THE ROLE OF EXPERTS IN POLICY MAKING

COLIN PAUL REEVE

Thesis submitted in partial fulfilment
of the requirements for the degree of
Doctor of Philosophy.

THE UNIVERSITY OF ASTON IN BIRMINGHAM

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As public policy issues increasingly have a technical aspect to them an interactive relationship has developed between science and policy. The aim of this thesis is to investigate the two aspects of this relationship: the influence of science on policy and the influence of policy implications on science.

Most existing studies in this area treat only one or other of these aspects. Furthermore, they tend to provide interesting case study material but very little theoretical analysis. This thesis attempts to overcome these problems by dealing with both aspects of the interaction between science and policy and by providing theoretical models of this relationship.

The thesis combines the theoretical development of these models with the analysis of three empirical case studies: the controversy in Britain over smoking and health; the application of educational psychology to the development of education policy in Britain; the controversy over the health effect of lead in the environment.

The theoretical models are developed in Part 1. In Part 2 the empirical case studies are presented and in Part 3 the theoretical material is assessed in the light of these case studies.

The main thesis of this study is that there is a fundamental mis-match between science and policy-making. Criticism is always essential in science. However, when science is involved in the policy process, either scientific claims are not subjected to a significant level of criticism or they are scrutinized so closely that no view achieves general consensus and conflicting advice results. In this situation, contrary to the traditional view, science can generate uncertainty. The role which science plays in the policy process is influenced by this level of criticism, by the context of political power and by the progress of an issue through the various stages of the policy process.

Experts, Policy, Smoking, Lead, IQ.

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It should be noted that Chapters 2 and 6 of this thesis represent summaries of work done previously for my MSc. thesis and are included as essential contextual information for the present thesis.

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1. INTRODUCTION

Throughout the century, and more especially in the period following the second world war, science in the industrialised nations has been brought into a close relationship with public policy-making. As public policy issues increasingly have a technical aspect to them scientists have been called upon, or have felt it necessary, to present advice to government and other decision-makers. This has led to the development of an interactive relationship between science and policy. They interact in such a way that policy implications influence the progress of science and scientific debate, and science provides an input into the policy-making process. The aim of this thesis is to investigate these two aspects of the interaction between science and policy. It is therefore concerned with what might be called 'science in policy' and not with the more straightforward phenomenon of 'science policy' through which science is supported and, to some extent, directed.

There are large bodies of literature concerning controversies over policy issues with a technical component and the role of experts in policy-making. Most studies within these bodies of literature treat only one or other of the two aspects of the interaction between policy and science. They therefore miss the point that the way that policy implications influence the progress of a debate amongst scientific experts will have an effect on the role which those experts and their scientific views have in the policy-making process. The converse of this point is also true; the role which scientific views play in the policy-making process will influence the progress of the scientific debate. It is important therefore to look at the processes of influence of science on policy and of policy on science together.

Another problem with the existing literature on expert advisors and technical controversies is that it has provided a wide range of interesting case studies but very little in the way of theoretical analysis of these studies. There are some notable exceptions to this claim and these will be discussed in the next chapter. Furthermore, while very few authors would admit to holding a traditional positivist view of science, some studies in the literature on controversies and the role of experts implicitly assume such a view. Finally, there seem to be a number of misconceptions about the nature of policy related technical controversies which stem from this implicit assumption of a naive view of science. For example, a whole range of characteristics common to controversies seem to be attributed to the effects of policy implications or political bias. Controversies are seen as abnormal occurrences, different in kind from the normal processes of science. A theme which runs through the whole argument of the present thesis is that many of the characteristics of controversies stem from the nature of science itself as well as from its interaction with policy. Furthermore, many of these characteristics are different from the normal functioning of science only in extent.

To investigate the interaction between science and policy some theoretical concepts and models are developed and employed in this thesis, along with some existing [illegible]-

theoretical tools. These theoretical developments concern; the nature of controversies and the relation between this and the policy implications of scientific work; the occurrence of criticism within science and the achievement of consensus; the influence of policy implic-actions on levels of criticism and the ability to achieve consensus; the role of science in policy-making and factors influencing that role.

The method which this study employs is that of theoretical development combined with a case study approach. The main problem with a case study methodology is the question of the extent to which conclusions drawn from particular studies can be generalised. However, case studies can provide a wealth of detail with which theoretical developments can be assessed, at least for their plausibility if not for their general applicability. It is hoped that by having a small number of case studies some of the problems of general applicability will be overcome without losing altogether the insight which is provided by the detailed study of a particular example.

The three case studies investigated are the controversy in Britain over smoking and health, the application of educational psychology to the development of education policy in Britain, and the controversy over the health effects of lead in the environment. These three were chosen because they involve different fields of policy and types of scientific research. It is hoped that their diversity will demonstrate the strength of the theoretical position developed in this thesis. They were also chosen because superficially they seem to demonstrate in a straightforward way three different aspects of the relationship between science and policy. At first glance the smoking and health issue seems to be a scientific controversy which simply resolved. The education case seems to illustrate the straightforward application of science to policy initially without any controversy. The lead case seems to be a situation where controversy has not resolved. It will be seen in Part 2 that these case studies are not as straightforward as they at first appeared and some of the conclusions to which they, combined with the theoretical developments, lead are quite contrary to what might be expected.

The format of this thesis will be to develop and discuss in Part 1 some theoretical models and concepts in the context of various relevant literatures. These will include literature on experts and controversies, literature from the sociology of science, the philosophy of science and policy studies. In Part 2 the empirical case studies will be presented. Finally in Part 3, using the case studies, the theoretical material presented in Part 1 will be assessed and conclusions will be drawn on a number of questions concerning the interaction between science and policy.

Among the general questions to be considered are the following:

a) How do policy implications influence the science presented as expert advice?

b) What role does science and do scientific experts play in the policy process?

c) What influences determine this role?

More detailed questions will be addressed when considering the empirical material but these will be introduced in the theoretical discussion to follow.

The main thesis of this study will be that there is a fundamental mis-match between science and policy-making. Criticism is essential in science to ensure that the best theories and experimental results are employed and that knowledge continues to grow. Criticism of scientific claims made in the context of public policy-making is also essential to ensure that the reliability of claims is adequately assessed. However, the way in which science has developed, combined with the character of public-policy problems, means that either scientific claims are not subjected to a satisfactory level of critical scrutiny or they are scrutinised so closely that no view achieves general acceptance. The role which science plays in the policy process will be influenced by this level of criticism and by the broader context of political power. This role will also change as various other factors change and as an issue moves through the policy process.

Finally, science can create uncertainty in areas including broad policy questions through the chaos caused by a high level of critism. It might also lead to errors in policy-making through the uncritical acceptance of scientific claims of dubious reliability. However, scientific consensus will usually be accompanied by a consensus on policy and, in this case, science has very little role in influencing policy-formation. Thus, in the context of policy-making, it may be dangerous to accept scientific views which have not been subjected to critical scrutiny, but critical scrutiny will produce a chaotic situation of conflicting advice. The policy-maker must therefore accept that policy must be formed in the absence of definitive answers from scientific experts.

PART 1

CHAPTER 2

DISAGREEMENT AMONGST EXPERTS

ADVISING ON POLICY

2. DISAGREEMENT AMONGST EXPERTS ADVISING ON POLICY.

2.1 The Content and Context of Debates Amongst Experts.

The literature on controversies and the role of experts presents a host of case studies analysed from a variety of perspectives. This has led to a range of questions being introduced and a similar range of approaches to these questions being employed. In an earlier review of some of this literature Reeve[1] attempted to draw out some issues of analytical interest to help bring together the great diversity of approaches to this topic. It will be necessary to reiterate some of the points made in that review here so that the current study can be seen in the context of the findings from other analyses of controversies.

In the earlier review a distinction was made between the explicit nature of the debate among experts and the motivation driving it. In the current study I shall refer to the former as the 'content' of a debate and to the latter as the 'context'. Generally, in the controversies literature these two elements are not clearly distinguished. This leads to confusion over what controversies are 'about'. Some authors argue that they are about facts, some that they are about values, and some that they are about a mixture of facts and values. Other authors argue that the distinction between facts and values is not a useful one in this context. The distinction between content and context makes it possible to study the interaction of 'facts' and 'values' and the role of science in controversies. The distinction shows that the character of the explicit public debate is different from the rival motivations driving the debate, while these motivations determine the positions taken by participants in the debate.

The first problem to look at is the content of disputes, that is, their explicit nature. To discuss this issue it will be necessary to clarify my usage of the term 'fact'. Within the controversies literature the terms 'facts' and 'values' are used widely. It has, however, been argued that the fact/value distinction is of little use for analysing decisions about technology or roles of the advisors.[2] This argument stems from an interest primarily in the sociological question of motivation for disputes without clearly separating this question from the one concerning the nature of disputes. The concept of a 'scientific fact' is problematic and different philosophical views of science give different accounts of the nature of 'facts'. Developments in the philosophy and sociology of science from Popper's work onwards have led to the rejection of the posivitist notion of facts as 'unproblematic reflections of reality'. Facts may be seen as theory laden, value laden, socially constructed or ideologically constructed, depending on one's philosophical view of science.

In what follows, the terms 'fact' or 'factual' will be used to refer to propositions which are normally considered as within the realms of science. More especially they will be applied to the products and processes of acquiring and interpreting data. In this usage the term 'factual' could almost be replaced by the term 'scientific'. I am not, therefore, at this stage making any claim about the epistomological status of 'facts'. All I wish to denote by that word is something which is usually accepted as being a part of science. It will be part of the aim of this study to illustrate the way in which these 'facts' are influenced by elements of the context of debates. Finally, where the terms 'facts' and 'values' are used in the context of an account of another author's views, then their meaning is intended to be the same usually unspecified meaning as in the original author's work.

Nowotny and Hirsch[3] have pointed out that the issue of the nature of the dispute may, itself, be a matter of hot debate amongst the disputants. For example, in the debate over the effects of low level radiation doses, a strategy of the orthodox scientists involved, who maintained that low level radiation was not harmful, was to claim that the issue raised by the dissident scientists was not a scientific one in an attempt to remove the whole issue and the dissident scientists from the field of scientific legitimacy. Various writers have argued that expert disagreement arises through the intimate intermixing of facts and values so that, when experts disagree, they are really disagreeing about values. These values are either so intermixed with the scientific issues that they are inseparable, or they could be separated from the factual issues but are not because of the specific context of the debate.[4]

For a number of other authors, disputes are about facts and values. These authors at least allow disagreement to occur on scientific grounds, but also see disagreement as being about values. Petersen and Markle[5], in their study of the Laetrile controversy, argue that the controversy began with a knowledge focus, that is, was initially a debate about facts, but, as the dispute went on, it moved into a value arena. Again, the nature of the controversy was a matter of dispute itself.

The analysis of the debate over the effects of low level radiation by Nowotny and Hirsch[6] seems to suggest that the explicit dispute involved both factual and value issues and Weinberg's notion of 'trans-science'[7] also allows disagreement to be about facts or values.

Although in much of the literature on expert disagreement the emphasis is on the political nature of the disputes, or the conflict of values, this analysis really applies to the motivation or driving force behind disputes. In many of the case studies analysed the explicit debate between experts is primarily, if not solely, factual. Furthermore, this factual debate tends to centre around the interpretation of the data, rather than on the actual data itself. Though there can be debate over data itself, such as in the Laetrile controversy where accusations were made that certain data existed but was being withheld, there may also be accusations of manipulated data, or criticisms of experimental technique calling data into question.

