published the switch to maternal care among the middle classes was already under way, although the sending out to wet nurse of artisans children was maintained and even increased, possibly due to the need of artisans' wives to work in newly developing industry. The decrease in middle class wet nursing was, Shorter suggests, due to "... the hideous mortality that struck nurslings sent to the countryside, a death rate that became higher as the century progressed."[13] (It will be noted that this is in direct opposition to Badinter's view, but it is possible that the two situations relate to different places or different times). Stone suggests that concern with child care increased with the development and rise of the nuclear family in the seventeenth and eighteenth centuries. (Before this time extended kinship relationships were the norm). This change may be related to such factors as increased family mobility, economic changes and the Protestant religion with it's: "... drive for moral reformation [which] brought with it an increasing concern for the sinfulness of children,"[14] and, hence, a greater interest in the upbringing of children whose moral (and hence physical) care could no longer be left to wet nurses, servants and tutors. Calvin concurs with this view of the importance of the development of the nuclear family and the industrial revolution in the changes in the concept of motherhood

> "Industrialisation, which split the wage labour of men and the private labour of women, was behind the exaltation of motherhood and the invention of maternal instinct. That is, maternal instinct came along precisely when it was required, making a virtue out of what seemed a necessity. It s enshrinement paralelled the development of the new - not God given - family which came to be called the 'nuclear' family."[15]

Whatever the causes, and it seems likely that all were influential in different groups, places and times, by the mid nineteenth century there was (at least amongst the middle classes) an increased and increasing emphasis on parental (especially maternal) responsibility. It is at around this time that such phrases as 'true motherhood' and 'the warm sun of motherhood' began to appear.[16]

In the nineteenth century, ideas began to develop which explained the requirements of infants and 'duties' of mothers, not only in terms of physical care but also of psychological needs. The most influential systematic treatment of these factors came from the work of Sigmund Freud who claimed that "... the very impressions we have forgotten have nevertheless left the deepest traces in our psychic life and acted as determinants for our whole future development,"[17] and more explicitly

> "... neuroses are only acquired during early childhood (up to the age of six), even though their symptoms may not make their appearance until much later ... the events of the first years are of paramount importance for ... [the childs] whole subsequent life."[18]

It is not relevant here to catalogue or describe the numerous works of Freud. (For commentaries of a more or less critical nature see note 19). Briefly, Freud psychoanalysed large numbers of adults and suggested that the origins of many of their psychological symptoms and abnormalities arose in early childhood.

> "The beginnings of psychoanalytic child psychology can be traced back to the period of 1890 - 1900 when clinical[20] observations first suggested to Freud that childhood experiences constitute one of the etiological factors in neurotic symptom formation in the adult."[21]

Of these early childhood experiences some of the earliest and most influential were deemed to be breast feeding and weaning, but all aspects of maternal care were regarded as important. A later writer summed up this view "Generally speaking, we now know how important is an undisturbed mother - child relationship during the first six years ..."[22]

Whilst not all psychological abnormalities in adults were claimed to be due to problems with the mother - child relationship, many problems were traced to this root. These theories were further developed by Freud and his followers and in 1932 Melanie Klein published a modified set of psychoanalytic theories.[23] These theories differed from Freuds' over definitions and emphases of certain aspects of childhood but were in broad agreement over the importance of childhood experiences.[24] The basis of these theories was the psychoanalysis of individual adults but the development of, and increasing interest in these areas, led, from the 1920's onward, to a number of studies of child rearing practices, primarily where children were separated from their mothers (for example in orphanages). These studies were, by and large, clinically based. The difference between clinical and 'scientific' studies is summed up by Kris

> "Even in an ideal case the difference between psychoanalytic and academic investigation does not only rest in a difference of emphasis. It is not only one of much 'Scientific rigor' versus less of it, of artificial laboratory problems versus the richness of life. Some of the differences can, I believe, be traced back to the dichotomy between what I should like to characterise as that of action research and 'pure' research ... Psychoanalysis has grown up as action research. We have learned to investigate as part of the therapeutic procedure and have been trained to take our own actions into account."[25]

This dichotomy continues to be in evidence with psychoanalysts criticising scientific studies for their lack of clinical insight and more 'scientific' investigation criticising psychoanalysts for their lack of scientific rigor (see discussion below.)

From the introduction it can be seen that the concept of motherhood is a relatively recent one and ideas relating maternal care to mental health are more recent still. In the next section I will deal with the earlier studies of maternal deprivation carried out (primarily by psychoanalysts) in the 1930's and 1940's.

6.2 Early Studies of Maternal Deprivation

Most of the studies referred to here took place before John Bowlby entered the field, but some later studies will be reported on when carried out by workers who were active in the area before Bowlby. The most influential of these early workers were Margarethe Ribble and René Spitz (both psychoanalysts) and it is on these that the discussion will focus, with lesser attention given to other authors. It is not intended to give a detailed review of all the studies carried out by the authors referred to, instead their conclusions will be considered followed by criticisms of these conclusions by other authors (and where available, replies to these criticisms). For detailed findings the reader is referred to the original papers (see references). All quotes are from original papers unless otherwise indicated.

Though by no means the first author on the subject, the work of Ribble was probably the first to be widely recognised and, certainly, her papers make some of the widest claims of the needs of the infant for maternal care and the adverse effects of it s lack. (See Ribble's papers of 1934,[26] 1941,[27] 1943,[28] and especially 1944 [29]). In her articles Ribble claimed that there is a "... necessity for a long and uninterupted period of consistent and skillful 'psychological' mothering by one individual (where the mother herself is not available.)[30] This is in order for the infant to develop normally both psychologically and physiologically. Ribbles suggestions for the physical effects of maternal care include development of the circulatory system, with foetal channels such as the ductus arteriosus open until about the third month,[31] development of the respiratory system, "the need for contact with the mother is urgent in order to keep the reflex mechanisms connected with breathing, in operation..."[32], the nervous system, "It seems clear that the nervous system of the infant needs some sort of stimulus feeding or rhythmic vibratory movement to facilitate it s development",[33] and, finally, gastrointestinal function, since, "... those who are not held in arms sufficiently, particularly if they are bottle fed babies, also develop gastrointestinal disorders."[34] (These disorders include regurgitation, diarrhoea, hiccoughs and air swallowing). Some of these physical phenomena are also claimed to have psychological effects on the infant. For example,

> "The evidence indicates that sucking experience is important for the general well being of the child, for the development of alertness towards factors outside the child's own body, for the age at which speech appears and for the facility of the speech function."[35]

Other psychological effects may be found if the mother and child are separated for a 'lying in' period (as was common when Ribble wrote her articles). "... Such early experience predisposes these sensitive infants to anxiety."[36] and babies who have not been 'mothered' or have received care for a short period only "... commonly develop one or two general types of action. They may develop a form of negativistic excitement or a form of regressive quiescence."[37] 'Regressive quiescence' is apparently considered to be the more important phenomena by Ribble since it is discussed at greater length. The reaction is said to be

> "... Similar to or perhaps identical with a chronic disease known as marasmus or 'infantile atrophy'... The present indications are that this malady was not due primarily to inappropriate feeding or digestive disturbances, nor, as some investigators have thought, to some biological deficit of circulation. It has, instead, the nature of a general disorganization of functions and a deterioration of primary body reflexes due, in large measure, to a lack of 'mothering' or stimulation."[38]

As these few quotes indicate, Ribble's position may be summarised as a belief that for normal physiological and psychological development, an infant needs a lengthy period of care by one individual (invariably referred to in the feminine gender).

Two of Ribble's earliest critics were Orlansky and Pinneau. Orlansky's article is dedicated to a generalised critique of some of the claims of psychoanalytic ideas in relation to child rearing and the importance of infantile experience[39]. He only presents a few references in opposition to Ribble's claims but makes it plain that he thinks these are overstated. For example, he states that: "... Ribble has waxed rhapsodic about the importance of adequate 'mothering' to the development of sound personality and adequate health"[40] and, "... takes too hysterical a view of the neonates organic and psychic resources."[41] He also considers it

> "... unfortunate that such an influential writer has not attempted to draw a line between her empirical findings and her personal opinions. There is so much panegyric and so little satisfactory evidence in her writing that it is difficult for an impartial critic to evaluate many of her statements objectively."[42]

As an anthropologist, Orlansky takes a more culturally relativistic view than Ribble and suggests that the extent to which care by a single adult is requisite to the development of a normal personality should be investigated rather than taken as read which, he suggests, is "... predicated upon the monogamous, nuclear family system of Western Society," and that.

> "... Ribble and those who share her beliefs are not so much making an absolute judgement on the type of care which is necessary for sound personality formation as they are making a series of recommendations which, implicitly, are based upon the nature of the child's social environment in the Western family ... [and that] ... scientific investigations of desirable patterns of child rearing might proceed more successfully if the investigation were more conscious of this fact."[43]

Pinneau was equally critical of Ribble and develops his critique by quoting over 80 references with findings running counter to those suggested in Ribble's articles. For example, with regard to Ribble's assertions of the underdeveloped and unstable nature of the infant's circulatory system Pinneau states: "No evidence is presented to substantiate these contentions. The available evidence appears not merely to refute this position but indicates a remarkable stability in the infant's circulation."[44] Many of Ribble's suggested effects are similarly criticised as being counter to the evidence adduced by Pinneau and he suggests that here conclusions may be dubious in other ways. Ribble is accused of 'adultomorphism', when she suggests that sucking may be pleasurable to the infant,[45] and though Pinneau does not deny that this is possible he recommends only "cautious acceptance of the interpretation."[46] Pinneau further accuses Ribble of inconsistency by quoting two of her claims (from different papers). Firstly, "In observing several hundred normal infants placed at the breast for the first time, approximately eighty per cent suck at once if they are held in comfortable apposition to the breast,"[47] and secondly, "Fifty per cent of the 600 babies in our study were definitely not 'self starters' in sucking."[48] Pinneau comments, "It appears rather hazardous for Ribble to use such discordant findings as support for her hypothesis."[49] Other criticisms are made in a similar vein and, in summary, it seems that both Pinneau and Orlansky would claim that Ribble has extended her theories beyond the available evidence, or even despite the available evidence.

At the same time, other authors supported Ribble's position (of course any worker citing Ribble's work is providing a degree of implicit support, but here I am concerned with support of a more explicit nature). Kris,[50] for example, writing in 1950, reviewed the development of psychoanalytic child psychology and deemed it, "appropriate to state how much we owe to Margaret Ribble's own investigations,"[51] and Kubie, in reviewing Ribble's book 'The Rights of Infants' described the book as "wholesome", "it s spirit and purpose is profoundly right," though he is rather critical of the evidence she presents this "does not seem to be particularly important because the author has arrived at the correct conclusions."[52] Stone[53] is also rather critical of Ribble but is even more critical of Pinneau and what he calls, "Pinneau's breathtakingly complete demolition of Margaret Ribble."[54] He implies that Pinneau may be over critical and describes his article as, "... a kind of hydrogen bomb perfection of destructive criticism, not a paragraph is left standing for miles around."[55] Stone suggests that Ribble's conclusions are correct, though her reasons for holding these conclusions may be wrong,"[56] and that Pinneau's dismissal of this one author should not be extended to the entire field.[57] It may be that Stone himself is overly critical, since, as already mentioned, in support of his views Pinneau quotes over 80 references, many of which advance hypotheses alternative to those of Ribble.

One further author whose support of Ribble is of interest is Spitz. Spitz[58] who was also criticised by Pinneau (see below) defends both himself and Ribble (and others) by stating that his and Ribble's work has been "... applied all over the world in the practice of many hundreds of hospitals and institutions, with a concomitantly demonstrable saving of innumerable human lives."[59] This theme will be returned to, but essentially Spitz appears to be saying that, whilst these ideas are not supported by scientific study, they can be seen in clinical practice. Spitz himself, published a series of papers from 1945 to 1951 in the journal 'Psychoanalytic Study of the Child', dealing with two institutions, 'Nursery' and 'Foundling Home'.[60-64] The main findings of these studies will be discussed below alongside criticisms by Pinneau,[65] but, briefly, two groups of infants were compared, one from Nursery (where most of the infants were apparently cared for by their mothers) and another from Foundling Home (where separation occurred after weaning). In Nursery, a penal institution for delinquent girls, mothers cared for their children for 6 - 8 months (and in some cases up to one year). If a child and it's mother were separated, another mother or pregnant girl cared for it. These mothers gave the child "... everything a good mother does and beyond that everything else she has".[66] The children lived in glass cubicles until six months of age when they were transferred to rooms with other children. Toys were available and the mothers spent much time with their children.

In Foundling Home the infants were breast fed but other than this the mothers seem to have had little contact with their children. Care was undertaken by nurses (caring for eight or more children) so that "Foundling Home does not give the child a mother, nor even a substitute mother, but only an eighth of a nurse."[67] The children also lived in cubicles but bed sheets were hung over the sides of the cribs so that the infants were largely isolated. Few, if any, toys were available. Spitz compared the physical and psychological development of the two groups of infants and concluded that most infants separated from their mothers for more than six weeks (most common in Foundling Home) developed 'hospitalism' or it s milder manifestation 'anaclitic depression' (if separation was temporary.) In Spitz's earlier papers these are considered separately with anaclitic depression (similar to adult depression) giving rise to a drop in development quotient (D.Q. as measured by the Hetzer-Wolf baby tests), weeping and sadness and a generalised lack of emotion accompanied, in some cases, by anal, oral and genital auto-erotic activities. A drop in D.Q. was also found with hospitalism, as was retardation in skeletal development, delay in sitting and walking and in the development of social skills as well as a mixed response to strangers ranging from extreme friendliness to fear. Spitz noted that the Foundling Home infants were deprived in other ways, as well as maternally but states "The presence of a mother or her substitute is sufficient to compensate for all the other deprivations."[68] Perhaps the worst finding of all was that in Foundling Home 34 of the infants died. Spitz initially reported that 23 of these deaths were due to measles with the rest due to a variety of causes[69] though in a later report the deaths were explained as due to "The progressive deterioration and the increased infection liability [which] led in a distressingly high percentage of these children to marasmus and death."[70]

In his highly critical review Pinneau questions Spitz's descriptions, methodology, results and conclusions.[71] Some of these criticisms relate to difficulty in evaluating Spitz's work, for example, Spitz not only does not name the institutions concerned, but does not even name the countries they are in, stating only that they are in two different countries in the Western hemisphere. Pinneau claims that it is possible to locate Nursery as being somewhere in New York State but Foundling Home only as somewhere 'South of the Rio Grande'.[72] Spitz, in a reply to Pinneau, states that the identity and place of the institutions were undisclosed to avoid uncalled for inquisitiveness and to protect medical confidentiality and that the term 'Western hemisphere' was used "in a cultural sense, meaning the Western world, including Europe."[73] Morgan, who has also been very critical of Spitz, suggests that the location of the home might be germane, and lists reports of Italian orphanages where children were "chained, flogged and tortured."[74] (Italy is suggested because Spitz mentions that the mothers were of Latin background[75]). She suggests that if this were the case, in Foundling Home, then the physical and psychological effects found are more likely to relate to this than any separation trauma. Here Morgan would tend to be taking an extreme case to make her point and furthermore overlooking Spitz's statements which imply that the studies were begun in 1942 or 1943[76] hence, (since Spitz was residing in the United States), almost certainly excluding institutions in the war areas of Europe. On the question of confidentiality, Pinneau comments:

"While a physician's responsibility to his patients or subjects is not to be denied, it is difficult to believe that in a matter of this sort, professional ethics or legal restrictions would be violated by identifying the institution concerned, or at any rate, by giving details as to the national, educational and socioeconomic samplings involved."[77]

Pinneau goes on to suggest that behavioural differences between the two groups could relate in various combinations to differences in racial extraction and mix, poor heredity and congenital defects, as well as differences in cultural factors and economic status[78], and notes that there is no discussion in Spitz's papers of the presence or absence of congenital or birth defects in either group.[79] He also notes that, whilst Spitz claims "a marked advantage" for the Foundling Home children in terms of background and heredity[80], there is no evidence for this in any of his reports.[81] (Incidentally, John Bowlby in discussing Spitz's data in his very influential review 'Maternal Care and Mental Health' (see below), reports that Spitz gives "explicit data" that the groups are of a "similar social class and as nearly as possible spring from similar stock"[82]). In his reply to this Spitz describes congenital abnormality as one of the uncontrolled variables in the sample, but claims that "... the nature of the institutions themselves implies that congenital abnormalities were excluded on admission, as the institutions in question were not equipped to deal with them"[83]. Pinneau finds this statement "difficult to reconcile with [Spitz's] description of the Foundling Home infants medical care"[84] since, in an early paper, Spitz had stated that Foundling Home infants were visited by physicians daily.[85] Further cause for disquiet is provided by the calculation that a staff of 12 persons would have been required to carry out the study. "In a study of this magnitude, we would usually expect to have the staff named and some information concerning their training and qualifications..."[86] Spitz replies that the staff "...