Nelkin's[8] primary interest is with the politicisation of expertise, but the case studies which she presents demonstrate the factual nature of the explicit part of the disputes. In the disputes over a proposed nuclear power plant at Cayuga Lake, New York, and a proposed new runway at Boston Airport, the technical debates were characterised by "...genuine uncertainties that allowed divergent predictions from available data",[9] and diverse premises requiring different sampling intervals and techniques. For example, in the Cayuga Lake dispute[10], where the main technical issue concerned thermal pollution, there were disagreements about the need to study thermal pollution as from a point source or by taking the ecology of the whole lake into account. In this case, the technical dispute was about the interpretation of data. With diverse theoretical positions, different scientists interpreted the sets of data in different ways. For those scientists in the Engineering College at Cornell, the data from the study sponsored by the power company was not of any significance since it only concentrated on information about effects at the source of thermal pollution, whereas, from their theoretical perspective, data was needed on the ecology of the whole lake. According to Nelkin, the crucial aspects of the technical uncertainties involved were the absence of conclusive data and the lack of an accepted theoretical framework from which to draw definitive quantitative conclusions. Nelkin emphasises that the technical debate involved considerable rhetorical licence, with accusations of bias and premature interpretation of data. This type of rhetoric is not generally associated with scientific discussions and Nelkin's emphasis on the rhetoric used suggests that it is an aspect unique to controversial decisions relating to public policy. However, although rhetoric may not appear in the science journals, that it figures significantly in scientific discussions has been illustrated by I.I.Mitroff in his study of the Apollo moon scientists.[11] Although Nelkin's interest is with the politicisation of expertise and, in her analysis of the Cayuga Lake controversy[12], she suggests the various roles which scientists play in controversies, including that of the political activist, the actual technical debate is clearly about scientific facts, that is, the collection and interpretation of data. The factual nature of the content of technical debates is borne out in a collection of case studies which Nelkin introduces[13]. In her introduction to this collection she, herself, makes a distinction between the content of the debates, which she argues is factual, and the motivation driving the debate which stems from political values in her view.

Mazur[14] is similarly interested in the effect of political commitments on scientific controversies, but again his analysis of the actual disputes associated with the Anti-Ballistic Missile and the effects of low level radiation reveal the factual nature of the disagreement. Mazur also emphasises the use of rhetoric in disputes, comparing the fluoridation debate with that concerning nuclear power he finds similar types of rhetoric used. He goes on to lay out a number of characteristics of the technical issues and of the disputes themselves that form the basis of the disagreements. One group of factors contributing to the disagreement Mazur sees as being unnecessary. For example, in both these case studies there are examples of scientists failing to confront each other's arguments and effectively talking past each other.

In the low level radiation case, one group of scientists produce data and argue on the basis of the permissible level of radiation, whereas another group reject the interpretations and data of the first group, arguing instead in terms of the actual exposure level. Yet this distinction is not highlighted so that, when the two arguments are used in confrontation with each other, different conclusions are drawn without it being acknowledged that the arguments refer to different concepts. Other problems of this nature include arguing from different premises and approaching different problems. According to Mazur, these are really problems of poor communication and/or strong motivations to win and "..a calm analysis of opposing views could clear this sort of verbal thicket".[15] There are, however, more fundamental forms of technical disagreement which are not so simple to clear up. Mazur calls these 'ambiguities'. Science involves judgement, in employing simplifying assumptions, in extrapolating from inconclusive data, or in assessing the reliability of empirical data, for example. It is in making these judgements, without formalised guides, that scientists may legitimately disagree. An example from one of Mazur's case studies concerns the relationship between the radiation dose delivered to a population and the resultant increase in leukaemia. Given the inconclusive nature of the data, it is possible to postulate several different models of this relationship, two that are employed are the threshold model and the linear model. Mazur sees this as an example of theoretical ambiguity, the problem is that there is always an element of judgement in selecting one model over another empirically consistent alternative. The interpretation of data is a rich source of disagreement since alternative interpretations are always possible, particularly in the case of statistical data and especially where there is substantial error variance. Scientists in disputes may employ alternative interpretations of data, or simply reject discrepant data as not valid.

Ravetz[16] has similarly pointed out some of the technical limitations which form the basis of disagreements. He is concerned, however, to highlight the differences between the traditional view of science and the reality of decision-making concerning large technological innovations. He contrasts the uncertainty of technological decisions with the supposed certainty of science, and presents a number of problems which face the scientists trying to analyse technical issues involved in these decisions which may lead to disagreement. One aspect is the weakness of the sciences involved in the assessment of the impact of technological innovations. These sciences tend to be poorly developed disciplines in which scientists can be "...in radical disagreement over the most apparently elementary matters of fact".[17] Ravetz points out that in these situations of technical uncertainty different theories proposed may call for quite different sorts of data, which poses problems for comparing the different theories. Another problem is what Ravetz calls the 'management of exactness', that is, how scientists cope with error. The main problem here is that experts are expected to produce 'hard facts' best expressed as precise numbers, and politicians and the public are not likely to be aware of the inexactitude attending all quantitative statements. But, from the point of view of expert disagreement, the problem may arise that experts disagree on the exactness of specific pieces of data and, thus, on their interpretation. This problem may be most acute in statements of probability, where the levels of 'significance' are a matter of human judgement and are not given by the data itself. The main problems analysed by Ravetz then, are that inexactitude and insecurity may lead to disagreement.

Kopp[18] is another writer whose interest is in the 'roots'of scientific debates, viewed from a sociological perspective, which she identifies as being a constellation of interrelated factors, disciplinary, institutional and political interests. But, again, the actual dispute which she analyses, the American debate over fallout hazards, is about the interpretation of data. The debate centres around the acceptance of conclusions based on the extrapolation of data. For example, Sturtevant, a Professor of genetics at the California Institute of Technology, who disagreed with the official Atomic Energy Commission(A.E.C) position on fallout hazards, believed that geneticists could, and should, extrapolate from data on flies and mice to estimate genetic effects in humans from radiation. The A.E.C.position, however, suggested that, since genetic effects of low-level radiation on humans were not known directly, it would be premature to try to estimate how many individuals were likely to be affected from fallout. So estimating genetic effects on humans by extrapolation from data on flies and mice would be unscientific. Another element in the debate, highlighted by Kopp, demonstrates its factual nature. This was the argument presented in a paper by E.B.Lewis and the replies to this by A.Brues. Lewis argued that there appeared to be a linear relation between the incidence of leukaemia in man and dose of radiation. This argument was on the basis of existing data and a theoretical explanation was provided for the relation. Brues questioned both the empirical evidence, the data and its interpretation, and the theoretical basis for the relation.

Gillespie, et.al.[19] similarly approach the problem of technical decisions and expert disagreement from a sociological perspective. In their analysis of the different decisions over the carcinogenicity of the chemicals Aldrin and Dieldrin used in pesticides in Britain and the U.S.A., the positions taken by the scientific advisors are technical ones relating to the carcinogenic hazard from Aldrin/Dieldrin and they stem from alternative interpretations of the same data. The British scientists reviewed the same data as the U.S. scientists, who had already decided that Aldrin/Dieldrin were carcinogenic, and came to the opposite conclusion. This difference was possible because of the limitations of toxicology leading to uncertainty in determining carcinogenic risk, and the use of different criteria for the assessment of what is carcinogenic.

In the literature on controversies over decisions concerning technology there are a number of interpretations of the nature of the technical debates. As we have seen, it is possible to argue that the actual explicit nature of technical debates, or their content, is factual, involving primarily disagreement over the interpretation of data, but also involving disputes about the validity of specific pieces of data. The disagreement over interpretation of data can stem from the adoption of different theoretical positions, from the use of alternative basic concepts and from the analysis of different problems. Although a lot of authors place emphasis on the political or value dimension of disputes, the case studies reveal that most disputes are about factual issues. Those who argue that scientific disputes are about values, or a mixture of values and facts, do not really separate the questions of the explicit nature or content of the debate and the motivation driving it. Generally the content of debates is factual.

The question which has been analysed by most of the authors, particularly where disagreement can be seen as relating to facts, is the sociological one of the motive for disagreement. A number of different approaches have been adopted towards this question. Robbins and Johnston[20] make a useful distinction between attributing disagreement to political or value factors and attributing it to factors concerning the nature of science itself. Following this distinction Kopp[21] has argued for an integration of the two approaches, maintaining that a multi-dimensional model is required. The analyses of Mazur[22] and Nelkin[23] fit into the 'political interests approach'. They both attribute the motives or driving forces of disputes to the political views of the scientists involved. Mazur maintains that a scientist's interpretation of ambiguous data is often tied to his position on the innovation about which the controversy exists, for example, in the debate over the effect of low level radiation the proponents of nuclear power are more likely to adopt the threshold model of radiation dose effect. Nelkin argues that scientific experts get used as a political resource, and the scientists are enrolled to justify political positions. In this way, their position in the scientific dispute is implicitly related to their political position. Nelkin's view on the content of debates and on the context of motivation is concisely summarised in the following quote, where she herself employs this distinction: "Whatever political values motivate controversy, the debates usually focus on technical questions." This comment is made in the context of introducing twelve empirical case studies of controversy.[24]

Robbins and Johnston[25] argue that attributing disagreement to political views, bias or 'irrationality', reflects a positivist view of scientific knowledge as an unproblematic reflection of reality. They argue that differentiation at cognitive and occupational levels in the scientific community can lead to disagreement among experts. They point out that scientists work within a large range of scientific specialties, each of which employ different methods and have a different framework in which to order and interpret information about the world. Thus, scientists from different specialities can legitimately offer different explanations of the same piece of evidence. Academic scientists at least have a dual role, according to Robbins and Johnston, in that they are members of a specialist community from which they derive their specialist information and expertise, and members of the scientific community in general from which they will derive their authority depending on the status of science in general. But the scientific community is differentiated even further in terms of occupational setting. Academic scientists make up only a small proportion of the scientific community, many more scientists work in the widely different institutional settings of industry and government. This differentiation may affect the professional norms under which scientists work and will further help to explain disagreement among experts on scientific questions relating to public policy.

Gillespie et. al.[26], take a similar line to explain the occurrence of disputes in terms of factors intrinsic to scientific development. They argue that there is a complex relationship between scientific concepts, theories and methodologies on the one hand, and ideological and value commitments on the other. Taking the disagreement over the carcinogenicity of Aldrin/Dieldrin between British and U.S. scientists, they suggest that the factual disagreement consisting of different interpretations of the same data can be explained in terms of a number of factors including the different types of scientist involved (i.e., belonging to different specialties), and the use of different criteria to assess carcinogenicity stemming from different institutional affiliations. They argue that the different standards employed to assess carcinogenicity can be explained by reference to the social commitments of the leaders of the two institutions in Britain and the U.S. responsible for producing the advice, and the character of the institution of which they were heads. They also see differences in occupational control exercised over scientists in different specialties and institutional settings as having important implications for the content and form of the knowledge produced. An example of how these various factors, scientific specialty of the scientists involved, institutional affiliation, and social commitments lead scientists to adopt different interpretations of data and different theoretical concepts is given by Gillespie et.al.in a comparison of the positions and views of the leaders of the two institutions in Britain and the U.S. responsible for advising on the particular issue studied. In Britain, the principal advisor was J.M.Barnes who was head of the British Medical Research Council's Toxicology Unit. The unit had close links with the chemical industry and Barnes' social commitments reflected this. Barnes was a toxicologist committed to a traditional toxicological mechanism of carcinogenesis requiring prolonged contact between cells and chemicals. In line with these social and scientific commitments, the British advisors concluded that Aldrin/Dieldrin were not carcinogenic. In the U.S. the principal advisor was U.Saffiotti, head of the National Cancer

Institute's Chemical Carcinogenesis Program, the leading agency in the U.S. crusade against cancer. Again, Saffiotti's social commitments reflected the orientation of this institution. Saffiotti adopted a molecular biological approach and was committed to a 'trigger' mechanism of carcinogenesis, whereby a single molecule can initiate a cancerous response. In line with these social and scientific commitments, the U.S. advisors concluded that Aldrin/Dieldrin were carcinogenic. Gillespie et.al. argue then that scientific experts may adopt different interpretations of the same data because they are motivated by commitments to different scientific specialties and different institutions with their specific orientations.

These are the two general views about the motivation of scientists in controversies, either their position in the dispute is motivated by political interests, as argued by Mazur and Nelkin, or by professional (occupational/institutional and cognitive) differences as argued by Robbins and Johnston, and Gillespie et.al. Kopp[27] argues for a synthesis of these two approaches, applying this multi-dimensional approach to the case of the American debate over fallout hazards. In this debate, she identifies four interrelated roots: disciplinary differences, both conceptual and methodological between geneticists and other medical and physical scientists; institutional differences between academic scientists and officials in the U.S. Atomic Energy Commission; personal conflict between scientists and the A.E.C. chairman Lewis Strauss; and political differences between liberal and conservative scientists over the role of nuclear weapons in American foreign policy. She interprets disagreement as conflicts of several interconnected group interests reinforcing each other.

On the nature of technical disputes there are various views. It has been argued that technical disputes are really about values which are mixed up in the technical issues or expert's advice. It has also been argued that disputes are about facts and values, but a large number of case studies analysed suggest that disagreement is at least primarily about facts. In those cases where disagreement is about facts, the main source of that disagreement is alternative interpretations of data, although there are cases where the validity of data itself is disputed. It may be argued that those views which interpret expert disagreement as concerning values, reflect a positivist model of science. Following this model, it is assumed that, if scientists are confronted with a set of data, then, unless they are biased or influenced by their values, they will come up with the same answer to a problem concerning that data. The fact that so many case studies suggest that the interpretation of data plays a key role in scientific controversies may point to the inadequacy of this model. More recent philosophical views of science would allow for data to be open to a number of interpretations so disputes can be factual and on scientific grounds.[28] Similarly, with respect to the motivation driving disputes between experts, there are a number of different views. Basically, there are those which attribute disagreement to political or value factors, those which attribute disagreement to factors concerning the nature of science itself, and those employing a combination of the two. Again, it may be argued, as Robbins and Johnston and Gillespie et.al. have, that the attribution of disputes to political views or values implies a positivist model of science. It has been shown that the motivation for controversies can stem from factors intrinsic to the nature of science itself, cognitive and occupational differentiations and institutional affiliations.

2.2 Models of Expert Advice

In the discussion above it was argued that some interpretations of controversies involving expert advisors in a context of decision-making about public policy implied an underlying model of science which can be seen as positivist. It may be argued that, to see the nature of these debates as being really about values and to attribute the underlying disagreement or motivation driving the debate to political views or values, implies a positivist model of science.[29] Robbins and Johnston[30] argue that this model is inadequate, particularly in the light of modern sociology and philosophy of science. Ravetz[31] has also pointed to the inadequacy of some traditional views in the sociology and philosophy of science to deal with the reality of the role of science in technological decision-making. It was seen above that a number of case studies clearly showed that disagreement was about facts rather than values and that the motivation driving the debate could be understood in terms of professional differentiation and need not be attributed to bias or political views. This finding does suggest the inadequacy of the positivist model of science which must attribute the disagreement among experts to values, bias or 'irrationality'.

Collingridge[32] has provided a more detailed elucidation of the positivist model and its implications, and has further provided an alternative model to account for disagreement between experts in a context of technological decision-making. He has constructed and assessed these models in the context of a case study of expert disagreement, the American debate over the health effects of lead additives in petrol. The positivist model, which Collingridge refers to as 'Model 1', holds that advice from an expert is objective provided that its author is neutral, unbiased or disinterested. Since, on this model, science is viewed as the collection of data from experience and observation to be used as a base for the construction of theories and, since the expert must be unbiased and disinterested, he must collect and review all the relevant data. Also, on this model, any set of data can only receive one interpretation. This means that, since unbiased experts can be expected to review all the relevant data and, since the data is only open to one interpretation, these experts should always agree. If the experts fail to agree, then this can only be explained in terms of incompetence through the failure by some expert or experts to apply the methods of science correctly or to consider all the relevant data, or through bias or corruption of one or more experts involved.

If the experts involved are unbiased and therefore, according to 'Model 1', they agree on the interpretation of data, they, or the decision-makers, may still disagree on the course of action to be taken on the basis of the data and its interpretation. On 'Model 1' if this type of disagreement occurs it can only be explained in terms of conflicting values. If the expert advisors are unbiased and therefore agree on the facts, any continuing disagreement about the policy decision must derive from a disagreement over the values involved.

Collingridge gives an example of how this model of expert advice would explain the disagreement between the Ethyl Corporation, the leading U.S. manufacturer of lead additives for petrol, and the U.S. Environmental Protection Agency (E.P.A.) on the policy to be adopted regarding lead additives in petrol. The E.P.A. recommended the removal of lead from petrol and the Ethyl Corporation recommended retaining lead in petrol. According to Collingridge, 'Model 1' would interpret this disagreement as one about values. The two organisations had access to unbiased experts who would interpret the data in the same way and reach the same conclusion on its interpretation, that lead from petrol is a health hazard to children. The disagreement would arise because the two parties have different values; the E.P.A. prefers protecting children's health to the continued existence of the Ethyl Corporation, while the Ethyl Corporation prefers its continued existence to protecting the health of children.

Collingridge criticises this model on a number of counts relating to its view of neutrality and its insistence on experts reviewing all the relevant data. Firstly, he argues that the model does not fit reality; in the lead in petrol case the disagreement was about the interpretation of data and not about values. Secondly, a problem with this model is that, by attributing disagreement to bias among experts, it provides a psychological and hence a covert and unexplorable view of bias. There are such a variety of factors that can serve to bias the opinion of an expert and, since there are no straightforward tests for the presence of bias, it is easy to see it everywhere. This poses a problem of how we can ever find a true unbiased opinion. Finally, the model has a 'blackbox' view of expert advice which assumes that a clear separation can be made between questions of policy and technical questions. Experts, on this view, are seen as providers of data and of interpretations of data, but as experts they have no interest in the policy decisions associated with the advice. Collingridge refers to this as an 'instrumental' view of expert advice and argues that it is completely divorced from reality. He argues that most expert advisors either belong to organisations with vested interests in various policy decisions, or are at least clearly identified with such organisations. According to Collingridge the expert can be seen as an advocate, whose task is to offer and defend interpretations of the data which "...lead to the policy option favoured by his master"[33].

After outlining the inadequacies of the positivist model of expert advice, Collingridge proposes an alternative which he refers to as 'Model 2'.[34] Where Robbins and Johnston have suggested that post-Kuhnian sociology of science creates a need for scientific knowledge to be seen as socially constructed,[35] Collingridge's model of expert advice needs us to go no further than Popper's philosophy of science. The model is based on the proposition that any body of data can receive a whole number of mutually conflicting interpretations.[36] Following from this, there is a quite different way in which disagreement over policy can arise. Where both parties are aware of exactly the same set of data, and may share exactly the same values, disagreement may occur on what policy action to take as the parties may favour different interpretations of the data. Collingridge provides an explanation of the lead in petrol case based on this model. In this case both the E.P.A. and the Ethyl Corporation agreed about the data and both operated with the same set of background values which were very uncontentious. The disagreement stemmed from the different interpretations which the parties gave to the data. The E.P.A. interpreted the data as indicating that lead from petrol was a health hazard to children and recommended that lead should be removed from petrol. The Ethyl Corporation, on the other hand, interpreted the data as showing no health hazard and therefore recommended that lead should be retained in petrol. Both parties were operating with something like the following values; "If lead from petrol impairs the health of children, then it should be removed" or "Lead should be removed from petrol only if it impairs the health of children".[37] On this new model objectivity lies in openness in submitting interpretations of data to normal critical scrutiny of scientific experts with rival opinions, according to Collingridge. He argues that this model makes clear the distinction between the 'public argument' which is like any similar debate in science and is about the interpretation of data, not about values, and the 'motivation' behind the argument.[38] The rival motivations of the parties involved ensures that the debate is carried out energetically and that the best case is made for each interpretation of the data and that each interpretation is subjected to intense scrutiny, according to Collingridge. He explains how the new model of expert advice overcomes the failings of the positivist model. Firstly, 'Model 2' gives a much more realistic account of actual disagreement about what action to take. The key elements in debates are factual and concerning the interpretation of data, they are not about values. Secondly, although bias can still occur in debates, it is no longer a hidden psychological element, it is now publicly identifiable and can be rectified by public action. For example, bias may take the form of keeping data secret, unfair distribution of funds or procedural rules which make some claims immune from criticism.[39] Thirdly, the model recognises expert advisors as advocates employed by agencies to put forward and defend as strenuously as possible interpretations of the data which favour the interests of the agency. The scientific debate is often long and critical and the vested interests of scientists and their employers ensures that the debate is a rigorous one.[40]

To summarise the position so far, a distinction can be made between the content of a debate and its context. The content of scientific controversies tends to be 'factual', usually focussing on the interpretation of data. The context of the debate, including the rival motivations of participants, need not be seen in terms of political bias. Disagreement can arise through cognitive and occupational differentiation amongst scientists. Thus, the 'professional differences' approach is important in identifying aspects of the development of science and the scientific community which can account for disagreement amongst experts and for demonstrating that the claim that disagreement stems from bias, assumes a positivist model of science. However, these 'professional differences' can be closely aligned with 'political interests' where a particular institution has a particular interest in an issue combined with a specific scientific approach, as in the Aldrin/Dieldrin case.

The positivist model of scientific advice has been shown to be inadequate in accounting for disagreement amongst experts and Collingridge has provided a model which is better able to explain the existence and character of disagreements. The 'content' of the 'public argument' consists of rival interpretations of data which, if combined with relatively innocuous sets of values, would lead to different policy recommendations. Fuelling this technical controversy are the rival motivations of participants. Combining this model with consideration of the 'professional differences' and 'political interests' approaches, it can be seen that these rival motivations can stem from a scientist's cognitive background or institutional affiliation , and may be tied up with his political interests or those of the agency which employs him. Thus an expert can become an advocate, using technical arguments to defend a position which favours a particular policy option preferred by the agency with which he is affiliated.

C H A P T E R   3

CONTROVERSIES IN

ESOTERIC AREAS OF SCIENCE

3. CONTROVERSIES IN ESOTERIC AREAS OF SCIENCE

3.1 Introduction

Sociologists of science have devoted a considerable amount of effort to the study of scientific controversies both at the 'margins of science' and in relatively esoteric areas of respectable science away from the direct influence of policy implications. These studies provide a useful insight into the way in which controversies arise and, particularly, into the processes by which they are brought to a conclusion. An appreciation of the findings of these studies will provide a balance to considerations of the interaction between policy and science in areas of controversy involving policy-relevant science. Particularly useful in this context are those studies which attempt to identify the processes by which scientific consensus is achieved. The functioning of these processes in areas of esoteric science can then be compared with the situation in policy-relevant scientific controversies.

As noted elsewhere[1] studies of policy-relevant scientific controversies have tended to identify a whole range of characteristics which, it is implied, are specific to controversies in the policy-science area. However, the sociological studies of scientific controversy serve as a useful reminder of the fact that many of these characteristics are common to controversies in esoteric areas of science. It must be pointed out that most of these sociological studies have been performed from a relativist perspective, which means that they tend to emphasise the social nature of science and the role of social processes in the development of controversies and the achievement of consensus. While there are some problems with this approach, there do seem to be a number of lessons to be learnt from this literature which do not entail the total acceptance of a relativist position. This point will be returned to later in the discussion of these studies.

3.2 Sociological Studies of Controversies

In the introduction to a special issue of Social Studies of Science devoted to these studies, Collins draws out a number of key points which they illustrate.[2] One is that, in a controversy, experimentation by itself is not decisive because there is always a degree of local interpretative flexibility. It is pointed out, however, that for a controversy to come to an end, this interpretative flexibility must be limited in some way. The sociological studies have illustrated some of the mechanisms by which this limiting process is achieved. This will be a most useful finding when making a comparison between controversies in esoteric areas of science and those in policy-relevant science. Among the mechanisms revealed by these studies are rhetorical and presentational devices, decisions to preserve prior agreements, cultural constraints, and attitudes amongst the 'inner circle' of scientists working within a sub-field as to the openness or closure of that and other sub-fields. It is argued, by Collins, that consensus is only possible within constraints coming from outside the laboratory work.[3]

One of the early studies in this field was Collins' analysis of the controversy in physics over gravitational radiation[4]. This study concentrated on the problem of the replication of experiments and the associated tranfers of knowledge. In the paper Collins presents two models of scientific knowledge and its transfer, the 'Algorithmical Model' and the 'Enculturational Model'. Under the former there is a finite series of unambiguous instructions which can be formulated, transferred and, when correctly followed, will enable a scientist to copy another's experiment exactly. However,this model is problematic because there is an infinity of possible algorithms and no formal way of selecting the right one. According to the enculturational model the suggestion that the copies are the 'same' presupposes a cultural limitation on the list of variables which might be measured for each experiment. This model, "...involves the transmission of a <u>culture</u> which legitimates and limits the parameters requiring control in the experimental situation, without <u>necessarily formulating, enumerating or understanding them</u>, and which <u>ipso-facto</u> generates the set of anomalous experiments".[5] It follows from this that the transfer of knowledge is not directly monitorable.