consisted of myself, Katherine M. Wolf and a number of assistants with PhD qualifications trained by Wolf and myself in testing and observing infants."[87] Pinneau questions both the results and the validity of the tests used. With regard to the results, Pinneau notes that the Foundling Home children show a difference of approximately 59 D.Q. points from age 2 months to one year old and that 43 points of this drop occurred between 2 and 5 months of age, when the majority of the mothers were still present.[88] A twelve point drop is found between the fifth and sixth months when separation occurred for the majority[89] with only four points further drop by age one year. Pinneau's interpretation of this is that:

> "The data support Spitz's contention that marked retardation characterizes the Foundling Home Infants; however, the information given by Spitz, rather than supporting his hypothesis that the retardation is due to separation of mother and child, indicates that it was in evidence before the separation."[90]

In further questioning the validity of the test results, Pinneau claims that, whilst the study was apparently longitudinal, it was, in fact, cross sectional, or at best, a mixture of the two.[91] Two points are raised in regard to this; firstly, that if the study was cross sectional then the D.Q. scores compared are of different infants at the same time rather than the same infants over time and so any "... conclusions must be rejected as he was contrasting two groups of children of whose former and future development he was not cognizant."[92] Secondly, if the study was cross sectional it "... may well have been carried out when the children were ill", (since Foundling Home suffered an epidemic of measles), and for this reason performances were poor.[93] Regardless of the test results, Pinneau maintains that the Hetzer - Wolf test itself was poorly standardised[94], under or over-estimating D.Q.'s at different ages[95] and having little or no predictive ability.[96] In reply, Spitz states that the Foundling Home infants were studied daily for three months and then at four monthly intervals,[97] criticises Pinneau for not defining what he means by longitudinal,[98] defends the Hetzer - Wolf test on the grounds that it had been widely used in many countries and, like any other test, is only meaningful with a number of successive applications to elicit trends[99] and, finally, attributes the drop in D.Q. during the first year to separation of mother and infants following weaning.[100] Pinneau's counter-reply appears to ignore Spitz's first point when he states that

> "... three months of observation can be considered a sufficiently long period of time to qualify as a longitudinal study for some purposes, it is quite obvious that it cannot be used to show the development of a constant number of Foundling Home subjects during their first year of life."[101]

With regard to the validity of the Hetzer - Wolf test, Pinneau maintains that "... frequency of use cannot, in and of itself, increase the validity of the test itself."[102]

In his original article Pinneau makes further criticisms of Spitz and also criticises a study by Fischer[103] whose conclusions are similar to those of Spitz. He concludes his paper with the statement that, "As yet ... we do not have convincing evidence, based on scientifically controlled investigations, as to any of the major problems in this area."[104] Some of Spitz's replies to these criticisms have already been quoted but it is worth recording some of his general comments on Pinneau's paper. It is described as "polemic","based on mis-understandings, arbitrary conclusions and unwarranted assumptions, [resting] on figures culled in a biased manner."[105] Pinneau "indignantly states"[106] certain of his points, "his unfamiliarity with the subject ... becomes embarrassingly evident"[107] and his critique is "... built on inference and implication and ... recourse to invention."[108] Spitz then reiterates the superiority of clinical findings and states that:

> "... the experimental, psychological and statistical material in the five articles discussed by Pinneau was not introduced by me to prove my point, as the articles were addressed to medical readers. They were used as supportive evidence, subordinated to the clinical data - an illustration, as it were. Dismissing these statistics as inadequate would, therefore, not invalidate my clinical findings."[109]

This point is interesting in that it implies that, whilst valid statistics may be used to support the clinical date, (which is presumably why they were included), the charge of invalid statistics cannot be used to detract from the study. Spitz further comments that "this is not the first attempt by Pinneau to attack fieldwork by purely deductive reasoning. He has tried to invalidate the pioneer work of Margaret Ribble..."[110] As noted above, Pinneau's critique of Ribble is based on over eighty references and, whatever the validity of the studies quoted, this can hardly be classed as 'purely deductive reasoning', though one does get the impression that Pinneau is over critical at times, for example, would the names and qualifications of Spitz's associates have been deemed relevant if the published findings were different?

Lest it be thought that Pinneau was carrying out a one man crusade against Spitz and Ribble, it should be noted that numerous other authors have expressed doubts in Spitz's methods and conclusions, albeit less vehemently than Pinneau. These criticisms have either been expressed in general terms or by the suggestion of alternative hypotheses to account for any findings (see below). Examples of those who have been critical include Wooten, who comments "... with true analytic fervour, [Spitz] regards evidence of retardation at less than five years old as a mark of irreparable injury."[111] She also reiterates some of Pinneau's criticisms.[112] Clarke comments that Spitz's theoretical orientation "... led him to overlook the wide range of deprivation and isolation experienced by these children, who lacked not only maternal care and attachment but also general care",[113] and Casler suggests that lack of stimulation may have played a part in the infant's abnormal development.[114] Finally, Morgan, writing twenty years after Pinneau's critique, not only reiterates his criticisms but extends them in a chapter entitled 'The Spitzian Scare'.[115]

Other authors have been more neutral or even supportive, for example, O'Connor suggests that much of the disagreement between Spitz and Pinneau reflects a difference in opinion concerning the meaning of the word deprivation,[116] and Glaser and Eisenberg, while accepting that Spitz's work is not perfect, state that, in general, his observations are in accord with those of other investigators.[117]

Two further authors will be considered briefly in this section. Firstly, Goldfarb, who published an extensive series of papers in the 1940's relating to the topics of institutional and maternal deprivation.[118-126] Goldfarb differs from the previous two authors in that, whilst he found that children and infants in institutions were victims of deprivation (with permanent effects), he attributed at least part of this deprivation to environmental causes.

> "The early impoverishment of the institution not only influences the child's mode of adjustment, but affects the living content of his mental existence and the specific tools and skills which may have direct bearing on his mode of adjustment."[127]

He further believed that mothering was not a 'cure all' since he concluded that, "It is probable that no matter how well both kinds of care [home and institution] are organised, there will be some groups of children most effectively reared in one atmosphere as against another."[128] Goldfarb's work has not escaped criticism. For example, Stott commented on the lack of information on such variables as education and occupation of the mother and criticised the implication that deprivation effects are permanent despite no follow up beyond adolescence.[129] Stott goes on to suggest that the findings of Goldfarb (and Spitz, Bowlby and others) can be explained by a hypothesis of congenital subnormality rather than some form of maternal deprivation.

The final set of papers to be considered in this section are those of Skeels and colleagues. These papers refer to a series of studies finding that children in institutions showed a drop in D.Q. with this drop being positively correlated to length of institutional stay.[130] Skeels attributed this decline to the effects of an impoverished environment and found that it was reversible if the children were transferred to an institution where more personal care was possible.[131,132] In an earlier study Skeels, et al, took two matched institutional groups, one of which received pre school teaching whilst the other did not. The I.Q.'s (Binet Scale) of the taught group were found to have increased by 9 points which was taken to indicate that any deprivation effects need not be permanent.[133] The statistics and methodology of these studies were criticised by McNemar,[134] but following a reply by the author[135] McNemar did not repeat these criticisms and, according to O'Connor, when McNemar compiled a list of statistically faulty researches, the Skeels study was not included. O'Connor concludes that "It might, therefore, be said that under some circumstances, life in an institution can be stimulating and improving."[136] Finally, a study by Skodak and Skeels showed a more favourable outcome of adoption than might be expected based on knowledge of parental and early adverse circumstances with the I.Q. of the adopted children found to be 20-30 points above that of the natural parents, again emphasising the importance of environmental factors.[137]

It is noteworthy, though perhaps not surprising, that of the authors quoted here, those that were apparently least criticised were those making the more modest claims. (For further reviews of these and other studies contemporary with those considered here, see especially

Stevenson,[138] Casler,[139] Yarrow[140] and Rutter[141]). Before going on to deal with Bowlby's work, as a final point, it is worth noting that though studies of maternal deprivation are widely identified with Bowlby and his colleagues, much work had been undertaken before his entry to the field and some dispute had already taken place over the interpretations of these results.

6.3 Bowlby's Contribution to the Debate.

At the same time as Spitz and others were working in the U.S.A., John Bowlby began looking at evidence for maternal deprivation in Britain. Bowlby was an M.D. trained in psychoanalysis and his work, at least in the first instance, was clinically based. A second approach in this area was the work of Hebb, based largely on experimental studies of the role of learning in development.[142] Here, I will follow Clarke and Clarke in considering the work of Bowlby rather than that of Hebb since the work of Bowlby and his colleagues had by far the greater implications for policy and views of early human development.[143] A full review of this work will not be attempted here though much of his early work will be considered.

One of Bowlby's earlier contributions to the field was his study of 'Forty-Four Juvenile Thieves' first published in 1944 and reprinted in 1946.[144] In introducing the study Bowlby notes that "Almost all recent work on the emotional and social development of children has laid emphasis upon the child's relation to his mother", and comments, "... observations such as those reported here are not found if old case records of similar patients are perused ... My experience has shown me again and again that if these factors are not looked for they are not found..."[145] Bowlby also notes the limits of the report

> "... the limited enquiry of the type here presented is grossly inadequate. This research was unplanned;

it grew out of the practical problems confronting workers in a busy clinic and has all the defects inherent in such conditions. The number of cases is small, the constitution of the sample is chancy, the recording of the data unsystematic, the amount of data in different cases uneven. Conclusions drawn in such circumstances are clearly liable to all sorts of errors..."[146]

(In his later monograph 'Maternal Care and Mental Health' Bowlby describes this work as "... a systematic clinical and statistical study..."[147]). The main findings of the study are that, of the 44 thieves, seventeen had suffered separation from their mother (or substitute) during the first five years of their lives compared to only two of the controls (drawn from other children attending the child guidance clinic), and of the fourteen thieves considered to be 'affectionless', twelve had been separated from their mothers, as against five of the remaining thirty thieves, with no 'affectionless' individuals found among the controls. On the basis of these results, Bowlby concluded that "... prolonged separation of a child from his mother (or mother substitute) during the first five years of life stands foremost among the causes of delinquent character development and persistent behaviour."[148] Morgan has been highly critical of Bowlby's work.[149] For example, of the 'thieves' study she notes an apparent determination by Bowlby to classify the children as abnormal since he states, for example, that

"... a large number of the children, perhaps half, at their interview appeared fairly normal. This impression is grossly misleading in a majority of cases and, if taken seriously, results in a disasterously erroneous diagnosis. For this reason I habitually ignore my psychiatric interviews when no positive signs of a disorder have been found and base my diagnosis on the reports of the mother and teacher."[150]

Based on these reports and interviews Bowlby categorised the children into six groups, normal, depressed, circular, hyperthymic, affectionless and schizoid.[151] Morgan comments that "Beyond the vague explanation of terms ... ('unstable', 'overactive', etc.), Bowlby just does not specify by what methods he selected members of these groups."[152] On the 'diagnosis' of stealing, Morgan can find no mention of the control group being checked for unreported stealing,[153] the importance of which may be noted by the classification as a thief of "No. 16, David J, a boy of 9.7 ... [who] had, together with another boy, pinched an ice cream from a barrow when the man was not looking. Apart from this he was regarded as absolutely honest,"[154] which, as Morgan says, somewhat undermines Bowlby's claim that "The fact we are studying mostly chronic delinquents has many advantages, the principle one being that our findings will not be diluted by the inclusion of material derived from casual and stray offenders."[155] Turning to the affectionless thief, the category apparently regarded by Bowlby as the most significant, these individuals

> "... were distinguished from the remainder by their remarkable lack of affection or warmth for anyone... [they] ... had apparently never since infancy shown normal affection to anyone and were, consequently, conspicuously solitary, undemonstrative and unremonstrative."[156]

Among those classified in this group were Betty I (No. 27), a 5.7 year old, who, though described as 'wooden' "... was extremely fond of the baby and liked mothering him. She played well and happily and was popular and sociable with neighbouring children;"[157] Norman K (No. 30), a 7.8 year old, who was "... said to be a very affectionate child who liked helping his mother in the home - 'more like a little girl'. But his mother also found him very secretive which made it difficult for her to understand him,"[158] and Kenneth W (No. 32), aged $10\frac{1}{2}$, who "... showed no affection for his mother, but much for his grandmother."[159] Morgan suggests that "... the term affectionless is being used not simply to denote lack of affection for anyone, but rather lack of affection for one's own mother. Grandfathers, babies, sisters, other children appear to be disqualified as objects of 'normal' affection."[160]

Although Morgan has carried out the most comprehensive critique of the Bowlby study, other authors have also been critical. These will be discussed below in conjunction with comments on the Bowlby monograph.

The monograph was first published in 1951 in the Bulletin of the World Health Organisation[161] and shortly afterward appeared in book form.[162] Since it's release it has attracted widespread comment, both approving and disapproving and in a review of child care literature, it has been described as "A classic work in child welfare..."[163] It consists of two parts, Part I, a review of some of the research carried out in the field and some of the problems inherent in that research, and Part II, on the prevention of maternal deprivation. The main conclusion drawn may be summed up by a quote from the book

> "For the moment it is sufficient to say that what is believed to be essential for mental health is that the infant and young child should experience a warm, intimate and continuous relationship with a mother (or permanent mother substitute) in which both find satisfaction or enjoyment."[164]

Where this relationship is not present 'maternal deprivation' may result, though Bowlby recognises that this term covers a number of different situations since, for example, a child may be deprived even in the company of a mother (or substitute) if the necessary loving care is not present. Removal of a child from his mother (or substitute) will result in deprivation, which may be mild if he is looked after by someone he knows and trusts but may be severe if he is looked after by a stranger. These arraingements are termed 'partial deprivation' and give the child some satisfaction, as opposed to complete deprivation which may be found in such places as institutions, residential nurseries and hospitals. Partial deprivation may give rise to acute anxiety, excessive need for love, feelings of revenge and, arising from these, guilt and depression. Associated with these may be backwardness in talking, drop in I.Q., retarded physical growth and inability to make relationships.[165] The central issue of the report is complete deprivation, which has "... far reaching effects on character development and may entirely cripple the capacity to make relationships,"[166] i.e. the effects may be permanent. The evidence for these views "... is largely clinical in origin ... it is unfortunately neither systematic nor statistically controlled and so has frequently met with scepticism from those not engaged in child psychiatry."[167]

The best way to consider the book is in the light of critical comment. It could be argued that this method might concentrate only on those factors of the book which are inadequate, but, in fact, most comment does concentrate on the central issues. One of the earliest critical review papers was produced by O'Connor.[168] In large part his criticisms relate to the inadequacy of the studies quoted by Bowlby in support of his theoretical views, with comments such as "... so far as the theory is concerned, the crucial experiment remains to be done;"[169] "The permanently adverse effect of separation does not appear to be established in this study"[170] and, in line with Morgan's comments (above), some of the data "... however meaningful clinically are notoriously subjective and unreliable unless rigorously checked and counter-checked by observers who do not know of the hypothesis."[171]

The studies by Goldfarb and some of those by Spitz (considered above) are extensively quoted by Bowlby who, though recognising limitations in the work, says "... for skillful planning and care of execution, Goldfarb's work ranks high; not until a comparable piece of work has been done with different results can there be any reason to doubt his findings."[172] However, as O'Connor points out "... theoretically, Goldfarb is not in quite the same position as Bowlby... "[173] since he does not appear to subscribe to the idea that maternal care is always best (see above). Thus, it is suggested that Goldfarb's work, howeverexcellent, does not directly support Bowlby. O'Connor goes on to comment on studies expressing ideas contrary to Bowlby's.[174] Some, such as the Skeels' studies (see above) are not mentioned by Bowlby, whilst others are criticised. It is claimed that the objections made by Bowlby to contrary findings can be applied equally to evidence put forward in support of his thesis,[175] i.e. Bowlby is biased in his criticism. In conclusion, O'Connor states that "The results of this survey tend to undermine confidence in the hypothesis of maternal deprivation and resulting social, intellectual and physical inadequacy ... "[176]