According to Collins the enculturational model fits the findings of previous studies better than the algorithmical model. An implication of the model which is supported by empirical evidence is that neither an observer nor a scientist himself can tell whether or not that scientists has the knowledge appropriate for replicating another's experiment before he has tried to make use of it. In other words, the only way of telling that an interaction has been successful is when a scientist produces results acceptable to the scientific community by doing an experiment which works.

The case study which Collins examines involves an experimental result which appears to support the existence of gravity waves, and is difficult to reconcile with current cosmological theories.[6] Several secondary experiments had been performed which, it was claimed, did not confirm the results of the original experiment. It was found, by Collins, that, although the originator's claims were already in doubt, the counter evidence did not amount to much in formal terms. He found that secondary scientists did not need detailed communication with the originator because they were not interested in building what they perceived as isomorphic models of his apparatus. Although a number of possible explanations were available for this phenomenon, Collins interpreted it as demonstrating that in the gravity wave field what counted as a 'working gravity wave detector' was still a contentious matter. So long as the scientists were not sure that the originator's device was detecting gravity waves, there was no special reason for them to try to build one isomorphic with his.

The evidence which Collins provides also shows: the variation in scientists opinion regarding the value of others' experimental set-ups and reported result; that scientists perceived differently the importance of minor variations in bar type detectors (part of the experimental apparatus), and perceived differently which are to be counted as copies of which others; the variation in scientists' perception of the value of various parts of the originator's experimental procedures. It also revealed a whole list of what Collins thinks would be considered 'non-scientific' reasons, which led scientists to believe or doubt experimental findings in the field, with different scientists being affected by different combinations of these factors. Finally, Collins provides evidence to show that scientists were engaged in other than formal methods of argument and persuasion, and that there existed a lack of consensus over formal criteria.

From the fact that scientists were led to their own conclusions about the value of particular experimental findings through considerations of apparently 'non-scientific' arguments, Collins draws the conclusion that there is no set of 'scientific' criteria which can establish the validity of findings in this field. This conclusion does not necessarily follow from that fact, however, since the question of what convinces individual scientists is not the same as that of what provides (logical or 'social') justification of findings.

For Collins, the debate shows that in the gravitational radiation field there are no agreed criteria of replication. Thus, he views the activities of scientists in this field as "..negotiations about the meaning of a competent experiment".[7] By doing this they are, furthermore, negotiating the character of gravitational radiation and building the culture of the field. This relationship between the meaning of a competent experiment and the character of the phenomenon is an important one. Collins explains his position:[8]

> " When a scientist claims that an experiment has been properly replicated, he is claiming that these two sets of events, the original and the replication, should be treated as the <u>same</u>. Further, he is claiming that all experiments which are to be included in the set of 'competent experiments' in the field, must be seen as manifesting the phenomenon."

According to this view, if scientists change what is to count as a competent experiment, then the properties of the phenomenon change. Furthermore, interpretations of the phenomenon provide the category of 'anomalous' experiments when unexpected results are produced. They do so because they limit the type of factor which can be invoked to explain an unexpected result, according to Collins. He argues that in the case of the gravity waves there is no general agreement about what is an expected result and so no clarity about the boundaries of the set of 'unexpected results' Thus, on this interpretation, aspects of the debate such as elaborations of the original experiment by the originator and other scientists' denial of the importance of these, are negotiations over whether the original experiment was 'competent'.

Finally Collins looks at the arguments used to explain the differences in findings between experiments in the field. He finds that these support his contention that the culture of a scientific field limits and legitimates the contents of arguments invoked to explain differences in findings between experiments, and that this culture is relatively underdeveloped in the case of gravitational radiation. He found that most arguments which have appeared in print fitted comfortably with normal physics, but that some other arguments have been suggested which were quite unorthodox, though scientists were less willing to print these or put their names to them. These unorthodox explanatory hypotheses were possible, according to Collins, because the culture of gravity waves was still open and in the process of negotiation and development.

In conclusion Collins points out that the originator could continue to argue indefinitely that experiments which claim to be disconfirmations of his results are not good replications, whereas critics of his findings will argue that they have produced good experiments. Finally, "As far as can be seen there is nothing outside of 'courses of linguistic, conceptual and social behaviour' which can affect the outcome of these arguments, and yet this outcome decides the immediate fate of high fluxes of gravity waves".[9]

In a follow up to this study, Collins re-iterates the point that arguments concerning the existence of the phenomenon turn, not upon experimental results, but on what comes to count as a '<u>well-done</u>' experiment.[10] By this later stage, views had been further clarified, experiments done and arguments more articulated. Again he argues that there were no purely cognitive reasons that would 'force' scientists to disbelieve the claims for gravitational radiation, but that this incredibility was a social product. The purely scientific arguments against the existence of gravity waves were, according to Collins, permeable to counter-claims and the critics of the gravity wave claim were aware of this permeability. Because of this, they attempted to reduce the credibility of the positive experiment and increase the credibility of the negative ones by, what can be seen as, more than purely scientific actions. Collins also found that the critical scientists had well formed opinions about the positive finding before anything which was subsequently claimed to be significant negative experiment had been completed. The role of experiment, according to Collins, was in providing legitimation for views which scientists had already developed. He distinguishes between the scientists' negative belief and their public statements. With respect to their personal beliefs experiment had neither a critical nor a clear cut role. However, he claims that experiment was important in legitimating the publication of negative results and the certainty with which it was claimed in public statements that the positive findings were false. This seems to go some way to meeting the criticism which could be levelled against the earlier paper that there is a distinction between the formation of individual scientist's personal views and the justification of particular knowledge claims which have been confused.

According to Collins' account, individual scientists first came to their personal conclusions about the positive experimental result from one or more of a set of strands of evidence relating the the experiment and its analysis. They are not, however, unanimous in their assessment of the importance of the different strands in demonstrating that the result was mistaken. After scientists had formed their own personal beliefs, a series of experiments were performed which legitimised the openly publishable statement of strong and confident disagreement with the positive results. But, according to Collins, this confidence came only after a sufficient number of experimental reports had built up, showing negative results, and this 'critical mass', as he calls it, of experimental results was triggered by the activies of one particular critical scientist.[11] The sceptical scientist disagreed about which experiments constituted the set of satisfactory confrontations with the positive result. An important point to note is that each of the sets of evidence which initially helped to form the critical scientist's beliefs were found to be unconvincing or less than satisfactory by one or more of the critics, and each of the experiments, with one exception, were found wanting by one or more of the critics. Thus, Collins concludes, the arguments themselves were insufficient to establish the validity of the negative findings.

A crucial step in the process of establishing the negative findings as valid was the activity of one particular group of scientists, according to Collins. This group set out to 'kill' the positive finding he claims, and they performed an experiment to develop a position from which they could criticise the positive finding. Collins suggests that this group acted in a way which is consistent with their being aware of the insufficiency of evidence and arguments in settling the existential status of a phenomenon.

Finally Collins develops the concept of 'interpretative charity' for interpreting a system of belief.[12] He argues that, since the evidence was not inviolable, scientists' conclusions regarding the existence of high flux gravity waves rested on the degree of charity invested in their interpretation of events. Following the relativist perspective, Collins interprets the action of the 'victorious' side in this debate as less 'charitable' rather than more 'rational' or 'correct' than those of the 'defeated' party. His conclusion is that the physics and politics of experiment are not easily distinguishable and that the accepted non-existence of high flux gravity waves was brought about through a social and political process.

The enculturational and algorithmic models of the transfer of scientific knowledge between scientists, which Collins has set up, are discussed by Travis in his study of the controversy over conditioning in planarian worms.[13] This is a controversy within behavioural psychology and the first thing to notice about it is its length - the controversy has lasted for twenty years. This illustrates the point that it is not just policy-relevant scientific controversies which continue for a long time. Again the controversy is seen to centre on the problem of the replication of experiments. The controversy centred on a series of experiments which purported to show classical conditioning in planarian worms, and further showed some surprising aspects of conditioning in these worms, and attempted replications of these experiments.

First Travis discusses the two models already referred to. From the enculturational model he draws a number of implications. One is that for knowledge to be transferred, as described by the algorithmic model, a shared culture must already exist between the scientists involved in the transfer. Second, the only way for scientists to know that they have the knowledge to produce a competent replication is actually to produce what everyone agrees to be a working experiment. Third, when scientific culture is sufficiently open to preclude consensual limitation of parameters, arguments about the status of experiments will, in effect, be negotiations about what is to count as a working experiment.[14]

In his account of the controversy Travis points out that positive results were achieved in attempted replications when the experimenters were ignorant of the important variables involved, so that successful replicators must sometimes do the right things as a matter of course without analysing their procedures in detail. For example, in the planarian learning case study some experimenters were achieving positive results and some negative, and it was not discovered until later that those achieving negative results had been cleaning part of the apparatus along which the worms were running and were thus removing the worms' slime trails, while those achieving positive results did not clean away the slime. Also brought out in the study of the controversy is the importance of tacit knowledge with respect to the particular objects of study, in this case planarian worms. While some experimenters had developed an extensive body of tacit knowledge relevant to planarian worms, others continued to call for a set of instructions for producing classical conditioning in these worms, even after a large amount of published material was already available. It is also noted, by Travis, that arguments from the philosophy and sociology of science were used in the debate about the nature of replications, and some participants in the controversy were explicitly aware of the social processes involved and of the ambiguity of the notion of replication.

Finally, according to Travis, the controversy represented negotiations about the particular sense in which experiments should be taken to be the 'same' or different. Furthermore, the characteristics of the phenomenon depend on the outcome of these arguments. The replicability and acceptability of the phenomenon are inextricably interwoven, according to Travis, so that replicability follows as much from acceptability as acceptability follows from replicability.

One of the most interesting questions concerning controversies in science is the way in which they are resolved and the study by Pickering of the Magnetic Monopole controversy in experimental physics begins to address this problem.[15] The controversy consists of a particular scientific discovery claim and its reception.

According to Pickering, controversies in which scientists examine and challenge each other's presuppositions in detail, can lead to a regress of arguments over the validity of rival positions which is potentially infinite. From this potentiality of infinite regress he argues, following Collins, [16] that the resolution of such a controversy must be regarded as the outcome of a social process. Resolution of a controversy is, for Pickering, "...a collective decision to stop arguing",[17] and he uses the Magnetic Monopole case to investigate the mechanisms by which this collective decision is reached. He claims that the debate was conducted within a "...culturally constructed field of discourse", and emphasises the importance of prior social agreements concerning the validity of particular experiments in structuring the debate.[18] He further emphasises the importance of theoretical conceptions of the world within which the agreements about particular experiments are constituted and which provide the vehicle for 'transmitting' agreements from the context of one experiment to another. Thus, for Pickering, the conduct and outcome of the debate were socially determined.

A distinction is made between the 'instrumental' and the 'phenomenal' levels of the debate. The instrumental level centres on the apparatus, techniques and procedures from which scientists derive a set of data in a given experiment. At this level debates can be kept up as long as participants desire by challenging the particulars of any individual experiment. The phenomenal level centres on the interpretation of data. As Pickering points out, there are in principle an arbitary number of interpretations of any set of data. He further suggests that agreement to close a debate is typically reached at both levels simultaneously.