Another extensive critique was published three years later by Barbara Wooten, a social scientist.[177] She repeats many of O'Connor's criticisms of possible bias in study selection and with regard to those studies quoted by Bowlby in support of his thesis, claims that "Even a first hand perusal of all the independent studies referred to ... by Bowlby, has failed to reveal any support other than the impressions of the investigators concerned for the existence of a link between maternal deprivation and delinquency."[178] She also notes that Bowlby dismissed the inadequacies of individual studies with the statement "What each individual piece of work lacks in thoroughness, scientific reliability or precision, is largely made good by the accordance of the whole. Nothing in scientific method carries more weight than this."[179] Wooten sees this as a "... decidedly dangerous doctrine, in as much as it comes near to an assertion that it does not matter greatly if all the work is slipshod so long as the answers are much the same."[180] It will be remembered that Kubie and Stone made similar comments (to that of Bowlby) about Ribble's work, essentially that the content was wrong but the answers were correct. Wooten goes on to discuss the possible role of heredity in any findings. She notes that in both the 'thieves' study and his monograph, Bowlby dismisses heredity as a relevant factor. For example, Bowlby states that, "In assessing heredity, the presence of neurosis, psychosis or serious psychopathy in parents or grandparents is taken as the criterion",[181] and, in reviewing a study in support of his thesis, "Since heredity is, so far as possible, held constant for these two groups, the difference cannot be explained in this way."[182] On the former point Wooten comments that the criterion "... implies a degree of confidence in the diagnosis of two generations earlier and as to the conditions governing the inheritance of morbid mental conditions which can hardly be justified by the evidence on the subject", and on the latter, "Even with the qualification 'so far as can be determined', such a light hearted dismissal of the influence of differential inherited factors is incredibly naive..."[183] She also notes that a study reaching conclusions different to those of Bowlby treats hereditary factors in a similarly cavalier manner and concludes that Bowlby's hypothesis remains unproven.[184]

One of the most extensive critiques of the data on maternal deprivation (up to 1961) was carried out by Casler.[185] Of the studies carried out he suggests, "It is my contention that these studies are, virtually without exception neither conclusive nor particularly instructive, because of their failure to take into account certain critical variables."[186] These variables include age of separation, the nature of the institution and reason for separation. Using the consideration (or otherwise) of these variables as the criterion for the adequacy of studies, Casler excludes all but twelve of the forty-five studies utilised by Bowlby in support of his ideas.[187] He also agrees with Wooten that many of the studies quoted by Bowlby do not actually support his theories and that studies opposing Bowlby's conclusions are not mentioned.[188] An alternative hypothesis is put forward explaining the emotional, intellectual and physical problems found after the age of six months as relating to the separation experience rather than any deprivation per se. He further suggests that separation before this time may give rise to perceptual deprivation, due to lack of stimuli and that studies purporting to show maternal deprivation may be explained in this way.[189]

Bowlby's work in the field of maternal deprivation continued after the publishing of his monograph. For example, in 1953 he produced a further case study[190] and in 1954, with Mary Ainsworth, he published a paper detailing the possible methods of studying the maternal deprivation hypothesis.[191] In 1956 a further study was published, dealing with 60 children suffering from pulmonary T.B., who had been separated from their mothers due to admission to a sanitorium.[192] These children were compared to a control group of 180 individuals from the same school matched for age and sex with the sanitorium group. Data for the controls were obtained from reports by teachers and psychologists and the controls were initially selected by the teachers, though it was found

> "... that in some cases the age criteria were not strictly adhered to and children who, for example, were thought representative of their age group or a credit to the school were occasionally selected. Thus, the selection of controls had ultimately to be made by the field workers..."[193]

As well as this problem, the reports by teachers and psychologists were not ideal. Doubts are expressed about the teacher's objectivity[194] and, in the case of the psychologists, there were variations in test situations and the psychologists were aware of the category of each child[195] (i.e. they may have been biased). The sanitorium and control children were compared on a number of criteria but on neither I.Q.[196] nor teacher's reports [197] were any statistically significant differences found. This led Bowlby, et al, to doubt the reliability of some of the teacher's reports and these were re-examined for reliability by two psychologists working independently.[198] They agreed that the teacher's reports were unreliable in 25 cases, reliable in 32, with disagreement on the remaining 3. The 32 reliable cases were compared with their controls and statistically significant ($p < 0.05\%$) differences were found between the two groups on 5 items (out of 28). These items were 'day dreaming', 'he does not seem to know what to do unless he is told', 'his attention wanders rather frequently', 'he is liable to get unduly rough during playtime and a combined item, 'he seems diffident about competing with other children/he does not seem to care how he compares with other children'.[199] Based on these findings and previous studies, the conclusion was drawn that "... in comparison to control groups, separated children are (i) less able to respond to a test situation, (ii) more given to 'day dreaming' and lack of concentration and (iii) more given to roughness and hostility."[200] Morgan has been highly critical of the methodology used in this study,[201] and Kraupl-Taylor commenting on the rejection of some of the teacher's reports, suggests that "... bias did inadvertently - though perhaps inevitably - creep into this secondary selection of acceptable reports."[202]

The second half of the paper goes on to look at the range of personalities in the sanitorium children. Firstly, based on degree of adjustment and maladjustment (from information supplied by teachers, psychologists and parents),[203] and secondly, based on patterns of personality. The ratings of adjustment were concerned with the degree of psychological disturbance and the form of personality organisation shown by the children.[204] This latter is worth looking at in more detail. Three independent judges sorted the children into different groups and then met to consider these groupings and agree on a basis for classification. Following this, the judges, again independently, regrouped the children. At a further meeting classifications were compared and disagreements resolved. These classifications (reproduced in full) were:

"Group A. Conforming: Children who, through good behaviour or achievement or both, are socially acceptable. Some, but not all, seem unduly concerned with winning the approval of adults by means of this behaviour.

Group B. Over-dependent: Children who are more dependent on the mother than is normal for their age, showing it by clinging to her or demanding reassurance of her affection and approval. These are divided into two sub groups: (i) those who do not express hostility and (ii) those who express some measure of hostility to other people including the mother.

Group C. Withdrawn and over dependent : Children who are not only extremely over dependent in all their relationships but are also unable to mix satisfactorily with other children.

Group D. Ambivalent: Children who show evidence of both affectionate and strongly hostile feelings to their mothers and others and who are not over dependent.

Group E. Mother rejecting: Children who show both a pronounced lack of dependence on the mother and some hostility towards her, with a preference for other members of the family.

Group F. Affectionless: Children who show no apparent dependence on, nor affection for, the mother and whose relations with other figures are also severly disturbed.

Group G. Superficial: Children in whom there is little evidence of overt disturbance or difficulty, but whose relationships are suspected to be lacking in warmth and depth though at first glance they may appear satisfactory." 205

Six children did not fit into any of these groups so an eighth, unclassified group was added. It will be noted that each of these groups involves some defect in the child's ability to make relationships,[206] and that five of the classifications (groups B, C, D, E & F) involve, primarily, abnormalities in the child's relationship with it s mother.[207] This finding, also noted in the 'thieves' study seems to be indicative of the psychoanalytic belief in the paramount importance of the mother - child relationship, rather than of any abnormality in the child. Morgan takes the view that "this study reveals a remarkable degree of filial affection in children who have been separated from home for long periods."[208]

Bowlby, et al, conclude the study with what has become a widely quoted statement

> "... it is clear that earlier workers[including Spitz and Goldfarb] , including the present senior author, in their desire to call attention to dangers which can often be avoided have, on occasion, overstated their case. In particular, statements implying that children who are brought up in institutions or who suffer other forms of serious privation or deprivation in early life commonly develop psychopathic or affectionless character (e.g. Bowlby 1944) are seen to be mistaken."[209]

In 1958 however, Bowlby, in a letter to 'The Lancet' claimed that: "... in including myself amongst those guilty of overstatement, I may have been unduly self critical."[210] Since this time Bowlby has continued to publish in this area and has developed a detailed theoretical framework attempting to explain the mechanisms of maternal deprivation.[211] Interestingly, some of this work has been criticised by psychoanalysts, including Spitz, as being 'non psychoanalytic' in nature.[212] The discussion and disagreement over the existence, or otherwise, of maternal deprivation has not been restricted to the medical and psychological professions. Some of the criticisms from other disciplines have been dealt with above, for example by Orlansky (an anthropologist) and Wooten (a social scientist). Below I will expand upon these.

6.4. Non-Psychological Evidence

In the above discussions the main emphasis has been on the work of psychoanalysts and psychologists (medically trained or otherwise) who have studied mother - child relationships in a variety of settings. Many other disciplines have also been involved in the debate, either because workers have been active participants, or because their work has been used as supporting evidence for or against the existence of maternal deprivation. Some of this work will be briefly discussed to illustrate the fact that the controversy has not been restricted to the psychological arena. Studies utilised in support of, or against, the existence of maternal deprivation can roughly be divided into two categories. Firstly, biological findings and, secondly, cultural studies.

6.41 Biological Evidence

Two main strands of work may be distinguished under a biological heading (though these are closely interlinked). The first is that male and female behaviour has a biological basis and the second is based on the applications of ethological studies to human behaviour.

On the subject of the inateness of male and female behaviour Gadpaille has said, "Maternalism is instinctive to females, not only in this species but in mammals generally,"[213] and Fox has agreed that

> "It is a basic ground rule for the primate species that, if we want healthy and effective adults, we have to associate mother and child safely and securely through the critical period at birth at least to the point where the children become independently mobile. In humans with their extremely long dependency period, this is even more important, so that in a very real sense the mother-child tie is the basic bond in our social relationships and one that is really taken over from nature."[214]

Blurton-Jones is more cautious and says

> "... we find evidence compatible with the assumptions that the mother-infant relationship (in it s gross aspects) of evolving man was indeed very similar to that in the other higher primates ... [but] ... even the beginning (which is where this research stands) of a systematic and quantitative examination of the child rearing practices of mammals throws doubt on some assumptions, such as that women always have stayed at home with the kids while father went out into the World and other such conclusions of 'evolutionary perspectives' that are really projections of our present state into primitive man rather than conclusions from systematic study."[215]

and he goes on to conclude "It is ... a myth that women always sat at home cooking and breeding."[216]

Some of the most vociferous opposition to the view of 'biological predestination' has been from feminists and researchers sympathetic to the feminist cause. Examples of criticisms expressed by the former may be found in the works of Friedan,[217] Millett[218] and Greer,[219] whilst in the latter category comes Oakley (a psychologist) who has said, "There is no such thing as maternal instinct. There is no biologically based drive which propels women into childbearing or forces them to become child rearers once the children are there."[220]

With regard to ethological evidence, the problem of applicability to humans arises. Views on this have been varied. Clarke,[221] Clarke and Clarke,[222] and Kohlberg[223] have been critical of this application for several reasons.

> 1) "Many experiments in early animal learning use either a duration or a severity of experience which could scarcely allow survival if analogously applied to human infants. In these cases, therefore, the findings may not apply to ordinary, less deviant conditions."
>
> 2) "Few attempts have been made to find out whether any behaviour modifications are reversible."
>
> 3) "Few attempts have been made to find out whether similar effects can also be induced by similar methods later in life, i.e. whether or not such effects are specific to early experience."
>
> 4) "It is possible that, with rapid maturation in a hostile world, early learning may have a very different function in animal as opposed to human development."[224]

Despite these criticisms, Clarke recognises that ethology provides potentially important evidence, not least because these can be very well controlled in comparison with human studies.[225] Blurton-Jones expresses similar caution with regard to the application of animal studies. "Information about animal behaviour only allows one to suggest things about human behaviour. Whether the suggestions apply or not has to be confirmed by direct data on man."[226] But Bronfenbrenner shifts the onus of proof to those wishing to deny the applicability of the data

> "... if an investigator can demonstrate that a given relationship or process operates (or fails to operate)

> in a phylogenetic series approaching Homo-Sapiens and the relevant characteristics or their analogues are present in the human species, we must then entertain the probability that the given process operates (or fails to operate) in man. At the very least the burden of proof for demonstrating that man is the exception surely lies with the sceptic."[227]

Operating under this philosophy, Bronfenbrenner, in a very extensive review of animal and human studies, concludes that the work of Spitz and Bowlby is corroborated by many of the reviewed studies.[228] Interestingly, in the same book as this paper by Bronfenbrenner are papers by Casler[229] and O'Connor.[230] These authors have been critical of maternal deprivation theory, (as noted above), and each quote a number of animal studies, not included in Bronfenbrenner's review, which either do not support, or support a modification of the theory.

A good example of early ethological work relevant to this area is that of the Harlows, whose studies of rhesus monkeys have been used in support of maternal deprivation theory. In one study infant monkeys were isolated and reared alone and some of them were provided with cloth or wire dummy 'mothers'.[231] When nature they were introduced into groups of other monkeys but were found to be incapable of sexual behaviour. By putting the females with older 'experienced' males some of these conceived and when the infants were born it was found that the females were "either completely indifferent or violently abusive to their infants."[232] These findings would seem to support those who warn of the hazards of mother-infant separation, but as Morgan points out, the monkeys were raised not only without a mother but also in total isolation from their own species.[233] Morgan reports other work by the Harlows where monkeys were brought up without mothers but with peers. These individuals showed almost normal social development and sexual behaviour whilst monkeys brought up isolated from peers but with their mothers were less well adjusted.[234] In a further work, the Harlow's discuss a study of one female monkey, who had been raised in isolation and then put in with experienced males and who was infertile. This female was left in the company of the males longer than those who had conceived. After some months the sexual behaviour of this female became normal, implying that the condition brought about by isolation may be reversible.[235] Morgan notes that various studies support differing degrees of reversibility[236] but suggests that since these experiments involved total social isolation they should not be extrapolated to human institutional (and similar) care.[237] Since these early works, ethological studies have continued with both Bowlby[238] and others[239] drawing many connections between ethology and maternal deprivation, but at the same time, criticisms such as those of Morgan and Clarke (see above) have also been maintained.

6.42 Cultural Studies

The two main (and interlinked) strands in this area are those of anthropological and sociological studies. The work of Orlansky (an anthropologist) has already been mentioned in the early phase of the controversy[240] but more recent authors have also been critical, with the main thrust of these criticisms being that twentieth century industrialised family life has been taken as the norm and hence, by definition, any type of child care that does not conform to this is abnormal. Moore, for example, has suggested that American social scientists were "doing little more than projecting certain middle class hopes and ideals onto a refractory reality"[241] and Calvin claims that "Sociologists such as Parsons who speak of the father's instrumental role and the mother's expressive roles in family life, fairly ossified the options open to adults."[242] The male 'instrumental' role is that which deals mainly with the external world, whilst the female 'expressive' role deals with harmonisation and committment within the group. Rapoport, et al, suggest that Parsons has been the most influential sociologist of the western family[243] and claim that Parsons' position argues that "the contemporary, relatively isolated nuclear family with it s contemporary sex-linked division of labour, fit it s environment and was functional both for society and for individual family members."[244] It is possible that some sociological support (or opposition) for the maternal deprivation theory is not 'independent testimony'. For example, Bocock has suggested that Parsons "tried to link his sociology systematically with aspects of Freud's theory of the development of personality"[245] and, if this is the case, then it is hardly surprising that Parson's work supports the psychoanalytically based maternal deprivation theory. (For examples of Parson's work see his 1942,[246] 1949,[247] and 1964[248]). Other social scientists have disagreed with Parsons, notably Margaret Mead, who stated

> "At present, the specific biological situation of the continuing relationship of the child to it s biological mother and it s need for care by human beings are being hopelessly confused in the growing insistence that child and biological mother or mother surrogate, must never be separated, that all separation, even for a few days, is inevitably damaging, and that if long enough, it does irreversible damage .... Actually, anthropological evidence gives no support at present to the value of such an accentuation of the tie between mother and child. On the contrary, cross cultural studies suggest that adjustment is most facilitated if the child is cared for by many warm friendly people."[249]

In a more recent article Mead made out the case for child care being wholly related to culture rather than some ideal method of rearing

> "... the accumulated evidence from primitive societies suggests that at a very early stage in human history, traditional modes of behaviour were evolved which were related not to any instinctive pattern of neonatal mother-child relationship ... but rather to other parts of the learned behaviour of the particular people, their mode of life, means of transport, type of shelter, system of kinship organization, methods of economic exchange and beliefs about the soul and the cosmos."[250]

It can be seen from these above quotes that maternal deprivation theory was receiving support and critical attention not only from within psychology but from a wide range of disciplines. In the next section

I will consider policy implications which writers in the area have drawn from their studies.

6.5 Policy Implications and Advice.[251]

It is apparent that many of the authors of reports, reviews and studies in the area of maternal deprivation have been keenly aware of the policy implications of their writings. Large numbers of these have made explicit suggestions with regard to policy both in 'scientific' and 'popular' publications. Furthermore, in the area of popularisation, newspapers, television and radio have all brought these implications to public notice. In this section some of the advice given with regard to policy will be reviewed and this will be followed by consideration of actual policy recommended and followed. The policy suggestions made with regard to child care may be divided into two areas. Firstly, those relating to short term separation, which includes the rights and wrongs of maternal employment, nursery education, hospitalisation of children, etc., and secondly, the effects of long term or permanent separation which includes adoption, institutional and foster care.

Short Term Separation

Early in the 1950's two influential reports were produced in this area. Firstly, the World Health Organisation Expert Committee suggested that the use of day nurseries and creches could lead to "... permanent damage to the emotional health of a future generation."[252] Secondly, the Bowlby monograph (discussed above), published in the same year, suggested that full time employment of the mother leads to a break up of the natural home group and hence is a "... potential source of deprivation."[253] He went on to claim that the primary motive for mothers working is financial need and so, government aid (allowances,etc) could obviate the need for maternal employment.[254] Baers[255] concurred with this view, though she suggested a further reason for maternal employment is that some mothers have been "... conditioned by a certain type of feminist propoganda."[256] In her conclusions Baers is less tentative than Bowlby and states, "The mothers' place is unquestionably in the home if there are very young children or many children in the family,"[257] and, "... anything that hinders women in the fulfillment of this mission [motherhood] must be regarded as contrary to human progress."[258]

In 1953 Bowlby published a popularised version of his monograph entitled 'Child Care and the Growth of Love',[259] and in 1958 a further publication on the same theme called 'Can I Leave My Baby?'[260] In it he advised the mother that

> "Leaving a small child whilst you go out to work needs care. If your own mother is living nearby or a dependable neighbour can be a daily guardian it may work out all right, but it needs regularity and it must be the same woman who cares for him."[261]

whilst, "Leaving a child in a residential nursery is usually a bad idea ... it is bound to upset him."[262]

Both Bowlby, in the above publication, and Winnicott (another psychoanalyst), in a popular publication of his own,[263] and in broadcasts on national radio (on which the book was based), built up the idea of the mother and reduced emphasis on the role of the father as carer for children. Firstly, Bowlby describes the role of the father as a cheer and support to the mother. "It is this indirect but immensely important way, through keeping his wife secure and happy, that fathers play such a vital part where their children are concerned."[264] Secondly, Winnicott stated that "The ordinary good mother knows without being told that ... nothing must interfere with the continuity of the relationship between the child and herself."[265] The role of the father is to "... help protect the mother and the baby from whatever tends to interfere with the bond between them which is the essence and the very nature of child care."[266] Another very influential author, in books aimed primarily at a North American audience, was Dr. Benjamin Spock. (In a newspaper interview Bowlby was once described as the British Spock[267]). Spock also claimed that the mother shouldn't work unless financially essential and in his earlier publications laid little emphasis on the role of the father in child care, though his more recent books have modified this stance somewhat.[268]

At the same time as these books were being published, a similar viewpoint was being stressed in popular magazines. For example, in 1950, Parents Magazine published an article 'Should a Mother Work?', in which it was said

> "A mother must ask herself whether her working will result in a happy child, a satisfied husband, a companionable home life, a better community. Or will her working cause her youngster to feel deprived of a normal happy childhood, her husband to feel he is an inadequate mate and provider? Will her home become a schedule ridden household? Because of her decision to work will the community have to deal with a broken home or a potentially delinquent child?"[269]

It seems very likely that these books, articles and radio broadcasts exerted a major influence on a generation of parents (particularly women), and that this influence relates in large part to the 'common-sense appeal' of the ideas, and to the certainty with which they were expressed. (For a more complete review of 'expert advice' in the popular media see Rapoport, et al.[270]). Not all writers were as certain of the dire effects of mother - child separation. For example, Cartwright and Jeffreys, writing in 1958 could find no physical or psychological symptoms in the children of working mothers,[271] Stoltz noted the disparate nature of experimental findings and commented "One can say almost anything one desires about children of employed mothers and support the statement by some research study",[272] and, more recently (in 1978) Belsky and Sternberg commented on the effects of day care

> "To even say that the jury is still out on day care would be, in our view, both premature and naively

> optimistic. The fact of the matter is, quite frankly, that the majority of the evidence has yet to be presented, much less subpoenaed."[273]

Other authors have questioned the relevance of studies based (mainly) on poor institutions, to the question of whether or not mothers should work. For example, Wallston suggested that "... the study of the effects of maternal employment must be separated from the research on maternal deprivation where institutionalised children have primarily been studied;"[274] Tizard, in reviewing a study of institutionalised children, stated "It is perhaps worth emphasising that the findings of the study have no bearing on the question of whether mothers should go out to work;"[275] and Rapoport, et al, comment "Bowlby's work focused on the consequences of extreme deprivation and the situations he wrote about do not relate directly to ordinary family life."[276]

Some of the major opposition to the work of Bowlby, Winnicott and others, has not been from 'experts' but from those with a feminist perspective. For example, Bruch, writing in 1952, described the work of the deprevation theorists as a new and subtle form of antifeminism in which men, under the guise of stressing the importance of maternity, tied women more tightly to their children[277] and more recently Freidan,[278] Millett[279] and Greer[280] have all developed and expanded upon this theme. Not all of those writing from a feminist perspective have been highly critical of psychoanalysis. For example, Mitchell noted that "... psychoanalysis is not a recommendation for a patriarchal society, but an analysis of one."[281] However, with regard to the application of this analysis, Mitchell would seem to concur with the above authors.[282] Bowlby has commented on the views of his feminist opponents

> "I think it is just sour grapes suggesting that there's something wrong with a woman devoting herself entirely to her child. Why shouldn't she if she wants to? I suspect that some of these Women's Lib people aren't capable of enjoying it. Many of them may have had poor mothers themselves."[283]

The picture that emerges is of a polarisation between, on one side, 'experts' emphasising the essential nature of maternal care and, on the other, a group of (mainly) feminist writers, with a middle ground occupied by scientists, questioning the relevance and certainty of any conclusions drawn. Of these groups, the former has had by far the greatest influence on both parents and child care workers, though it has been suggested that this influence is decreasing.[284]

In the second area relating to short separation caused by hospitalisation of children, there has, until recently, been much greater unanimity of opinion. The most influential writer in this area has been James Robertson, a colleague of Bowlby. In 1958 he wrote what was essentially a memorandum to the Platt Committee on the Welfare of Children in Hospital (see below). In this he stated that children in hospital can become seriously maladapted, both temporarily and permanently, due to separation from their mothers. From this premise he suggested that hospital visiting should be permitted at any time. (It was common hospital practice to either forbid parental visiting during the child's entire stay in hospital or to restrict visiting to one hour or less per day.) Further than this, he claimed that ideally mothers should stay with their children and that each nurse should look after the same few children on each shift in order to fulfil the role of 'mother substitute'.[285] Following this publication Robertson wrote a series of articles in the Observer newspaper,[286] and two films on the subject were shown on B.B.C. television.[287] This elicited a large written response from parents and these were published with an introduction by Robertson in 1962.[288]

These films and writings are claimed by Morgan to have strongly influenced both the Platt Committee Report and parental ideas.[289] As as example of this Morgan quotes a letter read on the radio, from a blind mother. This letter describes:

> "... how she might have to give up the chance of a lifetime (to go on a course on handling a guide dog), because she had heard, from descriptions of Robertson's work, how near her selfishness had gone towards doing permanent mental damage to her small child."[290]

Before and since the publication of the Robertson books there has been agreement on the fact that children get upset in hospital, though more recently, the mechanism of this distress has been questioned by Stacey, et al,[291] and most recently Brown has put forward an alternative hypothesis to that of the maternal deprivationists which "... locates the problem in a disruption of the child's social life when he is uprooted from his family and friends and deposited in the hospital ward."[292] Thus, in the area of short term separation, there has until recently, been agreement that children become upset or disturbed by hospitalisation and that the cause of this is the separation of mother and child, though now this cause is being questioned from some quarters.

<u>Long Term Separation</u>

There are two areas of long term separation which have been widely discussed. These relate to institutional and foster care and to adoption.

<u>Institutional and Foster Care</u>. The main thrust of argument in this area has been the rights and wrongs of 'taking children into care' with the attitude of supporters of maternal deprivation summed up as 'better a bad home than a good institution'. Bowlby's monograph discussed this area in some depth and it is worth quoting his view on the subject

> "It must never be forgotten that even a bad parent who neglects her child is none the less providing much for him. Except in the worst cases she is giving him food and shelter, comforting him in distress, teaching him simple skills and, above all, is providing him with that continuity of human care on which his sense of security rests. He may be ill fed and ill sheltered, he may be very dirty and suffering from disease, he may be ill treated but, unless his parents have wholly rejected him, he is

secure in the knowledge that there is someone to whom he is of value and who will strive, even though inadequately, to provide for him until such time as he can fend for himself.

It is against this background that the reason why children thrive better in bad homes than good institutions ... can be understood."[293]

It is accepted that some children will need to be removed from their homes, but this is permissible, "Only if the social worker, the doctor or the magistrate has a well considered long term plan for the child.."[294]

The cry 'better a bad home than a good institution' was widely taken up (see, for example, Myrdal and Klein[295]), and led to a reluctance by social workers to remove children from their parents. In the view of Howells, "The exaggerated emphasis on separation as an evil in itself has led to disturbing tendencies in child care, there is a reluctance in some quarters to rescue a child suffering privation in his own home ..."[296] and according to Cooper

"... the mistaken notion that separating a child from his family is always a last resort and harmful is widely believed by social workers. In fact, the contrary is true. Whatever the age of the child, removal from traumatic physical battering is always beneficial if the substitute care provides adequately for the child's needs."[297]

Wooten, writing in 1962, claimed that the main service rendered by those studying maternal deprivation was to indicate how bad the care in some of these institutions could be, though this did not prove that maternal deprivation existed

"By calling attention to the imperfections of many existing children's institutions, the separationists have undoubtedly rendered a valuable social service ... When Elizabeth Fry exposed the insanitary conditions that obtained in the nineteenth century prisons, no-one applauded her for her discovery that good sanitary conditions were to be desired; the merit of her work was it s demonstration that such conditions were not to be found in prisons."[298]

This sentiment was echo'ed by Glaser and Eisenberg who called for better planned foster homes and improved institutions that were more 'home -

like'[299] and later Glaser argued that "We have to change our concept from 'any home is better than an institution,' to 'a good home is better than a good institution.'"[300]

Institutionalisation was, and is, seen as a temporary step en-route to the return of the child to it s natural parents or adoption, with foster care also seen as temporary. In the words of Bowlby

> "It is the realisation that the child in a foster home (or institution) is living in two worlds - the foster home (or institution) and his own home - which has led to a new outlook in child care ... However good the foster mother or house mother, the child will regard her as a more or less poor make-shift for his own mother to be left as soon as possible. Only if the child is placed before the age of two is he likely to feel otherwise."[301]

These sentiments were (and are) widely held and where separation from the natural parents was to be permanent, adoption is seen as the best option.

<u>Adoption</u> In the early years of this century adoption was seen as 'a service to childless couples', with emphasis on late adoption, to ensure that only 'perfect' children with no congenital abnormalities were adopted. This is in contrast to the more modern view that the rights of the child are paramount, and hence, early adoption is highly desirable.[302] The impetus for early adoption came, not surprisingly, from the maternal deprivationists. For example, Bowlby stated that it was "... in the interests of the adopted babies mental health for him to be adopted soon after birth."[303] He listed the possible arguements against early adoption, which are firstly, that it requires a precipitate decision by the mother, secondly, the baby cannot be breast fed and, thirdly, that there is less opportunity to assess the baby's potential development.[304] It is concluded that these argu ments are not very strong and so "On psychiatric and social grounds adoption in the first two months should become the role."[305] Stone agreed with this view, although noting that the evidence was not complete,[306] whilst

Goldstein, et al, in a recent work used "... psychoanalytic theory to develop generally applicable guidelines to child placement,"[307] and believe that "Adoption in the early weeks of an infant's life gives the adoptive parents ... the chance to develop a psychological parent-child relationship. This chance is diminished if adoption occurs at a later stage."[308] Other authors have gone further than this, taking the rather pessimistic view that if some form of permanent home is not established in the first three years "... then however good the subsequent mothering, it may not be possible to make up for the damage done by early deprivation. The child's basic trust is never established, he becomes an affectionless and delinquent character,"[309] and Hann (a journalist) notes the commonly held view that "... six weeks is probably too late for an ideal adoption and a baby can definitely become disturbed after 3 months."[310]

It is against these very pessimistic views that most of the argument has been raised, particularly with the implication that if a child is not adopted early in life then permanent damage is the inevitable result which later adoption can do little or nothing to prevent. Seglow, et al, agree that early adoption is best but suggest that a child who is placed later is not necessarily more disturbed,[311] and so, "... adoption should be more readily considered for older children"[312] whilst Kadushin suggests that: "Agencies can take the risk in placing older children with a high probability of success."[313]

Thus, in the area of long term separation there are two main points of disagreement - Is any home better than a good institution? and, secondly, Can adoption of older children succeed? It is worth bearing in mind that none of the above ideas are static and change is gradually occurring, but to a greater or lesser extent many of these ideas still hold and all of them have been of major importance in the recent past.

In the next section some aspects of government policy with respect to child care will be discussed.

6.6 Government Policy

Before beginning discussion of policy issues, two caveats must be noted. Firstly, much of the discussion here will centre on reports to government rather than policy (legislation, etc) itself. It is recognised that these are by no means the same thing but, in most cases, actual policy and recommended policy are very similar and the reports serve as a background to policy decisions, as an indicator that policy didn't 'just happen'. Secondly, only a percentage of relevant reports, etc., will be considered, sufficient to give a 'broad flavour' of policy direction. Those quoted are representative of the whole. For more widespread consideration of Acts pertaining to children see Boss,[314] Randall[315] and Smith.[316] For the purposes of this discussion the policies have been divided into two time periods. Firstly, before about 1950, when it seems likely that the impact of maternal deprivationist findings will have been fairly small and secondly, post 1950 when the impacts of the findings (if any) are likely to be at their greatest.

6.61 Pre 1950 Policy

In the early and mid nineteenth century there was increasing emphasis on education for both working and middle class children and infants,[317] and in 1870 an Education Act was passed which set up School Boards. In 1880 elementary education became compulsory for all children over the age of five years with the schools also admitting large numbers under this age.[318] Blackstone lists one of the main objections to this situation to be "... at this age [children] ... should remain at home in the care of their mothers. This objection was not overcome and it probably increased rather than receeded."[319]

Some years later (in 1905) the situation was considered by the Women Inspectors of the Board of Education,[320] and shortly afterwards, by the Consultative Committee of the Board of Education[321] whose term of reference was "To consider and advise the Board of Education in regard to the desirability or otherwise both on educational and other grounds of discouraging the attendance of school under the age of (say) five years."[322] Both the committee and the Inspectors came to similar conclusions, that it was best for children aged between three and five to stay at home with their mothers, provided that home conditions were satisfactory, though where they were not, some form of nursery education was recommended.[323] Legislation to aid this was enacted in 1918, when Local Authorities were given powers to make arrangements to supply, or aid the supply, of nursery schools for "Children over two and under five years of age ... whose attendance at such a school is necessary or desirable for their healthy physical and mental development."[324] At the first reading of the bill, the President of the Board of Trade, Herbert Fisher, stated that where home was satisfactory children under five should stay with their mothers.[325]

It has been suggested that these provisions were stimulated by the economic boom which had occurred during and shortly after the First World War,[326] and that following the slump in the early 1920's, little action was taken. An example of this may be found in the Geddes Report of 1922[327] which recommended the raising of the lower age of entry to schools to six years - with an estimated saving of £1,785,000 in 1922/3.[328]

At this point, it is worth considering the effects of the First World War on women and work. Early in the war the number of women in work actually fell since the majority of women's employment was in the 'luxury' trades such as millinary and dress making and demand for these goods fell.[329] By mid 1915 substitution had begun, that is, the extensive move of women to industry into what had formerly been male jobs. This movement increased in 1916 and 1917 leading to a labour shortage in 'traditional' women's jobs as women moved into the better paid industrial work.[330] Women were praised at this time for their 'spirit of patriotism',[331] and in 1916 the Ministry of Munitions made grants in aid for the establishment and maintenance of nurseries,[332] though their report notes that some factories would not employ women with children under 12 months old.[333] An article at this time in 'The Lancet' suggested at "The industrial employment of married women must necessarily involve some neglect of the home and, especially, of any young children."[334] Some years later, near the end of the war, a government report discussing women at work noted an earlier study which suggested that poverty was more injurious to child health than was maternal employment,[335] but, at the same time, recorded the fairly common belief that both were 'evils' with poverty only being the lesser of these.[336] When the war ended it was widely assumed that women would stop work, or at the very least, go back to traditional women's jobs. Initially there was a reluctance by women to leave well paid industrial jobs but as demobilisation of the armed forces proceeded, public pressure grew[337] and by 1921 the proportion of gainfully employed females was slightly less than it had been in 1911.[338]

The emphasis on women remaining in the home was maintained and reiterated in 1933 by the Hadow Report on Infant and Nursery Schools.[339] In the report it was stated that

> "The natural and best environment for a child up to the age of five is at home and his natural guardian is his mother. Economic conditions, however, often oblige the mother to go out to work so that the home ceases to provide the right environment or guardianship. This was easily recognised and it became usual

and in some parts of the country is still usual, to allow children under the age of five to attend public elementary schools."[340]

These views remained unchanged until the outbreak of the Second World War when "The need for married women in the labour force was important in bringing about further action."[341] A useful indicator of this change is the wartime development of nursery places. (Table 6.1).