According to Pickering, in the Magnetic Monopole controversy phenomenal arguments were structured by socially-available theoretical conceptions, such that new interpretations were claimed to correspond to hitherto observed entities and not to conflict with accepted interpretations of other experiments. That is, no prior agreement at the phenomenal level had to be broken to accept these new interpretations. In fact ultimately as the controversy progressed it was the decision to incorporate prior agreements concerning routine experimental practice and theoretical conceptions of the natural world which led to the rejection of the Magnetic Monopole by the group whose experiment appeared to support its existence.[19]

Also discussed in Pickering's study is the use of agreed properties of the natural world as 'benchmarks' for instrumental practice.[20] He shows how the group whose experiment appeared to support the existence of a magnetic monopole decided to preserve agreement, over the existence of a phenomenon called the 'platinum peak'. This phenomenon provided a benchmark for the group to be used in the interpretation of their experimental data. By assenting to the platinum peak, the group were cutting off one possible line of counter-argument. But, Pickering emphasises that the platinum peak itself was not an unproblematic empirical 'fact' and that theoretical concepts were intimately involved in its production. Agreement between scientists who supported or desired the existence of a magnetic monopole, or the existence of the platinum peak, meant that they were resorting to and reinforcing a particular theory. They were also establishing links with other experimenters through this theory.[21]

According to Pickering the monopole debate was structured throughout by the participants' decisions to confine their arguments within a limited range of socially acceptable conceptualisations of the natural world. At the phenomenal level the controversy was conducted solely in terms of two alternative interpretations of evidence and at the instrumental level the platinum peak 'benchmark' emerged from three models of the genesis of heavy nuclei. It is also argued, by Pickering, that instrumental practice and phenomenal interpretations were assessed together as the conceptualisations which were agreed upon were constitutive of agreements over the validity of individual experiments. These conceptualisations also served to transmit agreement from one experimental context to another. The debate was structured by the participants' attempt to maintain prior agreements and, as mentioned before, ultimately the group whose experiments had appeared to support the existence of magnetic monopoles abandoned this idea in order to maintain the platinum peak which had been socially agreed in earlier experiments.

Emphasis is placed by Pickering on the flexibility of the empirical base of science which his interpretation of the magnetic monopole debate implies. The case suggests that benchmarks are used to constrain instrumental practice, but they do not specify it completely. Every experiment remains unique in concrete detail, and individual peculiarities can always be made the subject of debate. This means that assessment of the validity of an experiment involves assessment of its phenomenal interpretation, that is the very phenomenon under experimental investigation. He goes on to argue that this implies that whole complexes of instrumental practices might become 'tuned' within a framework of conceptualisations of the natural world, such that acceptance of specific practices or treating particular aspects of individual experiments as irrelevant will depend of the success or failure of individual experiments to generate socially-acceptable phenomena.[22]

This flexibility of the empirical base further implies the possibility of a radical incommensurability between the 'natural worlds' of societies subscribing to different conceptual frameworks, according to Pickering, so that each society would tune its instrumental practice to phenomena appropriate to its conceptual framework with no necessary relation existing between these sets of phenomena.

Finally, Pickering points out that this study raises the interesting question of how conceptualisations of the natural world change and evolve and how these conceptual changes affect changes in the patterns of prior agreements and hence the context in which the validity of new experiments are judged. The monopole debate itself, while being conducted mostly within a fixed set of socially-acceptable concepts, which were essentially static, did produce one example of conceptual development, though one which was rejected fairly quickly (the misplacement hypothesis).[23]

An especially interesting study among the literature on controversies is a discussion of a latent controversy which did not develop. This is Harvey's study of the case of an experimental result which appeared to contradict Quantum Mechanics and support a rival theory.[24] The anomalous result was not accepted by Quantum physicists, and, when an experiment was completed which represented an acceptable replication of the first and confirmed Quantum Mechanics (QM), even the scientists whose result was anomalous agreed that this result was not acceptable.

The conclusion of sociologists of science that empirical facts by themselves do not determine the fate of knowledge is reiterated by Harvey. According to the argument, the social context in which a scientist operates has an important bearing on his approach to his own work and his attitude to results produced by other scientists. Harvey's claim is that in this case study the outcome of the evaluation of both theoretical and empirical knowledge-claims was heavily dependant on the cultural context in which it occurred. In this particular case Harvey argues that consensus about the status of ideas under discussion was attained without difficulty, even though many of these ideas were not amenable to direct empirical test. Furthermore, the physicists involved were often explicit about the ways in which non-empirical criteria could be used to evaluate knowledge claims.

A concept of the 'plausibility of ideas' is developed by Harvey who prefers this to the notion of 'truth-content'.[25] He points out that in the case study scientists found it necessary to make assumptions about their experiments which could not be checked empirically, and different experimental designs implied different sets of assumptions. However, he noted that the physicists had no difficulty in choosing between the various possible assumptions and in providing a justification for the claim that the assumption chosen was plausible. To do this they drew on their experience and knowledge, or, as Harvey puts it, their culture. Similarly the physicists had little difficulty in deciding to reject the anomalous experiment, even though they were unable to identify a specific error in the experiment. Also demonstrated in Harvey's analysis is the way in which the plausibility of some ideas can change over time. He identifies a specific plausibility shift which was brought about, not by the use of empirical evidence, but by the behaviour of certain physicists, that is, according to his analysis, plausibility can be linked to social action.

This case study illustrates the importance of assumptions involved in scientific experiments and the implicit nature of these. Two stages in the evaluation of the plausibility of these assumptions are identified. First, a range of possible assumptions is examined, each corresponding to a different experimental design, and the most plausible of these is chosen. Then arguments are constructed to maximise the plausibility of the chosen assumption. In the 'QM' case study there were two possible experimental designs to chose between, each of which involved different assumptions. The physicists agreed that one set of assumptions was less reliable than the other although no vigorous arguments were put forward in public in favour of that assumption rather than the other. Significantly, both assumptions were in agreement with past experience, and in both cases it was possible to construct counter examples, according to Harvey.[26]

The physicists explicitly acknowledged that their assessment of the assumptions was in terms of 'reasonableness' which was regarded as a subjective judgement. In addition, this choice between assumptions implied a choice between two different sets of apparatus and Harvey argues that, in a sense, the 'real' choice amounted to negotiations over gaining access to equipment and funds.[27] In justifying the chosen assumption the physicists relied on their shared scientific culture, according to Harvey. They felt strongly that the assumption was unexplainable and used rhetorical arguments to criticise any argument which might be suggested as an attack on it. This is an important point to take note of. The shared culture is critical in that, "Agreement is only possible insofar as the cultural background and the aims of the participants are shared."[28]

With regard to the plausibility of the anomalous experiment the physicists whom Harvey studied seem to have made up their minds that it was unreliable before any further empirical evidence emerged. These opinions were formed on the basis of a variety of arguments of a mostly 'non-empirical' nature, according to Harvey's account. These included inductive arguments, <u>ad hominem</u> arguments, deference to expertise, and some technical arguments which, according to Harvey, were less than conclusive. Another argument was that the experiment and its interpretation involved systematic errors. Again Harvey argues that this relies on the physicists' shared tacit knowledge as to what constitutes an implausible hypothesis. The case against the anomalous experiment was then, according to Harvey's account, based on 'likelihood and plausibility'.[29]

Following the initial anomalous experiment and the formation of the view amongst physicists that this experiment must be in error, an attempt was made by a different group to replicate the experiment. Although there were several differences between the two experiments, these were seen as trivial and the second experiment was accepted as a replication of the first. The differences between the experiments were seen only as a possible source of the error in the first experiment. The knowledge which enabled physicists to accept the differences as trivial was, according to Harvey, theory-dependant. At this stage even those who had performed the anomalous experiment agreed that it must have been in error. All the arguments which were used to reject the anomalous experiment were based on appeals to reasonableness and plausibility, according to Harvey. As soon as each individual felt that the issue had been settled to his satisfaction he left the field and then the issue really had been settled, as Harvey puts it, "Ultimately physicists vote with their feet".[30]

A very important distinction which Harvey draws is that between what is logically possible and what is socially acceptable. He points out that it would have been logically possible for the experimenters who produced the anomalous result to have continued to defend the validity of their results. But, although this is logically possible, it would probably not be socially acceptable within the community of physicists involved in the field. Another possibility would be for a physicist to take up one of the remaining loopholes and devote his time to testing it experimentally. This, in fact, did happen and Harvey shows how this very action brought about a shift in the distribution of plausibility, confirming that plausibility is socially constructed. He shows that two loopholes which remained after the rejections of the anomalous result were both effectively dismissed without being shown to be invalid. However, while they were both initially seen as unimportant, one physicist has planned an experiment to test one of the loopholes. This has brought about a general acknowledgment that the proposal is at least plausible enough to be worth testing, while attitudes appear not to have changed with respect to the other loophole. This does not mean, however, that the other physicists expect a positive result from the test of the loophole, nor that they would accept one if it occurred. For Harvey, it shows that social action affects the distribution of plausibility independant of any inherent worth in an idea. One problem with this argument is that it does not explain why the physicist in question chose to test that particular loophole. In fact, the very action of chosing implies that the plausibility given to the two loopholes was not equal in the first place, at least for the physicist in question, or that the plausibility of both loopholes was fairly high and resources would only allow the testing of one.

The main argument of Harvey's study is that the apparent certainty of a group of beliefs about the plausibility of assumptions, the error of a particular experiment, and the falsification of a rival theory to 'QM', are based on grounds which are ultimately conventional. These beliefs depend on the acceptance of certain notions claimed from the general culture of physics and the local culture surrounding the scientific field of activity. It was these notions which determined the allocation of plausibility, according to Harvey.[31]

A comparison is made between this case study and other studies of controversies which have shown that, "Even 'defeated' parties can continue to erect logically-defensible positions".[32] For Harvey, "The suggestion is that the winning side does not possess truth, but rather that it has monopolised plausibility".[33] Another point which Harvey draws from this case study is that the two sides in a controversy do not start off with equal strength, or, as he puts it, "The 'social negotiation of reality' does not take place in a vacuum".[34] In this case there was no group of supporters for the rival theory to question the validity of the assumptions, to defend the anomalous experiment, or to press for a whole range of new experiments to test 'loopholes'. From this Harvey concludes that 'QM' had an 'easier ride' than it might otherwise have done.

Finally, he discusses the limits to the scope of negotiation which the social context places on cognitive activity. He attempts to show that relativism and the notion of plausibility can account for the apparent certainty of our present beliefs, while recognising the negotiatability of belief. He also argues that, since the social context is dynamic, accepted beliefs become the basis for future practice, generating new cases of cognitive development which, in turn, modify or support the accepted beliefs. It then becomes difficult to compare these beliefs with rejected beliefs:[35]

> "Even when it is pointed out that the viewpoint of the 'losers' remains logically tenable, it is difficult for the reader to remain impartial in the face of the sheer weight of numbers in the 'winning' camp. The plausibility of the technical argument cannot be treated as distinct from the social context in which these arguments operate."

The discussion of the debate on solar nutrino science by Pinch draws direct attention to the possibility of comparison between this sort of debate in an esoteric area of science with debates which are at the centre of public attention, such as that over the Nuclear Power issue.[36] He concentrates on the issue of scientists perceptions of the certainty of scientific knowledge and emphasises the importance of craft practices in influencing these perspectives. A major finding in this study is that the craft element in science gives scientists confidence in their own results, but can be a source of uncertainty for scientists unfamiliar with the craft practices in use outside their own fields. Thus, even in esoteric areas, scientific certainty can be a contentious issue. An interesting characteristic of the debate in solar nutrino science is that it draws on expertise from several scientific areas and Pinch identifies four main specialties involved: radiochemistry, nuclear physics, astro physics, and nutrino physics. He further points out that, while problems in solar nutrino science do not always fit neatly into any of the four specialties, proposed solutions to the solar nutrino problem are derived from one or another of them.

The problem in solar nutrino science is a mis-match between the theoretical prediction of nutrino fluxes emitted from the sun and the results of an experiment to detect this flux. According to Pinch this problem, which has existed for a decade, has become one of identifying the particular specialty most likely to be the culprit for the discrepancy. Since the scientists involved tend to have expertise in one specialty only, they tend to blame other specialties for the solar nutrino problem. Furthermore different perceptions of certainty tend to be polarised between different specialties, according to Pinch. He attempts to assess how scientists in different specialties of solar nutrino science evaluated the certainty of their own and other areas.