Date	Number of Nurseries			Number of Places		
	Part Time	Full Time	Total	Part Time	Full Time	Total
1941 (July)	82	36	118	ng	ng	ng
1942 "	144	500	644	ng	ng	ng
1943 "	127	1218	1345	4103	54613	58716
1944 "	112	1446	1558	3701	67546	71256
1945 (Jan.)	104	1431	1535	3501	67749	71250

Table 6.1 Numbers of Wartime Nursery Places.[342] (ng = not given)

In addition to these wartime nurseries there were, in 1943, seventy nursery schools and 570 nursery classes catering for a further 28000 children.[343] One of the ways in which women were persuaded to go out to work during the war was by the production of government films which showed "... happy, well looked after, contented infants, enjoying themselves in creches while their mothers worked - not at all upset by their absence,"[344] and this was backed up by a Ministry of Education booklet, 'not yet five', showing the advantageous educational and developmental functions which nurseries could provide.[345]

Following the cessation of hostilities hope was expressed that nursery services would be maintained and expanded[346] and in 1947 the Minister of Labour and National Service appealed for more women to work since "Women now form the only large reserve of labour left..."[347] The Minister emphasised that he did not want women to do jobs normally done by men (as had been the case during the war), or want women with very young children to seek employment, "... although for those ... who had children a little older, there were in many places day nurseries and creches.[348] The Minister also noted that the labour shortage was temporary and, as demobilisation continued, there was debate with regard to "... the extent to which public policy and the public services should facilitate employment of mothers with young children."[349] This view was also expressed in a Ministry of Health circular produced in 1945. It is rather long but is worth quoting at length

> "The Ministers concerned accept the view of medical and other authority that in the interests of the health and development of the child, no less than for the benefit of the mother, the proper place for a child under two is at home with his mother. They are also of the opinion that under normal peacetime conditions, the right policy to pursue would be positively to discourage mothers of children under two from going out to work; to make provision for children between two and five by way of nursery schools and nursery classes; and to regard day nurseries and day classes as supplements to meet the special needs ... of children whose mothers are constrained by individual circumstances to go out to work or whose home conditions are in themselves unsatisfactory from a health point of view..."[350]

Thus, although some provision was to be made for nursery education, this provision was, in the main, much as it had been for the previous fifty years - for the children of mothers who had to work for financial reasons or whose home environment was poor.

The institutional care of children received less national attention than did nursery provision in the first half of the century. Prior to the end of the Second World War, care was fragmented between local and central authorities and dominated by Poor Law legislation which had accumulated over the previous century.[351] Dissatisfaction with the system, led in 1944 to the setting up of the Curtis Committee with terms of reference "... to inquire into existing methods for children who from loss of parents or from any other cause whatever are deprived of a normal home life with their own parents or relatives."[352] One of the probable reasons for concern at this time was related to the state of children evacuated or orphaned during the war, coupled with a general climate of social reform extant at the time.[353] The evacuation of nearly one million children in 1939 - 40[354] had brought to the notice of large numbers of the public the poor situation of inner city children and a report of 1943 noted that some of the evacuated children "... were dirty and verminous, guilty of eneuresis and soiling both by day and night... that some of them were destructive and defiant, foul mouthed, liars and pilferers."[355] This report, and public contact with the evacuees increased concern,[356] to which the Curtis Report of 1946 was a response. The Committee examined all forms of child care from workhouses and institutions to nurseries and adoption. Most interesting are their comments on some of the institutions visited. Of one of these it was noted "The accommodation is typical of the ordinary workhouse wards. They are adequate in size but generally unsuitable for children ... It is an ordinary institution with very little equipment,"[357] of another, "The children did not have any toys at all," and a third, "There is no indoor playroom and there are no toys."[358] Other workhouses were found to be better than this but there are frequent references to poor physical care, dirty clothes, and so on. Several of the statements in the report are remarkably similar to those found in contemporary (and later) studies of maternal deprivation. For example

> "It was the exception rather than the rule to find children in the homes who were not either unduly hungry for attention from visitors, or more constrained in their relation with adults than is usual for children of their age."[359]

Amongst those giving evidence to the Committee were Bowlby and Winnicott and it might be thought that their evidence and deprivationist findings were influential in determining the findings. This, however, seems unlikely since the Committee found explanation for their findings in the common practice of separating infants and older children, "... The babies consequently suffered a certain loss of stimulus and interest and, in some cases, they actually appeared to be retarded. Where there was more mixing of ages the toddlers appeared better developed and they talked more."[360] Problems such as bedwetting are attributed to lack of attention, destructiveness to lack of play materials and the reputation for pilfering to stem from only a few of the more difficult children.[361] The study of foster homes convinced the committee that these were preferable to institutional care[362] and on adoption the committee

> "... investigated a suggestion that an abnormally large proportion of children in approved schools were adopted children. An inquiry covering a sample of 11000 boys in approved schools did not indicate that there is any significant difference between the proportion of adopted children coming into these schools and the general population."[363]

The Curtis Report led to the passing of the Childrens Act which came into force on the fifth of July, 1948. This act heavily emphasised the desirability for the early return of children in care to their parents. Care with a local authority was seen as very much a second best option, but there was little guidance as to what resources could be devoted to helping families stay together. This problem was, to some extent, cleared up by a Home Office circular of the same year which suggested that if a home could be improved to make removal of a child unnecessary, or where a child could be restored to his parents, this option should be pursued.[364] Other than this report, little consideration was given to the effects of adoption on children in the first half of the century and equally little to the effects of hospitalisation. This situation changed after 1950 and these changes will be considered below.

6.62 Reports and Policies after 1950

In what might be called 'the maternal deprivation era' all aspects of child care have been considered. One of the earliest areas to receive consideration was that of adoption. In 1954 the Hurst Committee met to consider adoption practices[365] and heard evidence from many groups and individuals, including Bowlby. Adoption had been put on a legal basis in England by the Adoption Act of 1926 but there had been changes in practice over time and the Hurst Committee aimed at achieving a more uniform situation. The Committee noted that

> "A few adoption societies told us that they prefer not to place the child until he is about three months old, the chief reason being that they consider that the child's future development can then be more easily assessed. On the whole, however, the consensus of opinion was that efforts should be made to settle the child in what is to be his permanent home by the time he is three months old because after that age, the disturbing effects of any uprooting are liable to be serious."[366]

The Committee concurred with this consensus and their report led to the passing of the Adoption Act of 1958 which embodied most of their recommendations.[367] There are problems in assessing changes in age of adoption brought about by the act. The reason for this is the very large numbers of variables which might affect adoption rate. These include birth rate, illegitimacy rate, numbers of parental deaths and divorces, numbers of prospective adopters and, after 1967, the legalisation of abortion. Naturally, all of these affect the numbers of early adoptions which could take place. Bearing these influences in mind, the Office of Population Censuses and Surveys present figures which do show an increase in the percentage of early adoptions (Table 6.2)

Table 6.2 Percentage of children in different age groups adopted in 1951 and 1968 (percentages are rounded up).[368]

Age of Adoption	1951	1968
Under 1 year	36	51
1 - 2 years	16	14
3 - 4 years	13	11
5 - 9 years	25	15
Over 10 years	11	9
Total Number	14198	24831

Unfortunately, the figures are not broken down below one year of age so the prevalence of very early adoptions cannot be ascertained. None the less, an increase in the adoption rate of children under one year of age can be seen. Incidentally, the influence of some of the above mentioned variables is nicely illustrated by the fact that 25% of those adopted in 1951 were aged 5 - 9 years. This is presumably related to parental death during the war, increased illegitimacy rate and, possibly, the break up of wartime marriages.

A more recent indication in the change in official attitudes to adoption may be illustrated by a quote from an appeal court judge, Lord Justice Cross, who, commenting in 1970 on a mother's refusal to allow adoption of her child by it's foster parents said

> "Before the war it was, I think, generally assumed that although a child might be made temporarily unhappy, a young child would not be lastingly disturbed by being transferred, even after a prolonged stay, from the care of foster parents or prospective adoptors to his natural parents, if both were equally well qualified to look after him. But nowadays, specialists agree in saying that there is some risk of lasting emotional disturbance to any child who is removed from the care of one woman to that of another between the ages of six months and two and a half years."[369]

In the field of delinquency similar attitudes on family stability were being expressed. For example, in 1955 a Home Office Memorandum stated

> "It is accepted that every child should be brought up in his own home unless separation from his family is unavoidable. The shock to a child of parting, even temporarily, from the people he knows and particularly from his mother, and from his familiar surroundings, is severe and may cause him lasting harm."[370]

This emphasis on the family was maintained by the 1960 Ingleby Report on Children and Young Persons which, when considering delinquency concluded

> "The problem is always one of the child in his environment and the immediate environment is the family to which he belongs. It is the situation

and the relationships within the family which seem to be responsible for many children being in trouble."[371]

Similar sentiments were expressed in a Government White Paper of 1965. "It is at least clear that much delinquency - and indeed many other social problems - can be traced back to inadequacy or breakdown in the family."[372] Attempts to overcome this problem (based on the Ingleby Report) had begun in 1963 with the Children and Young Persons Act of that year. The act called on local authorities to assist and advise families such that the number of children taken into care was reduced.[373] Whilst these ideas are not strictly 'maternal deprivationist', they clearly have their roots in the doctrine that the splitting up of families means mother-child separation which is regarded as a bad thing.

Consideration of the separation of mother and child due to the hospitalisation of the child was also taking place at this time, notably in the Platt Report of 1959.[374] The Committee believed that, "Admission to hospital appears to be potentially more damaging than any other common form of separation because it so often involves an element of fear."[375] To ameliorate this problem the following suggestions were made, firstly, to nurse a sick child at home where possible,[376] secondly, to make visiting hours longer and more flexible[377] and thirdly, to make provision for mothers to stay with their children in hospital.[378] It is difficult to know the extent to which this first suggestion was taken up, but the second, flexible visiting, has become very widespread during the 1960's and 70's. Accommodation has been provided in some hospitals but only to a limited extent. For example, in 1961 the Minister of Health was asked in Parliament to what extent new or reconstructed children's wards built or under consideration since the publishing of the Platt Report, contained accommodation for mothers with children under five years of age. The Minister replied that "... seventy five schemes for 131 new or reconstructed wards have been approved and 27 schemes for 44 wards are under consideration. Forty four schemes make specific provision for mothers of children under five,"[379] i.e. 102 schemes were approved or under consideration, for a total of 175 wards, and of these schemes 44 made specific provision for mothers of children under five - only 43% of the total.

As before 1950, the main area of concern and debate with regard to child care was in connection with the rights and wrongs of maternal employment and nursery education. The attitude to working mothers and nursery care would seem to be unchanged from that expressed in the early years of the century. Numerous examples of this attitude may be found. For example a Ministry of Health circular of 1968 stated

> "Since the issue of Circular 221/45 [see above] much attention has been focused on the needs of children and on social situations that can endanger family stability. Day care is one way in which help can be given, but it must be looked at in relation to the view of medical and other authority that early and prolonged separation from the mother is detrimental to the child, that wherever possible the younger pre-school child should be at home with his mother, and that the needs of older pre-school children should be met by part-time attendance at nursery schools or classes."[380]

This view was supported by the Seebohm Report, also published in 1968

> "It is widely accepted that it is detrimental to the child to be separated from it s mother for long periods during early childhood, and the decline in the number of local authority day nurseries since the war is the outcome of official acceptance of this view."[381]

The Committee was 'helped on an informal basis' by Bowlby.

The 1967 Plowden Committee[382] on Schools in Britain, also took note of 'The Freudian Scheme'[383] and the work of 'Bowlby and others'[384] and concluded that, where possible, nursery education "should be part-time rather than whole time because young children should not be separated for long from their mothers."[385] The Committee's position seems to be rather an ambivalent one since they note, firstly that "The government, for reasons of economic policy, wish to see more women working"[386] but, secondly, "that mothers who cannot satisfy the authorities that they have exceptionally good reasons for working should have low priority for full time nursery education for their children."[387] The actual number of local authority day nurseries was, in 1969, 444 providing 21000 places,[388] compared with 21250 places in 1945 (see above). Recognition, by the government, of the demand for more nursery places led to a policy of expansion. These plans were outlined in a 1972 White Paper 'Education : A Framework for Expansion.'[389] In it the government outlined their aim as "... within the next ten years nursery education should be available without charge ... to those children of three and four whose parents wish them to benefit from it."[390] The nursery education would only be part time[391] and specific mention is made of the benefits to children "... whose home and life are restricted for whatever reason."[392] In the same year that this white paper was published, Mrs. Margaret Thatcher, the Minister of Education, made it plain that nursery care was not to be regarded as a substitute for maternal care "... the Government plans for an expansion of nursery education intend this to be for three hours a day only, so that mothers cannot avoid their full time task of child care."[393] The Finer Committee on One Parent Families, reporting in 1974[394] also agreed that nursery education should be part time, but recognised that this

> "... presupposes a satisfactory home life situation in which the mother is able to look after her child adequately for the rest of the day. The hours she works or any number of adverse social conditions, such as poor housing, social isolation, a large family or her illness or incapacity or sometimes a combination of these circumstances may prevent her from doing this and make it necessary for her child to be given full-day care."[395]

Coming right up to date, two quotes from members of the current government will be used to conclude this section. Firstly, the Prime Minister, Mrs. Margaret Thatcher, "It is a woman's job to be doing that most important work of caring in the home because women bear the children and create and run the home."[396] Secondly, Dr. Rhodes Boysen, the Under Secretary for Education and Science, "Many people feel that it is always satisfactory when the mother can be at home with the children." Boysen then quotes a speech with which he apparently agrees, stating that, "I do not see it as the duty of the government to see that there is national [nursery] provision for every child up to the age of five."[397] This concludes the account of the maternal deprivation controversy and it s associated policies. I will next consider what inferences may be drawn from the story.

6.7 Discussion

The controversy was clearly a multidisciplinary one, with involvement and comment from sociologists, anthropologists, biologists and psychologists. The main protagonists were psychologists, and on the surface this might appear to cast a degree of doubt on the importance of interdisciplinary conflict in maintaining disputes, since it would be expected that psychologists would have a 'shared culture' and hence far greater areas of agreement than disagreement. Closer inspection, however, reveals the existence of 'schools' of psychological and psychoanalytic thought which serve as guides to research and theory for their respective practitioners. These are not paradigms in the full Kuhnian sense since they have not 'captured' the entire field of psychology, but may fruitfully be viewed as 'proto paradigms'. The existence of these schools has been considered by several authors who have termed them ideologies. The differentiations between these are somewhat crude and tend to identify only two or three categories but are a useful basis for discussion. Two of the ideologies most relevant here are the psychotherapeutic and the sociotherapeutic. The psychotherapeutic ideology "... implies a belief that mental illnes is primarily a result of early childhood experiences..."[398] with the practitioner

"... relatively uninterested in the social context and biological anlage..."[399] The sociotherapeutic ideology, on the other hand, supports the belief that "... mental illness is caused by social and environmental factors..."[400]

In a questionaire study of psychiatrists, Hollingshead and Redlich noted that the supporters of these ideologies were socially separated, had different professional societies and educational establishments and read, and wrote for, different professional journals.[401] Thus, in one sense, these practitioners can be viewed as belonging to different and competing, disciplines. This area may be nicely illustrated by consideration of Spitz's Foundling Home/Nursery study. It will be remembered that in Foundling Home the infants were kept isolated in cots with sheets hung around them, had no toys and only one nurse to eight or more children, whilst in Nursery the children were in group, with toys and much maternal contact. The delayed development of the former group was taken by Spitz as clear proof of the essential nature of maternal care, whereas for others they are clear evidence of the essential nature of external stimuli for normal development. Each of these concentrated on the evidence that they felt was most relevant and used this to draw widely differing conclusions. It is not my intention to suggest that any of these individuals were being in any way unscientific, but merely to show that what is a relevant variable depends to a great extent on one's theoretical outlook. Thus, considerations as to whether experiments were well carried out and correctly interpreted were rooted in something deeper than the practice of any single experiment - there was little or no shared culture on which discussion could be based and for psychoanalysts to admit the major influences of factors other than maternal care would, in effect, mean that they ceased to be psychoanalysts since these ideas form one of the bases of their practice; whilst the converse holds true for those opposed to psychoanalytic concepts.