It was found, by Pinch, that all four specialties were deemed to be sufficiently uncertain as to be suspect, and in every case it was scientists from one or more of the other areas who cast doubt on a particular specialty. He did also find that scientists were aware of the problem of having confidence in their own field, whilst criticising others and they stressed that they had made efforts to explore possible avenues of uncertainty in their own field as thoroughly as possible.

The most common criticisms which Pinch found of each specialty involved references to large complicated systems, extrapolated data, and reliance on arbitary assumptions. In one area, uncertainties were suggested as arising from laboratory measurements, and in some cases semi-philosophical objections were used. These types of criticisms, Pinch argues, are more compelling for those on the 'outside' of the immediate specialty. [37] In identifying the common theories which emerged from criticisms of the various specialties involved, Pinch highlights the basing of these criticisms on highly idealised models of well-grounded reliable science. He suggests that the usage of such models reflects the rhetorical power which they can have. The interpretation which Pinch gives to this cross-specialty criticism centres on the notion of craft practices. The craft practices and tacit knowledge incorporated in scientific activity provided the ammunition for criticism. As Pinch points out, the certainty of any area can always be challenged by attacking its craft practices. " The taken-for-granted assumptions and extrapolations which are part of the routine craft practices of one area may seem mysterious and open to doubt to scientists unfamiliar with that specialty." [38]

After studying scientists' experiences of uncertainty Pinch turns to look at perceptions of certainty. He discovered that scientists who found uncertainties in other areas had no trouble in defending their own particular specialty in which they had a good deal of confidence. From his respondents' replies Pinch concludes that the confidence which specialists place in their own techniques results in part from the fact that they produce self-consistent answers in terms of the solar nutrino problem and that such techniques 'work' when they are used on other less controversial problems.[39]

For Pinch, then, it is the craft activity within science which provides scientists working within a specialty with their confidence of that specialty and provides the focus for criticism of scientists working outside that specialty. The tacit nature of craft practices means that scientists employing them may not be able to spell out the details of the procedures followed in such a way as to convince a determined critic, which is why they become the focus of criticism and uncertainty.

One further argument which Pinch introduces relates to the audience for which presentations of certainty and uncertainty in science are intended. He discusses the concept of the 'core-set', developed by Collins,[40] which describes a group of scientists working directly in an area of controversy at the frontiers of research. These areas are characterised both by controversy and by uncertainty of scientific knowledge, even from the point of view of the practitioners. Most scientists rarely get the opportunity to be involved in core-set type activity according to Pinch, since most of science is uncontroversial. According to Collins it is only scientists within the core-set who are aware of the lack of compulsion of experimental claims and, therefore, of the general uncertainty encountered at the research front. The solar nutrino scientists represent a core-set for Pinch. This poses a problem in that the scientists whom Pinch studied seemed not to express the doubts and uncertainties that would be expected from core-set members. There were some respondents who were willing to entertain the possibility of uncertainties within their own specialties, but the main reaction was to express confidence in their own specialty and doubt about others' specialties. The key to this problem, Pinch argues, is the audience for which comments are meant. The presentation of certainty within a scientist's own specialty reflected the fact that the sociologist studying the scientist represented a public audience outside the specialty to whom uncertainties were unlikely to be revealed as long as specialists were unable to provide solutions to perceived problems within their own field. Thus, Pinch argues, the perceived audience for the comments determined the presentation of scientific certainty to a large extent.[41]

In conclusion Pinch reiterates that it is the craft nature of many scientific practices which makes it difficult to assess certainty in science. "Confidence is high amongst those who routinely use such practices; but, to those unfamiliar with them, they provide a source of doubt and uncertainty".[42] He argues that the lack of agreement over scientific certainty in solar nutrino science is consistent with the view that the origin of assessments of certainty lie in the social world. Furthermore, consistent with this view, according to Pinch, is the observed constraint which the potential audience for comments on certainty can have on these comments. Finally, he draws out the implications of his analysis for public-science debates. These are that, since scientific certainty can be a contentious issue in an area of basic science, it is no surprise to find similar debates appearing in public-science controversies. Also, in public-science, it may be useful to take into account the relevant audiences for comments on scientific certainty.[43]

While the account of the different assessments of certainty and uncertainty in the solar nutrino controversy is interesting and Pinch's analysis useful, there seems to be one problem with this analysis. The two main points of the author's conclusions seem to rely on contradictory interpretations of his evidence. First he takes the comments of scientists at face value to argue that the craft nature of science means that specialists in a particular field have confidence in that specialty. Then he argues that those public statements of confidence in fact masked uncertainty and doubt about even their own specialties because the scientists involved were members of a core-set, of which a defining characteristic is uncertainty. Both suggestions, that the craft nature of science can lead to different assessments of certainty across different specialties by scientists working 'inside' and 'outside' those specialties, and that the audience for comments by scientists must be considered since more uncertainty would be admitted to 'insiders' working within the same specialty than would be expressed to 'outsiders' or more public audiences, are eminently reasonable. But, it seems that the two together cannot follow from this particular set of evidence. Either the public statements of certainty in a scientists own specialty are reliable and the 'craft practices' argument holds, or they are over-stated and the question of dressing up comments for different audiences arises.

3.3 Discussion: The Implications for Controversies in Policy-Relevant Areas of Science.

While most of these studies have been performed from a relativist perspective, many of the useful points which they make do not depend on this perspective. For the following discussion it will not be necessary to analyse the relative merits of this approach.[44] It should be noted, however, that while much sociology of science work has been done using this approach, particularly by the Edinburgh and Bath 'schools', the perspective is not without its critics within sociology of science. For example one of the main criticisms of this type of work is that it is not clear that its authors have actually been faithful to their own tenets of 'impartiality' and 'symmetry'.[45] Furthermore, it is argued, intriguing episodes often resist generalisation and the cases tend to focus on the activities of the more visible scientific elite, ignoring rank and file scientists, non-scientists and the 'external' culture.[46] Another criticism which is levelled specifically against the 'interest' model of the Edinburgh school is that, while it explains why particular individuals or groups believe in a set of propositions, it does not tell us whether and how these propositions themselves embody social factors, nor whether and how social factors influence the survival of and acceptance of knowledge claims.[47] It is pointed out that the question of what causes individual's belief preferences is epistemologically irrelevant.[48] Now this criticism can also be levelled against some of the studies under discussion here. As was noted above, Collins for example jumps from the argument that individual scientists' views about the value of particular experimental findings were formed on the basis of 'non-scientific' arguments to the conclusion that there is no set of 'scientific' criteria which can establish the validity of findings in the field in question.[49] While both statements might possibly be true the latter does not follow from the former.

There are a number of common inter-related themes in these studies which it is worth drawing out. One is the importance of tacit knowledge in the production of scientific knowledge and in either limiting or contributing to controversy. One aspect of the tacit dimension is captured by Collins' 'enculturational' model of the transfer of knowledge relevant to experimental replication. This model is supported by evidence from his own studies of the gravitational radiation controversy and the development of the TEA laser,[50] and by Travis's study of the planarian worms controversy. It is worth repeating the implications which Travis draws from this model: for knowledge to be transferred as described by the 'algorithmic' model a shared culture must already exist between the scientists involved in the transfer; the only way for scientists to know that they have the knowledge to produce a competent replication is actually to produce what everyone agrees to be a working experiment; where scientific culture is sufficiently open to preclude consensual limitation of parameters arguments about the status of experiments will, in effect, be negotiations about what is to count as a working experiment.[51] So, where a particular scientific culture is developed and shared, experimental replication is straightforward. Where the culture is developed but not shared the transfer of knowledge must involve the transfer of a culture but, where the culture is not developed it must be negotiated by assessing what is to count as a valid experiment.

Another illustration of the importance of tacit knowledge and scientific culture is given in Harvey's study, where it was shown that physicists justified the assumptions which they made by relying on their culture. Scientists were able to agree about the relative merits of different assumptions and to provide justification for the accepted assumption without reference to any rigorous arguments. A very important conclusion which Harvey draws from this is that agreement is only possible when participants share this cultural background and have the same aims. These considerations show how tacit knowledge and scientists' cultural background enable knowledge to be transferred and agreement to be reached on what is to count as a good experiment, or what assumptions are justified. The other side of the coin is brought out very clearly in Pinch's study of the solar nutrino controversy.

In the solar nutrino debate it is the lack of shared craft practices which leads to uncertainty and controversy. This case study is particularly instructive for the consideration of public science debates, since it involves a number of different specialties, a situation common in more public debates. While Pinch's conclusions appear to rely on conflicting interpretations of his evidence, the arguments, that craft practices produce uncertainty to those unfamiliar with them, and provide the focus of criticism from 'outsiders' while providing 'insiders' with confidence in their own specialty, and that the audience for which statements of certainty are made should be taken into account in assessing certainty are, on their own, very convincing. It could be argued, however, that scientists working within a particular specialty, regularly employing its craft practices, are more aware of the uncertainties in that specialty and, being unacquainted with the craft practices of other specialties, have to accept them at face value. Expressions of certainty in one's own specialty and uncertainty in others' such as Pinch found, could then reflect more a move to protect one's own specialty from outside criticism rather than a genuine expression of certainty. This is a problem that is not really resolved in Pinch's paper, except to say that criticism of other specialties tended to focus on their craft practices. What can be fairly confidently concluded, however, is that scientists' perception of certainty will be different for their own specialties, where they are accustomed to working with particular craft practices, than for other specialties where they are not familiar with the craft practices. Furthermore, if they do want to criticise work from other specialties, the craft practices are a rich source of ammunition.

So tacit knowledge and craft practices involved in scientific activity can, in some situations, provide the mechanism by which controversy is restricted and ultimately consensus is achieved. However, in other circumstances, they can lead to uncertainty and controversy, particularly if the scientists involved in debate share different bodies of tacit knowledge. This leads to another of the themes which emerge from this literature. That is, the potentially infinite nature of controversy and the mechanisms by which it is contained and ultimately closed. Related to this theme is that of the inconclusive nature of experiment. These themes are exemplified in the distinction which Harvey makes between what is logically possible and what is socially acceptable. In all the controversies under discussion it seems thatit would have been logically possible for the 'losing side' to continue to defend their initial viewpoints. In the controversy over the challenge to Quantum Mechanics (QM), it would have been logically possible for the experimenters who produced the anomalous result to have continued to defend its validity by arguing that experiments which were claimed to be disconfirmations of their results were not good replications of the original experiment. In the gravity waves case, none of the arguments or evidence which convinced scientists of the invalidity of the claim for the existence of high flux gravity waves was conclusive or inviolable, and, in discussing the magnetic monopole controversy, Pickering points out that controversies in which scientists examine and challenge each other's presuppositions are potentially infinite, because they can lead to a regress of arguments over the validity of rival positions. At the instrumental level, debates can be kept up by challenging the particularities of any individual experiment; at the phenomenal level there are an arbitrary number of interpretations of any set of data. The point, with reference to the instrumental level, is brought out in studying experiments and their replication, and it forms the foundation of the difference between Collins' algorithmic and enculturational models of knowledge transfer. The problem with the algorithmic model is that any algorithm describing an experiment is potentially infinitely long.

So controversies are potentially infinite, and there is a flexibility to the empirical base of science. What also emerges from these studies is that various mechanisms are available for limiting this potentially infinite debate, which ultimately allow consensus to emerge. What the authors under discussion argue is that these mechanisms lie in the social realm, or are provided by the tacit knowledge and craft practices in science. This is the point of Harvey's distinction; while it was logically possible for experimenters to continue to defend the validity of a result which conflicted with QM, it would have been socially unacceptable within the community of QM physicists. It is also the point of Pickering's claim that resolution of controversy is, "...a collective decision to stop arguing"[52]. In the magnetic monopole case, Pickering argues that it was decisions to preserve prior social agreements which structured and limited the debate. These agreements concerned the interpretation of other experiments and accepted theoretical conceptions. They limited the debate in that even the group who produced the experimental result which suggested the existence of the magnetic monopole ultimately rejected this claim by choosing to preserve prior agreements over other experimental findings and theoretical conceptions, such as the platinum peak. They structured the debate in that it was conducted within a limited range of socially acceptable conceptualisations of the natural world, such that only two alternative interpretations of the evidence were considered, and only three possible experimental observations were considered as benchmarks for the interpretation of the new experimental result. One mechanism of closure, then, is the decision to preserve prior agreements.