This view of different psychological ideologies also explains very neatly the longevity of the controversy. The mechanisms of closure considered in Chapter 3 were bowing to authority, professional interests and decisions to maintain earlier agreements. Clearly, none of these have any force for two groups largely insulated from each other with separate trainings, professional societies, journals, etc. Thus, not only is there no interest in resolving the dispute, there is a positive impetus not to, if this means compromising one's professional theories, practices, and so forth. The implication of this suggestion is that the controversy was, and is, unlikely to be 'solved' all at once, or over a short period, but by gradual changes with individuals coming into the area with little or no commitment to one ideology or the other, perhaps along the lines of the resolution of the Baldwin - Titchener controversy considered in Chapter 3. If the multidisciplinary nature of the dispute provided the means, what was the motive? The numerous policy pronouncements of researchers in the area show that they were keenly aware of the policy relevance of their work and were prepared to publicise this via both scientific and mass media. In this sense, the controversy was a conflict by proxy, though as in the previous examples, not in the sense that the controversy was avoidable by some removal of bias. Instead, development of a controversy may be aided by the numerous opportunities for dispute in both the scientific and the policy area relating to the relevance and interpretation of animal studies, the interpretation of experiments, the relevance of certain variables, the policy applications of studies and evidence and finally, a seeming difference between those researching for 'action' and those researching for 'knowledge'. This distinction was noted in Chapter 4 and seems to have played some role in the disputes over the health effects of smoking and lead in petrol. In the maternal deprivation example it will have been noted that Spitz emphasised the utility of his results on the grounds that they worked in practice and that he played down the relevance of statistical support, whilst O'Connor noted that a drawback of research in this area was it s clinical basis (and therefore it s subjective nature). These differences certainly fit in with the suggestions in Chapter 4 relating to error cost. Those whose work is clinically based were conditioned to making decisions for practical action on evidence that, for the more academic researcher, might seem inconclusive. The case study material presented here is too limited to serve as a strong support for this suggestion but it is worth bearing in mind in areas of dispute that there may be a difference in 'shared culture' between those oriented towards action and those concerned with theory.

To summarise these suggestions. The multidisciplinary means, the political motive and the opportunity, existed for the development of the controversy. On these grounds the surprise would have been if a controversy had not developed. Next, what influence did this have on policy?

It is clear from the consideration of policies in this area that these existed many years prior to any research. Reference to Section 6.6 reveals that there were, in fact, two distinct classes of policy. The first centred on the belief that the place of children is at home with their mothers, an idea which was expressed in years as far apart as 1880 and 1982. These beliefs were not absolute, and it was accepted throughout this time that where home conditions were not ideal children might be better off outside the home for some part of the day. The second policy class may be noted at times of national need, for example during the First and Second World Wars, or when labour was in short supply. At times like these it has been considered acceptable, and indeed, praiseworthy, for women to work. Several authors have commented upon this dichotomy of policy views and suggest that women serve as a reserve labour force to be called upon in time of need and discharged when need diminishes.[402] If these suggestions are correct, and the policies reviewed support this, then policy has been made in a 'political' way largely unaffected by scientific findings. Thus, the synoptic model (or it s derivatives) do not seem to be an appropriate description of what happened. Does incrementalism fare any better?

From Chapter 2 it will be recalled that some of the basic tenets of incremental policy making related to considering only a limited number of incrementally different policy alternatives, with problems dealt with remedially and in a fragmented manner, and with ends adjusted according to means. Were these the case in the policies of this area? Firstly, it is clear that the alternatives considered did only differ in minor respects from what were then current policies. For example, nursery places were adjusted up or down, hospital visiting hours were changed and, more recently, emphasis has been placed on family support to keep families together. No radical or major changes were considered, at least publicly.

Secondly, perceived problems were dealt with in a remedial manner. The issues raised by adoption, hospitalisation and delinquency of children were dealt with not by an attempt at foresight but when they had become clearly apparent.

Thirdly, solutions presented were fragmented. Reports and studies were carried out separately into hospital care, adoption, delinquency and so on. There was no attempt at an overview of these linked areas.

Finally, means were adjusted to ends. The ends varied from the requirements of a sufficient labour force in time of national need to the attempted maintenance of full male employment. The means of achieving these was, by and large, the incremental adjustment of nursery places. Overall, policies with little or no financial cost, such as hospital visiting and earlier adoption have been pursued with far more vigour than those which would raise major financial or political issues, such as providing genuine equality for mothers. Thus, it can be concluded that these policies were carried out in an incremental manner. Could the synoptic view have been utilised? In my opinion, the answer is a definite no. The controversy in this area would have precluded any clear statement of scientific 'truths' on which to base policy and it is worth bearing in mind that research in the area began voluntarily, with little or no direct political influence. Certainly, outside influences would have been present, for example the 'received wisdom' that children were better off at home, but research was pursued in a more or less autonomous manner. Despite this, the controversy has been maintained for forty years. Policy makers demanding results on which to base policy would hardly have helped the conduct of research, and a more likely result would probably be that no policies would have been formulated.

This said, what was the relationship between science and policy? It has been noted above that policies were in place before any formal scientific findings were produced and that these policies changed according to social and political factors (e.g. during World War 2). Thus, these scientific findings did not exert any major influences on policy but they were nonetheless called upon at various times to provide support for certain policies and here they served as legitimation rather than as adviser. One of the other roles of deprivationist ideas seems to have been to raise issues of child care in institutions and so on, and here, by highlighting poor standards, the ideas seem to have performed a valuable service. This said, the Curtis Report of 1946 shows that awareness of these issues already existed, so deprivationist ideas served only to increase visibility. The controversy itself, seems to have passed more or less unnoticed in the policy arena and the fact that certain ideas were disputed did not cause any rethink in policy ideas, primarily because these ideas, being based on politics not science, could not be shaken by scientific dispute. In many ways this area is closely paralell to the early years of the British I.Q. example considered in the previous chapter; however, unlike that example, there was no major questioning of policy to bring the scientific dispute to the fore, the controversy was essentially insulated from policy. It was suggested in Chapter 4 that multidisciplinary controversies might exhibit a heightened level of criticism, particularly when there was uncertainty as to which policies to pursue (the overcritical model), and that where there was no dispute over policy then criticism would tend to be muted (the undercritical model). How does this case study, with policy agreement and a scientific controversy, fit into these categories? At times the controversy was rather heated and certainly Spitz and Pinneau pulled no punches in their exchange, with Spitz accusing Pinneau of polemic, bias and lack of knowledge. However, aside from these and a few other individual disputes of limited extent, it is difficult to find examples of the heated exchanges which have occurred in say, the lead in petrol or nuclear power disputes. The controversy seems to have been maintained on a relatively low key and suggests that the case study illustrates a special case of the undercritical (negative feedback) model where a low key scientific controversy has occurred, with this being damped down by the lack of a political platform and political response. Policy was decided on factors other than scientific ones and, as in the case of smoking and health, this resulted in a lack of interest in the information and ideas that research could provide, to the extent that obvious questions were not asked with regard to practices of child care both in institutions and the home. Thus, the agreement on policy served to constrain research efforts by non provision of funding, non provision of a 'political' platform and so on, in just the way that the undercritical model predicts. Of special interest here for policy makers is the fact that, whilst the synoptic model fails when there is no scientific agreement (because of uncertainties in what information to 'plug into' the model) and also fails when the undercritical model holds (because the issues are not sufficiently explored for enough information to be available); whilst the incremental model may be applied in both of these cases, and furthermore may take account of changes in scientific and political ideas over time as exemplified in this chapter by changes in policy due to war or economic conditions.

The next chapter deals with a further example of a policy related scientific controversy, that over the (claimed) relationship between food additives and hyperactivity.

C H A P T E R 7

HYPERACTIVITY AND FOOD ADDITIVES

7. HYPERACTIVITY AND FOOD ADDITIVES

7.1 Introduction

Hyperkinetic behavior pattern or hyperactivity (HA) is defined as a long term childhood pattern characterised by excessive restlessness and inattentiveness. It usually begins between the ages of two and six and fades during adolescence. The features most commonly associated with HA are inattentiveness, learning impediments, behavior problems (particularly in the classroom) and immaturity. These may cause major difficulties both at home and at school and are associated with an increased level of failure at examination, juvenile crime and 'emotional deviance'[1].

The incidence of HA is difficult to assess. A study in the UK found the term to be appropriate to only 0.1% of children[2], whilst in the USA estimates of incidence vary from 4 - 10%[3], and in one case as high as 28% of the school population[4]. Whether or not these variations reflect actual differences in prevalence or, as has been suggested, the use of different diagnostic criteria[5], there has been increasing concern about the problem over the past ten years or so. In large part this has been stimulated by the work of Benjamin Feingold who suggested that a major cause of hyperactivity is the ingestion of various food additives and some substances naturally present in foods. This has given rise to a scientific dispute with obvious policy implications which will be discussed below.

7.2 Possible Causes of Hyperactivity

There have been many possible causes of HA suggested. These may be classified as genetic, psychological, organic and environmental.

Genetic causes. The importance of genetic influences has been supported by the study of twins and first degree relatives which suggest that HA may have familial aspects. However since these are associated with similarities in environment and upbringing, the connection remains speculative[6].

<u>Psychological causes</u>. On this view HA is associated with methods of child rearing and reinforcement of certain behaviors[7]. It has further been suggested that a tendency to hyperactivity may be fairly common but may only be expressed in certain highly structured situations such as the classroom[8]. An extension of this view is the suggestion that hyperactivity is a social creation, a label used for children who do not suffer from any ailment "but whose behavior is regarded as troublesome to adults"[9]. Support for a psychological component in the aetiology of HA is found in the fact that behavioral therapy can be useful in treating these children[10]. Clearly however, this does not rule out other concurrent causes.

<u>Organic causes</u>. Hyperactivity was initially believed to be a manifestation of some brain damage or injury before or during birth. Though causes other than frank injury are now suspected to be important, the finding remains that HA is more common in children who have suffered some pre-natal or birth trauma such as maternal smoking or anoxia[11].

<u>Environmental causes</u>. A plethora of environmental causes have been suggested, ranging from lead[12], to fluorescent lighting[13], food allergies[14] and reactions to food additives. Whilst all of these remain contentious, most recent attention has focussed on a possible association between food additives and HA which was suggested in the early 1970's by Dr. Benjamin Feingold.

7.3 The Feingold Hypothesis

Feingold developed his ideas whilst working as a clinical allergist at the Kaiser-Permanente Medical Center in San Francisco.

In 1968 he published a paper suggesting that persons allergic to aspirin (salicylic acid) might also be sensitive to foods containing natural salicylates and to certain food additives (even if these did not contain salicylates). He recommended that these foods and additives be excluded from the diet of those showing symptoms of allergy to aspirin[15]. This elimination diet (later known as the Feingold, Kaiser-Permanente or KP diet) excluded many fruits and vegetables, large numbers of artificial flavourings and colours and also non food products containing these (such as drugs and cosmetics)[16].

By 1973 Feingold had also associated increases in the use of food additives with increases in the incidence of behavioral problems in children (particularly hyperactivity) and he suggested that the KP diet could be used to treat hyperactive children with consequent behavioral improvements, reduction in the need for drugs and improved scholastic achievements. His evidence for these effects was anecdotal and hinged on the way that certain behaviors could apparently be 'turned on and off' by the ingestion of forbidden substances[17]. The responses were no longer believed to be allergenic in nature but no specific mechanism of action was presented[18]. At around this time Feingold's ideas received publicity on TV and radio and in the newspapers[19], and in 1975 he produced a popular book 'Why Your Child is Hyperactive' which advocated wider use of the elimination diet for the whole family (to reduce dietary infractions) and suggesting that foods and other products should be labelled for the presence or absence of additives[20]. The diet proved to be extremely popular with the parents of hyperactive children and resulted in the setting up of 'Feingold Families' in the USA to support hyperactive children and their parents and to lobby for food labelling[21].

Shortly afterwards a similar group, the Hyperactive Childrens Support Group, was set up in the UK[22]. In his book Feingold emphasised that children reacting to these additives have genetic variations not abnormalities, and that no 'blame' for these attaches to either the child or its parents[23]. In later works Feingold became more specific in the responses to be expected to the diet, suggesting that younger children were more likely to respond rapidly and completely whilst older children respond more slowly and less completely. Overall 50% of hyperactive children were expected to improve, with infractions of the diet causing a recurrence of hyperactive behavior within two to four hours and persisting for up to four days[24]. In further papers Feingold reduced this expected favourable response rate to 30 - 50% [25] but continued to emphasise the nutritional non-necessity of many additives and to push for some form of labelling of foods and for a research programme and public information campaign[26].

These ideas, and particularly their rapid acceptance by the public, attracted much scientific and professional scrutiny and research. Preliminary clinical studies were, by and large, supportive of the hypothesis[27], but later 'scientific' controlled studies have been much less supportive leading to a variety of critical comment and alternative explanations being offered for positive results. These will be considered below but first I will briefly consider the use of drugs in the treatment of hyperactivity.

7.4 Drug Therapy for Hyperactivity

The use of drugs in the treatment of HA has a pedigree of several decades[28], and was initially based on study in clinical situations[29]. In this section I am not concerned with the studies themselves, which are reviewed in the literature referred to, but in some of the problems encountered with the study and the use of drugs to treat hyperactivity. Firstly, the studies themselves. Several authors have pointed out the difficulty in separating real and placebo effects[30], particularly since the physiological effects of the drugs mean that subjects are able to differentiate between active and placebo treatments, so the studies cannot truly be double blind[31]. Other difficulties encountered include the impossibility of controlling the numerous potentially relevant variables[32], the problems of comparing studies due to the use of different rating scales and measurements, the heterogenous nature of the population considered and to contradictions and confusions in the literature[33]. The second issue is the existence of side effects. Many have been reported including a worsening of hyperactive behavior, anorexia, insomnia, gastro-intestinal disorders, slowed sexual development and reduced growth rate[34]. Finally, concern has been expressed that drug treatment may instill in the child a belief that he or she lacks self control and that without his or her daily 'pill' bad behavior is to be expected[35]. Despite these issues a newspaper article reported that in 1970 between 5 and 10% of Omaha's 60,000 school children were receiving drugs to modify behavior[36]. A more recent estimate suggests that between 1.7 and 1.8% of US schoolchildren receive drugs for hyperactivity (with UK use being one tenth of this)[37]. Several writers have commented on the 'gross overuse' of drugs in the USA[38] and have found explanation for this in the 'push' of drug companies aiming to increase sales[39]. In the UK marketing is apparently less aggressive[40]. Of course this push by itself could not achieve wide prescription of drugs but this, it is suggested, has been accompanied by changes in the attitudes of physicians, teachers and parents such that "Pharmacological cures for social problems appeal to the mind set of our society. They suggest the possibility of a 'fix' without the need to examine fundamental questions..."[41], and furthermore the prescription of drugs allows the doctor to be seen to be 'doing something'[42].

I will not further consider drug studies at this point but some of the issues raised will be referred to later. Next I will turn to some of the work which has been carried out on Feingold's ideas.

7.5 Experimental Tests of Feingold's Hypothesis

In this section I will concentrate on critical comments applied to experiments both for and against Feingold's ideas (and replies to these where made). Most early tests of the hypothesis were uncontrolled and clinical in nature. Their prime aim was to treat children suffering from hyperactivity and they were, by and large supportive of the idea that additives influence hyperactive behavior. Critical comment upon these revolved around the methodological weaknesses of non-blind studies, the non specificity of Feingold's ideas (making tests of these difficult) and alternative explanations of positive results. Based on these comments criticism has also been made of the ethical aspects of the use of a non proven therapy and the possible social and nutritional risks of the diet. Most later studies were controlled and double blind and in many cases have not supported the hypothesis. Critical comment upon these has attempted to highlight weaknesses in their conduct in an attempt to explain their failure to achieve positive results. Three areas of debate may be identified. Firstly, the pro's and con's of controlled and uncontrolled studies, secondly, the relevance of the experiments to the hypothesis, and thirdly, the social and nutritional aspects of the diet.

7.51 Controlled and Uncontrolled Studies

Many of the early tests of Feingold's ideas were via uncontrolled clinical studies. These have been taken to task for being impressionistic, anecdotal and non-objective investigations carried out by observers with an interest in confirming the hypothesis[43]. Further, it is claimed that these studies did not use standard rating scores[44], nomenclature, procedures and measurements, and did not use rigorous statistical tests[45]. The non-blind nature of these studies (it is claimed) left them very vulnerable to placebo effects, that is a bias in reporting and interpretations by investigators and parents with a belief in the efficacy of the diet. Other placebo effects are also suggested to account for the successes of the diet based on the idea that strict adherence to the diet (often by the whole family) exerts a major pressure on the child to modify it's behavior. This is accompanied by a great increase in the amount of attention paid to the child and has been seen as sufficient to explain the vast majority of positive results from uncontrolled trials[46]. Based on these issues Feingold has been attacked for not submitting his ideas for peer review in recognised journals[47], but instead appearing on TV, radio and in newspapers, and for writing a book aimed at the lay public[48]. It has further been suggested that the advocacy of a treatment in the absence of controlled trials may be unethical since treatments must be proven to be safe[49]. "The scientific approach to problems cannot be replaced by untested hypotheses..."[50]. An editorial in 'The Lancet' commented that "Believers in the scientific method felt challenged by the speed of public acceptance and by the lack of objective evidence"[51]. The general sceptical response seems to have been that 'further research is required', and this has been carried out via control and challenge studies. In the former the child is maintained for a given time (e.g. one month) on the KP diet, and for a similar time on a diet containing food additives. Behavior during this time is rated by parents and teachers who are unaware of the presence or absence of additives in the diet. In the latter, groups of children who appear to respond favourably to the KP diet are 'challenged' with (usually) doses of food colours. Their behavior is then rated by various psychological tests[52]. Clearly neither of these are immune to placebo effects since both influence the way that a child may be treated and (as noted with drug therapy) if the additives have some pharmacological effect then the treatment may not truly be blind.

Feingold has also considered the ethical aspects of the diet and has suggested that since food additives have not been proven to be safe then the diet involves stopping the ingestion of potentially injurious substances[53]. This is further justified by the (clinically based) suspicion that some children react adversely to substances present in food[54]. A number of Feingold's supporters have also emphasised the value of clinical results in informing and justifying treatment. For example Blouin has noted that "In the early stages of a topic both passive observational and clinical reports are necessary to provide direction for research"[55], and Rapp has asked scientists and academicians to "...realize that clinical observations can be of value, can be produced, and even if not explained can be valid"[56].

As to why Feingold did not publicise his ideas via recognised scientific channels, it has been claimed that he tried to do so but his papers were rejected[57], and Connors has suggested that Feingold (born in 1900) was 'a man in a hurry' who couldn't afford to wait for double blind clinical trials to support his ideas[58].

The issues raised here are closely akin to those described in earlier chapters as 'academic versus medical'. Whether or not these terms are exactly appropriate, it seems that in this case there existed a group of researchers advocating early action based on information gleaned from clinical studies and a second group apparently more concerned with the advance of 'true' knowledge than with treatment, basing their ideas on the results of double blind controlled studies (see also below). These controlled studies have not escaped criticism, mainly on the grounds that they aren't relevant to Feingold's hypothesis.

7.52 <u>Relevant Experiments?</u>

In his early works Feingold identifies 34 food colours, 1610 synthetic flavours and 1120 other chemicals intentionally added to foods, to say nothing of substances naturally present in foods[59]. He acknowledged that the chemical composition and critical level of causative agent(s) in food was unknown and that his elimination diet was to some extent arbitrary[60]. On the basis of this Feingold has been criticised for failing to list the specific active substances[61], and Sieben has noted that the diet eliminates so many things that even if it were successful one wouldn't know which additives were the guilty ones[62]. Comments such as these would appear to support the suggestion, made above, that for some the diet was seen (primarily) as a tool for knowledge advancement, whereas for Feingold and his supporters it was a means of treating hyperkinetic children. It is hard to see why failure to specify the exact substances involved should invalidate the diet as a means of treatment. It will be remembered that similar critical comments were applied to Barkla's ideas (see Chapter 3) on the grounds that he could not fully explain his results, but here as there, there is no logical reason why failure of explanation should infer failure of observation.

Most of the later controlled experiments (particularly challenge studies) attempted to overcome the problem of non specificity by concentrating on a standard dose of food colours in a 'challenge cookie'. As noted above, many of these studies produced results which were negative or equivocal, and supporters of the diet have attempted to explain these perceived failures in several ways. Firstly, concentration on food dyes ignores other relevant substances, secondly, that challenge cookies may contain active ingredients, thirdly, that doses of food colours are inadequate, fourthly, that dietary infractions occurred and fifthly, that the study population may in some cases be irrelevant to the hypothesis. On the first of these, Feingold noted early on that supposed tests of his ideas ignored the vast majority of substances which he believed to be important[63], and Rimland has characterised many studies as very nearly irrelevant[64]. This concentration on colours is based on their wide use, especially in foods consumed by children, and on the belief that Feingold has emphasised the role of colours[65], though Feingold denies this and states that his recommendation to concentrate on food colours was related to the complexity of research and not because of any belief that they are the only important factor[66]. The essence of this criticism is that many potentially active ingredients have been ignored in the majority of challenge studies. These potentially active ingredients include such commonplace substances as sugar. The KP diet restricts the intake of many sugary substances (because of their additive content) and some authors (though not Feingold) suggest that a high intake of carbohydrate is associated with hyperactivity[67]. If this is the case then the diet may be successful for reasons not associated with additives and furthermore a diet concentrating on food colours may not eliminate 'active' ingredients. This leads on to the second criticism, that the challenge cookies used were not inert. In many studies the dose of either food colour or placebo has been given via a chocolate cookie (chocolate is permitted on the KP diet). Clearly these cookies can only be genuine placebo's if they contain no substances likely to influence behavior. It has been noted that allergy to chocolate is relatively common[68], and Rippere notes that certain challenge studies have utilised subjects who may have been allergic to chocolate so that "... conclusions which rest upon the assumption that... 'control' conditions are inert must necessarily be invalid"[69].

The third area of possible doubt relates to the amount of food colour used in the various studies. This has varied from 1mg[70] to 150mg[71] with the most commonly used amount being 26mg made up of a mix of eight different colours and given in two 13mg doses each day[72]. This dose level was arrived at on the questionable basis of calculating the total amount of colours used annually in the USA and dividing this by the total population (adult and child) to give an average intake per person[73]. Later estimates criticised this calculation on the grounds that children are far more likely to consume coloured confections and soft drinks, and it was calculated that the average daily dose was 59mg for one to five year olds and 76mg for six to twelve year olds, with ten percent of children in each age group consuming more than 121 and 146mg respectively and the maximum consumption being over 300mg. On the basis of these figures the doses of colour used in many studies have been dismissed as 'ridiculously small' and Rimland asked "Could you be convinced that handguns were not lethal by studies using popguns to test the lethality hypothesis?"[74]. Two main replies have been made to this criticism, firstly, that food colour is not usually ingested in one or two bolus doses per day and hence effects could reasonably be expected with doses of less than the total daily intake, and secondly, that Feingold has claimed that even minor infractions cause obvious behavioral changes and thus the colour dose in the challenge cookie should be sufficient to give rise to changes in behavior[75]. In an apparently ad-hoc response to this Rimland suggests that children who have been on the KP diet for long periods of time may be able to withstand the effects of any additive better than those who ingest additives regularly[76]. This is in dispute with Feingold's observations with which Rimland is expressing agreement but it is noteworthy that high doses of colour (150mg) have been claimed to produce responses in the majority of children tested[77]. Furthermore, the reason for using a daily dose of 26mg was the belief that this was close to the daily intake of the average child and studies were presumably designed on the basis that this was the appropriate dose to use. If these beliefs were adhered to it seems that the logical response to the discovery that these doses do not mimic daily intakes would be to disallow the studies for inadequate dosage. Instead of this a low dose has been used to extrapolate 'no effects' at higher levels, rather at odds with the dose response assessment methods discussed in Chapter 4.

The possibility of dietary infractions has also been used to explain any lack of differences between the KP and control diet behaviors. Almost all studies report infractions and if these occur every two to three days then (according to Feingold) this is sufficient to explain the maintainence of HA behavior. In reply to this various authors have pointed out that if compliance cannot be achieved during periods of study then it may be equally difficult to ensure during day to day use, and if this is the case then the utility of the diet is open to doubt[78].

The final explanation for non supportive results is the suggestion that the test population is not relevant to the hypothesis. If a relatively small percentage of children are responders to the diet then any positive results may be lost when statistics of overall responses are calculated[79]. For example, Weiss calculates that "If only 30% of a sample are responders and they shift by an average of one standard deviation then the total sample average shifts only minutely"[80]. Challenge studies with claimed responders have been used in an attempt to overcome this (see above) but these may fall foul of the earlier criticisms relating to dose of colour, inertness of placebo, etc. A specific example of the use of a possibly inappropriate population may be a study by Mattes and Gittleman[81]. This involved eleven children considered by their parents to be good responders to the KP diet. Of these children five were diagnosed by the research team as being hyperactive but only three of these were hyperactive on teacher rating scores (one scored below the hyperactivity criteria and the other was not at school). During the study three of the participants did not complete the diet programme. These are not identified and so could be those who were most clearly hyperactive. The study concludes that "... artificial food colorings do not affect the behavior of school age children who are claimed to be sensitive to these agents"[82]. In a review of this study Trites and Tryphonas describe this conclusion as unjustified[83], and even if justified its relevance to the Feingold hypothesis is open to question[84]. This study is not discussed with the intention of categorising certain studies as 'good' or 'bad' but to indicate the ease with which results may be questioned. Whether or not questioning takes place seems to relate to any results and conclusions reached (as noted in Chapter 3). For example Rippere suggests that Connors (an 'opponent' of Feingold), minimised and discounted findings supporting these ideas whilst at the same time not applying similar criticisms to studies with findings running counter to them[85].

The next area of critical comment relates to the potential social and nutritional risks of using the diet.

7.53 Social and Nutritional Dangers of the Diet

Several writers have warned that the diet may create or increase family hostility to the child because of the time, effort and expense devoted to maintaining the child on the diet (especially if it is consumed by the whole family)[86]. Further concern has been expressed that the diet may isolate the child from its peers making it feel different because of the inability to join in with snacks, drinks, etc.[87] and that the child may believe that it cannot control its behavior without the diet so that eating a 'forbidden' food is an excuse to 'act up'[88]. It is noteworthy that remarkably similar comments were applied to drug treatment in its early years (see above).

The second area of comment relates to the possible nutritional dangers of the diet. Early writers suggested that restrictions in the intake of fruit and vegetables could lead to inadequate doses of certain nutrients and especially of vitamin C[89]. Actual measurements of nutrient intakes has produced a variety of results, ranging from improvements[90], to adequate though reduced intakes[91], to the finding that nutrient intakes were below recommended daily intakes (though this was probably associated with dietary preferences rather than the diet itself)[92]. Rippere contrasts these suggested risks which are (she says) easily overcome by vitamin supplements, with the known and potential risks of long term drug therapy which are far less easy to remedy[93].

The next question we are faced with is why has such an inconvenient and contentious treatment as the KP diet become so popular with parents?

7.6 Popularity of the Diet

Clearly the simplest explanation for the popularity of the diet, one that would be put for ward by its supporters, is that it is successful in reducing hyperactivity in a large number of cases. Naturally this reason would not be acceptable to opponents of the diet, who have instead sought explanation in some form of parental peculiarity. The most common of these relates interest in the diet to the growth of interest in ecology and 'natural foods' which make Feingold's ideas easy to accept[94], and others have categorised those interested in the diet as a 'back to nature' movement[95], interested in 'food fads'[96]. These responses bear a close resemblance to those noted in an earlier chapter with regard to fluoridation and at the Windscale Inquiry. Any failure to accept the received wisdom of some part of science is seen as explicable only in terms of some aberration on the part of those refusing acceptance. The similarity between these and earlier responses is further enhanced by a comment from Dr. Juliet Gray, a consultant nutritionist who said "In a climate of general discontent and anti-establishment feeling, it is not surprising that those responsable for our food supply- notably government and the food industry- should be open to attack"[97]. This suggestion is identical to that used in the fluoridation debate, namely that viewpoints at odds with the official view are due to alienation and a desire to strike at those in authority. Finally, those supporting Feingold's ideas have implicitly been accused of being anti-science. For Werry,

> "... the most chilling aspect of Feingolds work lies in the enthusiasm with which it has been embraced by the anti-medication, anti-psychiatry section of the American public and used as a cudgel to try and close down pediatric psycho-pharmacological research in that country"[98].

It is interesting to compare these ideas with those expressed earlier on the possible costs and inconveniences of the diet. Presumably if the diet is very inconvenient to manage then parents utilising it must perceive some major benefits for themselves or their child. Perhaps for them the diet works (for whatever reason) or perhaps they are interested in getting 'back to nature'. The essence of criticisms of use of the diet would seem to be that the issue is a purely scientific one and that only a scientifically validated diet should be considered, with any values and preferences of the parents or child playing a secondary role – the 'classic rationalist response' referred to in Chapter 5, with evaluative aspects of an issue being overshadowed and constrained by scientific aspects.

Before drawing any conclusions on the controversy it is worth briefly considering the use of additives in Britain.

7.7 Use of Additives and Policy in Britain

Over 200,000 tonnes of additives, costing £235m, were used in the UK in 1980. These ranged from vitamins to preservatives to food colours (9000 tonnes at a cost of £12m)[99]. The primary reasons for the use of additives are to aid the preservation and stability of foods and to facilitate food processing and treatment[100]. The use of colours has been increasingly questioned since these do not fit into any essential category. Those who believe additives may be harmful have proposed two possible courses of action, firstly, labelling and consumer information campaigns, and secondly removal of many additives from food (not only colours). The first of these was advocated by Feingold and has recently been adopted by the British Government (see below) and by some supermarket chains[101]. The second has been proposed on several grounds – that it would defuse any dispute[102], that it would be prudent since many additives serve no nutritional purpose[103] and based on a lack of knowledge of long term and combination (cocktail) effects[104]. Resistence to action has also been on two counts. Firstly, since many studies have not found any significant relationship between additives and hyperactivity action is not justified[105], and would be premature since "Public policy changes should only be made with the support of sound scientific evidence"[106]. Secondly, despite claims to the contrary, colours do perform a useful function, they help in the visual identification of foods, they provide information as to the quality and condition of foods and they replace colour lost during processing - broadly satisfying public expectations on the appearance of foods[107]. Other additives are deemed essential to provide food at reasonable cost throughout the year[108] and are said to be essential to processing, though there may be doubts about this in some cases (for example it was recently reported that certain additives have been removed from bread after years of insistence by the bakers that this was not possible[109]). It has been suggested that if colours are not added to foods this "... precipitates drastic consequences"[110] - presumably loss of sales[111], though these consequences are more likely to be drastic for the producer than for the consumer.

What has the response of the British Government been? By and large action has been general and designed as a framework for future action. For example the 1984 Food Act is an enabling act which talks in broad terms, stating that it is an offence to use as an ingredient in food any substance which renders that food injurious to health[112], (the act also contains certain defences for this). More specific information and advice has come from the Ministry of Agriculture, Fisheries and Food. In various reports they have prohibited the use of certain food additives in foods specifically designed for infants and young children[113], and though colours are not included in this prohibition there has been a voluntary reduction in their use[114]. The most recent government action has been to promote legislation such that, by 1 July 1986, food additives (except flavourings) in quantities sufficient to perform a technological function must be identified on product labels[115], and this legislation has been accompanied by government and other publications to inform consumers about additives[116]. Despite these moves pressure group attempts to achieve a ban on additives seem to be growing, for example via national petitions[117] and certainly public interest in the use of additives remains high, as witnessed by recent television programmes and newspaper articles[118].