Another mechanism suggested by Harvey is the cultural constraint of the social acceptability of continuing to defend a position which others in the field have rejected. This pressure is particularly strong when, as Harvey suggests, scientists start to leave the field as soon as they are satisfied that the issue is resolved, at least to the extent of no longer being interesting. That is, once the 'winning side' have established the 'plausibility' of their own position and the 'implausibility'of their opponents.[53] In the case of expressions of certainty within a particular specialty and uncertainty towards other specialties, this could be seen as an attempt to restrict controversy to discussions of possible errors arising from a particular specialty other than the scientist's own. In a sense, then, this might be considered as at least an attempt at closing a debate, and Collins includes it in his list of mechanisms of closure when introducing this literature.[54] However, it seems more likely that different perceptions of certainty across different specialties would stimulate rather than restrict controversy.

Another mechanism which is discussed is the rhetorical and presentational action of particular groups or individual scientists. This process is suggested as significant in the gravity wave case, where one group of scientists set out to kill the positive experimental finding in as short a time as possible, according to Collins. It is also emphasised in Harvey's study, where he claims that the plausibility of various ideas shifted over time simply as a result of one scientist planning an experiment to test these ideas. In Harvey's case, however the argument is, as was seen above, rather weak. In the gravity wave case Collins argues that one particular group of physicists performed an experiment to establish a platform from which to criticise the positive result and that they acted in a way which is consistent with their being aware of the insufficiency of evidence and arguments in settling the existential status of a phenomenon. Of course, it could be argued against Collins that, by performing an experiment they acted in a way which is consistent with their being aware of the powerful, if not decisive, role of experiment in settling the existential status of a phenomenon. What Collins shows, using evidence taken from interviews with the actors involved, is that the aim of the group in question was to kill the positive result as quickly as possible. There already existed what Collins describes as a 'critical mass' of experiments which appeared to contradict the positive result and, he claims, these were 'triggered' by the critical group's vociferous attack on the positive finding. Thus, the group's role was as a catalyst. However, Collins bases his argument on evidence which clearly shows the intentions and attitudes of the group in question, while he does not assess the influence which their experiment and rhetorical attack had, claiming only that, "There is no reason to think that Quest (the head of the group) was unsuccessful in his aims..." . He claims that this group persuaded scientists to focus on the accumulated evidence supporting the non-existence of the high flux gravity wave, while ignoring the various weaknesses in the arguments. It seems rather odd to claim that a particular group of experimenters had a crucial effect in a controversy and then to base this claim, not on an analysis of the effects of their action within the controversy but, on the aims of the group. However, if Collins' claim can be accepted, it does show how specific actions by individuals at a crucial point in a controversy can be influential in moving the controversy towards consensus.

At this point, it is necessary to return to the problem of the relationship between individual scientists beliefs and the validity of knowledge claims. While it may be legitimately claimed that a particular group's action influenced the progress of a controversy by helping to persuade other scientists to reject a particular knowledge claim, it would be a mistake to claim that it was this action which established the validity of the counter-claim. While experiments may be seen as providing a platform from which to make knowledge claims, the actual outcome of these experiments is crucial. In the gravity waves case, one positive result had been followed by a series of negative results, and clearly if the group which Collins discussed had produced a positive result instead of a negative one the development of the controversy would have been very different, and their vociferous attack on the original experiment would have been untenable. It is the arguments and experimental findings which scientists accept that establish the validity of a knowledge claim, not the process of individual scientists acceptance of them.

Related to this issue is the question of the relationship between scientists' personal beliefs and their public statements. Thisdistinction clearly emerges from the studies under discussion.[56] Again, it is Collins who makes the observation that, whilst scientists' beliefs are formed before any significant experimental results emerge, or before any rigorous arguments are available, their public statements are usually accompanied by experimental results. He argues that the role of experiment is to provide legitimation for views which scientists had already developed. Experiment legitimates the publication of negative results and the certainty with which claims are made in public statements. In Harvey's study it can also be seen that scientists had made up their own minds before any new empirical evidence had emerged. He concludes that the case against the anomalous experimental result was based on likelihood and plausibility, but it was scientists' individual beliefs which were based on this, not their public statements. It was not until an acceptable replication of the anomalous experiment had been performed that the latter's results could be publicly rejected. This distinction between scientists' individual beliefs and their public statements is an important one.

One theme which does clearly emerge from this literature is the lack of any hard rules for assessing knowledge claims. In the QM study, Harvey found that physicists had little difficulty in deciding to reject the anomalous experiment, even though they were unable to identify a specific error in it. Also in this case, physicists agreed that one set of assumptions was less reliable than another, although no rigorous arguments were put forward in public to support this decision, even though both sets of assumptions were in agreement with past experience, and, in both cases, it was possible to construct counter examples. In the studies of replication by Travis and Collins, it was found that the controversy represented negotiations about the particular sense in which experiments could be taken to be the 'same' or different. So no hard rules existed for judging an attempted replication. In the magnetic monopole study Pickering argues that, while benchmarks serve to constrain instrumental practice, they do not specify it completely. Evidence also supporting the lack of formal criteria of assessment is provided in Collins' study, which shows that scientists disagreed on: the value of other's experimental set-ups and reported results; the importance of minor variations in experimental apparatus; the value of various experimental procedures.

Finally, a point which a number of these authors[57] make is that, when scientists are negotiating the validity of a particular experiment, they are also negotiating the character of the phenomenon under test. Thus, the character of the phenomenon and the validity of experiments are intimately related. For example, Collins claims that arguments concerning the existence of the phenomena turn not upon experimental results, but upon what comes to count as a 'well-done' experiment. However, what he misses is that what comes to count as a 'well-done' experiment is influenced by experimental results and the way they relate to previously accepted results and theories.

This does not conflict with the main argument, though, that the negotiations over what is to count as a 'well done' experiment, and the character of the phenomena are bound up. It follows from this that if what counts as a 'well-done' experiment were changed, then the character of the penomena would change.

There are a number of implications of this work for the analysis of controversies in more policy relevant areas of science. First, of all, as Pinch points out, since controversy occurs in esoteric areas of science we should not be surprised to find it in more public science. Furthermore, care must be taken in attributing aspects of the development and conduct of public science controversies to the influence of policy implications for, while policy implications do have an influence, there can be quite legitimate scientific reasons for a controversy to arise and for it to have particular characteristics and to develop in a particular way. Another point is that, even controversies in esoteric science can be very long running so that a prolonged public science controversy does not, in itself, reveal the influence of policy implications.

The importance of shared tacit or craft knowledge for the ultimate closure of debates revealed in these studies has significant implications for public science debates. The latter almost invariably involve a number of scientific specialties and sometimes completely separate disciplines. This means that participants in these debates will not have the shared tacit knowledge required for the sort of closure mechanisms identified in these studies of esoteric science to operate. This will mean that consensus is very difficult to achieve, since practitioners from different specialties will always be able to criticise each other's craft practices, including tacitly accepted and justified assumptions. These cross-specialty problems came out clearly in Pinch's study of the solar nutrino controversy, where different craft practices formed the focus of criticism and uncertainty. Some of the case studies revealed that craft practices are not always well established in new areas of research, so that a negotiation process occurs where ones which occurred within the same specialty (classical conditioning in behavioural psychology, experimental physics) so that a large body of shared tacit knowledge surrounding the new area would already exist. In a situation where scientists came from different specialties or disciplinary backgrounds with little shared culture, a situation of 'over-criticism' will arise in which a controversy does not move towards closure as both sides attack each other's assumptions and craft practices.

In this context, the fact that controversies are potentially infinite becomes important. If the mechanisms of closure of debates are primarily social, consisting of decisions to preserve prior agreements, social pressure and rhetorical and presentational devices, then some motivation to close a debate must exist and there must be at least enough restriction on rivalry to allow a negotiation process to take place. But in public science debates participants are typically highly motivated, not just through their cognitive backgrounds, but through professional affiliations and their role in the policy debate. Furthermore, the importance of shared culture is again evident, since such mechanisms of closure as the preservation of prior agreements require that both sides of a debate are willing to, or have an interest in, preserving these agreements. If they are from different specialties however, or if they are highly motivated to 'win' the public science debate, they may be more prepared than otherwise to call prior agreements into question.

The lack of any hard rules for assessing knowledge claims in science, which is demonstrated in these sociological case studies, also implies problems for the resolution of public science controversies, since this must rely on a negotiation process. As pointed out above, with motivations to 'win' running very high, the process may not have much chance of success.

A number of other points which have been made in these studies are relevant to the consideration of certainty in policy relevant science. For example, Harvey makes the point that choosing between different assumptions, which imply different experimental procedures, is constrained by the availability of equipment and funds. This is as true for public science as it is for esoteric science. Another point made by Harvey is that the assessment of rival theories takes place within a particular social context, which means that the two sides may not start off on an equal basis. In his example QM started off at an advantage because there was no group of supporters for the rival theory to question its assumptions, defend the anomalous experiment, or to press for a whole range of new experiments to test 'loopholes'. This situation might be described as 'under-critical' and its occurrence in policy relevant areas of science can have profound implications. A phenomenon which Pinch observed and which has been found elsewhere[58] is the use of idealised views of science as rhetorical tools in controversies. This is another phenomenon which occurs in public science controversies as well. Finally, another point made by Pinch refers to the problems caused when issues transcend scientific specialty boundaries. He claims that solutions to the problems in solar nutrino science tend to derive from one or other particular specialty, whereas the problems themselves do not fit neatly into any of these specialties. In areas where science is policy relevant the problems almost invariably transcend scientific specialty boundaries, so that this mis -match between the scope of the problem and the scope of the proposed solutions is very likely to occur, as are the other problems associated with issues which involve a number of scientific specialties.

C H A P T E R 4

CRITICISM IN SCIENCE

CHAPTER 4 CRITICISM IN SCIENCE

4.1 Some Philosophical Views on Criticism

Different philosophical models of science give different roles to criticism in the development of knowledge. However, it will be the aim of this chapter to demonstrate that the major philosophical models all give an important role to criticism while recognising that there must be mechanisms by which it is moderated. A recognition of the dangers of too much, or too little, criticism leads to the development of some models describing these dangers. After an analysis of some of the characteristics of policy relevant science, the models are used to predict problems stemming from the level of criticism to be expected when science attempts to answer policy relevant questions.

For Popper, criticism is the essential feature of science. His whole philosophy is built around the notion of the critical revision of theories, and it is this criticism which makes science rational. "We may identify the critical attitude with the scientific attitude"[1], and, "(I) look upon the critical attitude as characteristic of the rational attitude"[2]. In Popper's scheme, the demarcation of what is, and what is not, science; its aims, methods, foundations, and the growth of knowledge, are all based on criticism of scientific theories.