7.8 Discussion

The controversy has all the hallmarks of a clash of 'scientific cultures' between two groups with differing methodological standards. On the one side Feingold and his supporters accepted anecdotal, clinical and non-blind observational findings used primarily as a basis for treatment. These tended to produce results supportive of the diet. On the other side were a group of more academic researchers for whom the only acceptable evidence was carefully controlled, standardised and double blind, and these tended to produce negative results. Whether or not these differences reflect scientific beliefs or extra-scientific rationalisations is a matter for further research, but the two groups appear to differ in their methodologies, in the degree of evidence needed to support any hypothesis and whether or not this justified action. Each group criticised findings not supportive of their views, often for ad-hoc reasons. For example, many studies used a dose of colour of 26mg based on the belief that this approximated daily intakes. When it was shown that intakes of colour could be far above this, the experiments were not dismissed as irrelevant, but instead explanations were put forward to show that this intake would have been enough to produce effects. Supporters of dietary effects used similarly ad-hoc explanations to explain negative results. These findings tally with the ideas considered in Chapter

3 - experiments are judged as good or bad, valid or invalid, by the extent to which their results accord with pre-existent conclusions, so that the experiment cannot be the final arbiter of what is accepted as knowledge.

It is interesting to briefly compare the comments applied to the use of drugs in hyperactivity and those applied to use of the KP diet. Drug therapy was initially studied clinically, had problems with placebo effects and with experimental control due to the many potentially relevant variables and is prone to social, psychological and physical side effects. The diet has been criticised in more or less identical language, and whilst it is clearly invalid to say that since both have been criticised in similar terms and drug therapy is now accepted then the diet should be also, it is valid to try and explain this difference in acceptability. Perhaps the simplest answer is that drugs are perceived to work, but at the same time these also accord with desires for a 'technical fix' for problems whilst the diet accords with opposite beliefs in a non technical solution. Opponents of the diet explained its acceptance by parents in 'rationalist' terms. Those accepting the diet were keen to go 'back to nature', were suffering from 'anti-establishment feelings', and in some cases were attacking scientific research, i.e. acceptance of the diet was seen in terms of some peculiarity or pathology whereas rejection or non consideration was seen as a rational response. Policy action was also seen in these terms, policy changes were only justifiable with sound scientific support. Since the evidence for any effects was seen as dubious no action should be taken. Feingold and his supporters took a different view based on the belief that a possible risk existed and thus removal of some additives was justified, especially those such as colours which have no direct nutritional function. These different views are

are explicable in terms of error cost (see Chapter 4), with one group being orientated towards knowledge and regarding the most costly error as being the acceptance of a dubious 'truth' whilst the other group is more orientated towards action with the most costly error being inaction in the face of risk.

The main influence of science on policy in Britain seems to have been to put the issue on the political agenda, in part by direct means and in part via the activities of pressure groups such as the Hyperactive Childrens Support Group and the Consumers Association[119]. The government response has been an incremental one, a balancing of the partisan pressures of those advocating action and those advocating further research. Labelling of foodstuffs accomodates all interested parties to some degree, none got exactly what they wanted but at the same time none was entirely disappointed. A further advantage of this strategy is that it allows the application of further partisan pressure as parents choose (or not) to purchase additive free foods thus allowing them to send messages to producers in a way which either a ban or no action could not.

How does this model fit in with the idea of over and under-critical relationships between decision making and science? Prior to the early 1970's most toxicological standards were set by discussions between the food industry, toxicologists and government. These were fairly low key affairs and tie in well with the under critical model. In the 1970's Feingold's ideas stimulated debate and eventually a scientific controversy on both sides of the Atlantic. This controversy involved not only experimental results and the interpretation of these, but also the methodologies by which the results were produced. This scientific battle has spread to the policy arena where pressure groups and manufacturers organisations have pushed their particular cases for and against additives. Clearly the policy debate would not have taken place in the absence of scientific debate, and this suggests that the situation may be explicable in terms of the positive feedback overcritical model. Feingold's work had policy implications which encouraged the food industry to fund research in an area with an enormous number of potentially relevant variables. Thus research was stimulated by interests outside science and was carried out by several disciplines, with all of the problems of loss of autonomy, increased visibility, multi-disciplinarity and increased level of criticism which are the hallmarks of the overcritical model. These supported the development of a variety of scientific interpretations and policy positions and have aided the maintainence of dispute in both the scientific and policy arenas.

Hence, in this case, as in those discussed earlier, science was able to raise an issue, to help place it on the political agenda, but it could do little to answer many of the questions raised and actual policy could only be settled by incremental actions and by partisan mutual adjustment.

P A R T III

CHAPTER 8

CONCLUSIONS

8. CONCLUSIONS

8.1 Introduction

In the introduction to this thesis three questions were asked. Firstly, are controversies in sciences related to policy aberrant and atypical processes, or are they merely more visible examples of what happens in science at all times? Secondly, if controversies are 'normal' then what implications does this hold for decision models requiring unproblematic information?, and thirdly, what role does scientific information play in decision making? The discussions in Chapters 2 - 4, and the case studies in Chapters 5 - 7 allow some answers to be given to these questions. Firstly however, I would like to offer a brief comment on the case studies. To the sociologist of science these may seem to be lacking in specific detail, but their aim is not to pinpoint the highly individual and specific factors driving each controversy but to relate these to decision making. Thus, the 'broad brush' approach has been adopted to draw out the major factors pertaining both to the controversies and to the policies pursued. Certainly future work could fruitfully deal in detail with the specific social, political and technical aspects of each dispute but for the moment I am more interested in setting up a framework within which this study can take place.

8.2 Scientific Controversies in the Policy Arena

In Chapter 3 research in pure science was considered and it was suggested that decisions as to what is to be regarded as a fact are heavily influenced by social consensus rather than simply being based on reference to some external reality. Furthermore, judgement as to the validity or otherwise of an experiment is largely based on factors which are not directly related to that experiment. If 'facts' are based on consensus rather than external reality then there can be no ultimate arbiter of scientific truth and all science is, in principle, controversial. The factors influencing the acceptance of scientific ideas were the acceptance of the authority of one discipline or group over another to define experimental methodologies, results and conclusions, the decision to maintain prior agreements on the interpretations of earlier experiments such that only a limited number of explanations may be invoked, and related to these the role of interests and skills so that certain explanations are favoured because they support the importance of those skills. These factors are far more likely to operate rapidly and successfully where a 'shared culture' exists, such that there is broad agreement on experimental practices, expected results, interpretations of data, and so on. Much of this shared culture is based on tacit knowledge and hence is not open to inspection or discussion. In pure science, research problems are usually chosen internally, they are 'do-able' based on current ideas, with current equipment- in Kuhnian terms they are puzzles with solutions. Things are rather different for science related to policy. Firstly, knowledge wants tend to cross disciplinary boundaries- problems are multidisciplinary. Thus, in these areas there is no common tacit knowledge, no unitary shared culture, on which to base agreed experimental practices. This cognitive differentiation leads to a situation (which has been discussed above), where experimenters can readily find causes for disquiet in the practices and results of other disciplines . Secondly, the decision to carry out research, and the direction of that research is influenced by political considerations. Thus, scientific autonomy is lost and research may be pushed into areas where no obvious solutions or methods exist. Thirdly, politically relevant results may be highlighted prematurely as significant, and since these results have policy implications extra motivation exists to criticise them. All of these mean that controversies are more likely to develop in sciences related to policy, but for reasons that are intrinsic to scientific practice and are therefore not in any sense correctable. Not only are controversies more likely to develop in these areas but the closure mechanisms which aid the achievement of consensus in uni-disciplinary pure sciences are less likely to operate. As noted above, these mechanisms include authority, maintainence of prior agreements and shared interests. These are unlikely to operate in novel areas with several disciplines since here, not only are there no prior agreements but also each discipline is motivated to support its interests, skills and methodologies and to criticise those of opponents.

The case studies support these contentions. Each study is multidisciplinary in nature, as exemplified by the maternal deprivation controversy where separate training, professional societies, skills and so on not only reduced the possibility of closure but actively promoted disagreement as each discipline supported its own knowledge and methodologies at the expense of others. The hyperactivity example shows very neatly the failure of experiment to decide 'facts' with experiments being judged by the results that they achieve, a 'good' experiment supports previously arrived at ideas and a bad one does not.

One interesting area is the postulated difference between research for action and research for knowledge, which gives rise to different criteria on whether or not results justify action. Further study is required to ascertain the extent to which conduct in controversies is influenced by these beliefs- are they merely rhetorical devices employed for political or other reasons?, or are they the result of genuine 'cultural' and occupational differences between medical and academic disciplines? It is noteworthy that there is a close parallel between 'medical' beliefs and incrementalism, and between 'academic' beliefs and the synoptic model. The former base action on indications of possible problems and on 'what we know' rather than on 'what we would like to know', whilst for the latter acceptance of knowledge or policy is based on large amounts of research with the aim that what has been decided should stand for a lengthy period of time. The only changes considered tend to be major ones, acceptance of a new theory or policy, hence a great deal of certainty is required before these can be supported. The incrementalist on the other hand looks for opportunities for minor changes and small actions where the error cost of a wrong decision is small and thus it is possible to take opportunities for perceived improvements when these arise. In the next section I will consider these decision models in more detail.

8.3 Decision Making Models

In Chapter 2 it was noted that for decisions to be taken in a synoptic manner large amounts of certified information is required, whilst incrementalism, via the mechanism of partisan mutual adjustment, recognises that decisions are essentially political. This recognition means that incrementalism can function with either uncertain information or in the absence of information. Clearly, in the case studies, no decisions could have been taken if the synoptic model had been utilised, all that decision makers could have done was to delay any decision in the hope of future agreement. Furthermore, as noted above, any knowledge claim is open to challenge so any decision model depending on certain, guaranteed and unchanging information must fail. Of the decision models discussed only incrementalism can cope with this view of knowledge, firstly since it does not depend on scientific information to make decisions, and secondly, because decisions are made in increments and thus it is easy to modify or reverse the direction of policy if values change and research accumulates or is interpreted differently. Do the case studies support the idea that policy is made incrementally? Certainly the two 'in-depth' examples support the model. In the maternal deprivation study policy was made in a remedial, fragmented and incremental manner with ends adjusted to means. Information was used as a political tool primarily to legitimate rather than to inform policy. In the hyperactivity example policy was based on a balancing of pressures from different partisans and again information played a legitimating role. In many of the case studies in Chapter 5 information was used in a similar manner, that is to legitimate policies arrived at in a partisan manner, and at times, to support the pretense that these views were 'rational'- serving a 'fig leaf' function. Again this would be expected, based on the incremental model, with partisans pushing their viewpoints using a variety of means including information. In the two examples where a 'rational' view was apparently attempted (the Windscale Inquiry and the fluoridation debate), it is interesting to note that the discussions ignored the arguments considered to be important by the opponents of the policies. This highlights one of the major drawbacks of even paying lip service to any sort of synoptic rational model - values are downgraded in importance, they are seen as secondary to 'factual' inputs. As this thesis has shown, political disputes are in large part due to value differences, they cannot be adequately dealt with by consideration of facts alone. Of course the basis of this argument is also evaluative, for example it assumes that some form of democracy and thorough consideration of issues is a worthwhile goal, but as these views are likely to be supported by the majority of individuals in Western democracies, as implicit values they would seem to be fairly innocuous.

It has been suggested in the above section that scientific information is used to legitimate political views. This and its other functions will be further considered below.

8.4 The Role of Science in Decision Making

One of the major functions of science in policy areas is, clearly, to raise issues and to identify possible areas of action. Scientific research fulfilled this role in all of the case studies considered. To reach the stage where an issue is discussed it must get onto the political agenda, and it is here that science comes into its own, in raising issues and in acting as a support and a legitimating device for partisans aiming to place a particular issue on the agenda. Once an issue is on the agenda science may continue to play a legitimating role, supporting political viewpoints and in turn being supported by them. This is the essence of the over critical model, where scientific research which is deemed relevant to a certain political view is taken up and injected into the political debate. This provides scientific findings with increased (and often untoward) visibility and almost inevitably gives rise to critical comment from those with opposing political viewpoints, and at the same time encourages them to support scientific views and research which runs counter to the original findings. This provides a further (external) impetus for more research in the original area accompanied by criticism of differing ideas. The cycle continues and scientific findings are continually subject to increased visibility and publicity, decreased autonomy and increased and often unhelpful criticism, which in its own turn supports further political dispute. Conversely, political consensus provides little or no support for research, there is no 'need to know' and so scientific findings are regarded as more or less irrelevant, they are not subject to testing and scrutiny- they are undercriticised. Though this thesis supports the idea of scientific activity as the invention of models and descriptions of nature, rather than the discovery of truth, it does not go against these ideas to also support the belief that the best way for science to 'advance' is for a certain level of criticism to take place so that models are neither too easily accepted nor demolished. (This advance need not be towards any ultimate goal, but could for example, mean the building of more powerful models). Science involved in policy tends to receive either too little or too much criticism. Of course there are times when there is consensus in science (or policy) and dispute in policy (or science) but it seems likely that this is a temporary situation and depending on the political, social and scientific circumstances criticism will either develop or cease in both spheres.

An interesting possibility is that appeals to 'rationality' may actually be counter productive for science. If an issue is presented as primarily scientific, with values as secondary, then those with differing values, if they argue on the basis of values are likely to come off second best in any argu ment. Their views are likely to be passed off as due to 'irrationality', 'anti-science', alienation and so on (as was found in several of the case studies). This provides an impetus for the transformation of evaluative disputes into scientific ones, encouraging extensive criticism of scientific findings. This could be avoided, or at least reduced, if it were recognised that policy making is not a case of rationality versus irrationality, truth versus error, but of balancing partisan pressures, with partisan positions arrived at by a mixture of fact and value and having as their basis values which are in no way inferior to facts. Whether or not recognition of this would make decisions in some sense better is open to debate, but at the very least decisions could be taken based upon the beliefs and values which are the real basis of disagreement rather than by reference to some 'fig leaf' of facts which are only of peripheral relevance to the real areas of concern.

8.5 Discussion

The above discussions and the case studies leave us with a very different view of scientific activity from the popular view of the objective gathering of neutral facts. Science is revealed as a social activity of creation not discovery. The postulated under and over critical models suggest that for scientific activity to be at its most successful an optimum level of criticism is required and that deviation from this optimum is especially likely in science relevant to policy decisions.

The consideration of decision models, and the case studies, reveal decision making to be mainly 'political' in nature, made by a series of bargains and negotiations between interested partisans, and with the role of science being far more peripheral than adherents to the synoptic model would suggest. It might be said that this thesis has merely illustrated the commonplace, that political decisions are made politically, but even if this were the case it is at odds with the synoptic view of the 'objective scientific decision'. More importantly the thesis has shown that one of the major roles of science (as well as raising issues), is to mask values and transform what are essentially evaluative disputes into purportedly factual ones, which can only be to the detriment of both science and policy making.

The view of science and policy presented in this thesis has obvious implications for the whole of science and scientific practice but its impact on day to day life is perhaps most germane to the current discussions. Arguably, acceptance of the ideas expressed herein would create major uncertainty in decision makers (and the public) many of whom are accustomed to turning to science for certain answers to policy problems. However, it has been suggested above that this referral to science merely masks uncertainties and it could be said that the acceptance of the view of science put forward here would not create uncertainty but merely unmask it and place it in its true place- the evaluative/ political area, which could only be of benefit to policy and decision making.

Finally, based on the ideas considered in this thesis, there are several areas where future study could be very fruitful. These include further testing and refinement of the under and over critical models, and the study of the role of science in other places where science enters the public arena, for example in the teaching of science. This seems to be based on an algorithmic model of research, where nature comes pre-sorted into objective and clearly defined boxes. It would be hardly surprising, on this view, to find disillusionment amongst non scientists when scientists disagree or science fails to deliver the goods, and prospective scientists must find their early brushes with 'real science' equally traumatic. Teaching methods which brought children into contact with the creative and consensual nature of science could change expectations with regard to what science can and cannot do and should and should not do. This in turn would inevitably influence the views held of the role of science in decision making and would aid the process of redirecting uncertainty in policy and decision making.

N O T E S   A N D
R E F E R E N C E S

1. F.Bacon, "Knowledge itself is power". (Nam et ipsa scientia potestastos est). Meditationes Sacrae De Haerestibus, 1597. Oxford Dictionary of Quotations, OUP, 1979, p.28.

2. See for example D.Collingridge and J.Douglas, "Three Models of Policy Making: Expert Advice in the Control of Environmental Lead", <u>Social Studies of Science</u>, <u>14</u>, 1984, pp.343-370.
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CHAPTER 2

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