The criterion of demarcation which Popper uses to distinguish between science and what he calls 'pseudo-science' is the falsifiability of theories. Scientific theories are falsifiable by reference to factual statements, derived from observations, whereas pseudo-scientific theories are not falsifiable. In other words, scientific theories are open to criticism on the basis of observations and, for Popper, it is very important that such theories should not be immunised from falsification by 'conventionalist strategems'. To avoid this possibility he develops methodological rules intended to ensure that theories in science remain open to falsification. While the critical attitude is to be identified with the scientific attitude in Popper's scheme, so the dogmatic attitude is the pseudo-scientific attitude. Furthermore, the former represents the attempt to falsify laws, and the latter is related to the tendency to verify laws.[3]

For Popper the aim of science is to attempt to describe and explain reality but, unlike the verificationists (or logical positivists), he is not interested in the security and justification of knowledge claims but only in the growth of knowledge.[4] In common with the verificationists, Popper thinks that science is a search for truth but, in contrast with the earlier school, he does not think that we can ever know the truth. Even if a particular theory were true, we could never know that it was. "We are seekers for truth, but we are not its possessors."[5] For Popper, since truth is unobtainable, the aim of science is nearness to truth or, as described in Objective Knowledge 'verisimilitude'.[6] In this formulation a stronger theory will be one with a greater verisimilitude, which increases with its truth content and decreases with its falsity content. We can have strong arguments for claiming that one theory has greater verisimilitude than another, though degrees of verisimilitude can never be numerically determined (except in certain limiting cases).[7] While Popper continues to argue that science aims at truth, he appears to have rejected the notion of verisimilitude in the light of criticism, claiming that this rejection does not affect the validity of his theory.[8]

While science aims at truth, what it produces is 'objective knowledge'.[9] This consists of linguistically formulated expectations submitted to critical discussion and is in contrast to subjective knowledge. The latter consists of dispositions and expectations and is, furthermore, not subject to criticism. Objective knowledge consists of the logical contents of books, libraries, computer memories and such like, which Popper refers to as 'world 3'. Subjective knowledge, or the 'world of our conscious experiences' is 'world 2' and the physical world is 'world 1'.[10]

The notion of criticism is important in two senses for Popper's conception of the foundations of knowledge. With respect to these foundations he might describe himself as a 'weak empiricist'.[11] He believes that only 'experience' can help us to make up our minds about the truth and falsity of factual statements, or, as formulated elsewhere, "only observations can give us 'knowledge concerning facts'."[12] However, he maintains that this knowledge does not justify, or establish, the truth of any statement. Like the verificationists, Popper employs a notion of 'basic statements' which might be described as the foundation of knowledge. Unlike the verificationists, these basic statements do not represent the secure bedrock of truth but, on the contrary, are themselves open to criticism. These 'basic statements' have the form of singular existential statements, and they assert that an observable event is occurring in a certain individual region of space and time.

In Popper's scheme every test of a theory "must stop at some basic statement or other which we decide to accept".[13] Here, then, is the dual role for criticism with respect to basic statements. On the one hand basic statements are accepted as true, on the basis of observations, and it is in the light of these statements that theories are subjected to criticism. Thus a theory can be falsified by a basic statement, according to Popper. On the other hand basic statements are, themselves, fallible and so open to criticism. Although these statements are based on observations, it is only a matter of agreement that they are accepted. This fact leads Lakatos to describe Popper's view as 'conventionalist'.[14] In contrast to the verificationists view of science as resting on a bedrock of true statements, for Popper:[15]

> "(Science) is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being."

Criticism is the key to scientific method in Popper's philosophy. "The method of science is the method of bold conjectures and ingenious and severe attempts to refute them"[16]. In this model theory is primary; observations are preceded by theory. Hypotheses and theories tell scientists what sort of observations to make. Theories are proposed and then tested on the basis of observations. Since observations follow from theory, they are theory impregnated, and all theories are conjectural. It is emphasised that this method of science is all within the scope of deductive logic and, here, there is a fundamental logical assymetry between verification and falsification by experience. While falsification based on deductive logic is logically valid, verification based on inductive logic is logically invalid. When a theory is severely tested on the basis of observation, it may pass the test in which case it has a degree of corroboration, or it may fail the test in which case it has been falsified. The key element in the scientific method is, then, subjecting theories to severe tests on the basis of statements derived from observations and thus attempting to refute them.

A most important element in this scheme is the way in which scientific knowledge grows. Progress is made in science when one theory, which has been falsified, is replaced by a better theory. Though all theories are conjectural, Popper argues that there can still be rational arguments for preferring one theory to another. Although all appraisals of theories are also conjectural.

Such appraisals are, in fact, appraisals of the state of the theory's critical discussion, according to Popper.[17] He argues that a better theory is one which has a greater empirical content, has greater explanatory and predictive power and which can therefore be more severely tested. It is a consequence of this view that a better theory is one which has a smaller probability. The appraisal of a theory starts, to some extent, before it has been tested in that, according to Popper, the criterion that a preferable theory has more empirical content can be seen as a criterion of 'potential satisfactoriness,' or 'progressiveness'. It can be said that, if a theory with high content passes certain tests, it will be preferable to a theory of lower content.[18]

Continued growth is essential to the rational and empirical character of scientific knowledge according to Popper. This growth occurs, as we have seen, through the repeated overthrow of scientific theories and their replacement by better ones. Furthermore, the critical examination of theories leads to attempts to test and overthrow them, and this can in turn lead to the development of new and original experiments and observations. This process of growth in scientific knowledge is the same as the process of growth in human knowledge generally according to Popper; that is, growth through trial and the correction of error. He presents three requirements which must be met for growth of knowledge to occur. These are that: a new theory should proceed from a simple new idea about new connections between things or facts or new theoretical entities; a new theory must lead to the prediction of phenomena which have not so far been observed; a new theory must pass a new and severe test.[19] So, for Popper, the growth of knowledge consists in the modification of previous knowledge and, as theories are replaced by better ones, with higher verisimilitude, that knowledge hopefully approaches nearer to the truth.[20]

It is clear that criticism is central to every aspect of Popper's philosophy of science. However, there are a number of problems with his model, of which two are particularly relevant in the context of a discussion of criticism. The first point, which is discussed by Lakatos[21], is acknowledged and accounted for to some extent in Popper's later development of his model, particularly in the area concerning the growth of knowledge. This problem concerns the relation between the falsification of a theory and its rejection. The problem is that vigorous criticism of theories through empirical facts is likely to lead to a situation where most theories have been falsified. In fact, most theories do have disconfirming observations accompanying them even when they are first proposed.[22] Thus, if criticism were rigorously pursued and all falsified theories were rejected, as a 'dogmatic falsificationist'[23] might demand, then science would be empty of theories. However, Popper is aware of this problem and admits that there is some role for dogmatism in science, "Somebody had to defend a theory against criticism, or it would succumb too easily, and before it had been able to make its contribution to the growth of science."[24] Furthermore a falsified theory should not be rejected until it can be replaced by a better theory. This is the point of Popper's later discussion of the growth of knowledge through theory replacement. So, while emphasising the role of criticism in science, Popper also recognised that criticism must be moderated.

The second problem, also discussed by Lakatos, is that presented by the so called Duhem-Quine thesis. According to Quine: "Our statements about the external world face the tribunal of sense experience not individually but only as a corporate body."[25] He goes on to say:[26]

> "But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to re-evaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole."

So that: "Any statement can be held true come what may, if we make drastic enough adjustment elsewhere in the system".[27]

In other words, when a particular test appears to falsify a theory, it is quite possible to save the theory from falsification by rejecting any one of a network of other theories, hypotheses and initial conditions surrounding that theory in the test situation. In fact Quine seems to be suggesting that modifications can be made anywhere in the total system of science if a favourite theory is to be saved from falsification.

The answer which Popper gives to this problem is a pragmatic one, based on moderating the amount of criticism in a particular situation. All criticism must be piecemeal, he argues, in contrast to what he calls the 'holistic' approach of Duhem and Quine. While Popper agrees that all our assumptions may be challenged, he argues that it would be impracticable to challenge all of them at the same time. Therefore, he argues, in every critical discussion we should specify our problem beforehand and stick to it, attempting to solve only one problem at a time. "While discussing a problem we always accept (if only temporarily) all kinds of things as unproblematic: they constitute, for the time being and for the discussion of this particular problem, what I call our background knowledge." [28]

While any part of the background knowledge may be challenged, to challenge it all could lead to the breakdown of critical debate, according to Popper. He argues that this reliance on taken for granted background knowledge creates no difficulty for the 'falsificationist' or 'fallibilist'. This is because it can always be challenged if it is not accepted and, although we cannot be certain that we have challenged the right bit, it does not matter because the quest is not for certainty. Furthermore Popper agrees that often only a large chunk of a theoretical system or sometimes the whole system, can be tested and, "In these cases it is sheer guesswork which of its ingredients should be held responsible for any falsification".[29] This does not worry Popper who argues that all theories are guesses anyway. He further argues that there are cases where it is possible to find which hypothesis is responsible for the refutation and gives the example of independance proofs of axiomatised systems. He concludes that the"..holistic dogma of the 'global' character of all tests or counter examples is untenable".[30] Finally Popper emphasises the need for success in the form of empirical corroborations of some theories as well as the need for refutation. He argues that it is only through these temporary successes that we can successfully attritute refutations to specific parts of the theoretical maze, thus contradicting the Duhem-Quine thesis. For Popper, science would stagnate if it did not obtain refutations but it would also stagnate if it did not obtain verifications of new predictions.

The above solution to the problem presented by the Duhem-Quine thesis represents what Lakatos would call a naive falsificationist position.[31] However, Popper's own work allows another solution which Lakatos would call the sophisticated falsificationist position. That is, when a theoretical system has been falsified and it is not clear whether it is the theory under test which is to blame for the falsification or if it is some auxiliary hypothesis or initial conditions, then the theory should only be rejected if a better theory is available to replace it. This second theory should be 'better' than the first in the sense that it obeys Popper's three rules which will ensure that the move is a 'progressive' one.[32]

So, in Popper's philosophy of science, we already see two aspects to the question of the role of criticism in science. First, criticism is essential, it is what provides science with its rational character. The very method of science is based on criticism and the growth of knowledge occurs through the critical revision of theories. Second, criticism must be moderated. Theories must not be rejected too quickly or science would never progress and, since the empirical base of science is fallible and since falsifying tests only provide us with a set of theories, auxiliary hypotheses and basic statements which together are falsified, we must be prepared to accept for the time being some basic statements and background knowledge without criticising them.

For Lakatos the problems of criticism and the moderation of criticism are also central. He starts with Popper's ideas on the growth of knowledge and develops what he calls a 'sophisticated methodological falsificationist' position.[33] Dogmatic falsificationism which admits the fallibility of all scientific theories, but which retains an infallible empirical base, claiming that science cannot prove any theory but can disprove a theory, Lakatos rejects. His grounds for rejecting this are that the two assumptions on which this position rests; that there is a natural psychological borderline between theoretical propositions on the one hand and factual or observational propositions on the other, and that if a proposition is factual or observational then it is true, are both false. First, there can be no sensations unimpregnated by expectations and therefore there is no natural demarcation between observational and theoretical propositions. Second, no factual proposition can ever be proved from an experiment, it can only be derived from other propositions. Therefore, the truth value of an observational proposition cannot be indubitably decided. It follows that, since factual propositions are unprovable, they are fallible. Thus clashes between theories and factual propositions are not 'falsifications' but merely inconsistencies. The conclusion which Lakatos draws from this is that, not only can we not prove theories but, we cannot disprove them either. Finally, exactly the most acclaimed scientific theories do not forbid any observable states of affairs and therefore would be eliminated from science by the dogmatic falsificationist. This is because these theories are usually accompanied by a _ceterus paribus_ clause when tested, according to Lakatos, so that any falsification can be avoided by replacing the _ceterus paribus_ clause. This is a version of the Duhem-Quine problem.

'Naive methodological falsificationism' which accepts that the truth value of singular basic statements cannot be proved but is decided by agreement, is also rejected by Lakatos. The problem which he sees with this view is that it has a strong conventionalist element. He identifies four levels of decision which must be taken as part of this methodology: decisions about what is the set of all basic statements; what is the set of 'accepted' basic statements; what rejection rules are to be used to render statistically interpreted evidence inconsistent with a probabilistic theory; and when is a refutation of the conjunction of a theory together with a _ceterus paribus_ clause to be taken as a refutation of the specific theory. Furthermore, according to Lakatos, both this view and that of the dogmatic falsificationist support two propositions that are contradicted by the history of science. These are that a test is a two-cornered fight between theory and experiment, and that the only interesting outcome of such confrontations is conclusive falsification. Whereas, the history of science suggests that tests are at least three-cornered fights between rival theories and experiment, and some of the most interesting experiments result in confirmation rather than falsification.

The answer to these problems with naive methodological falsification, in Lakatos's view, is to reduce the conventional element in it and to develop a 'sophisticated' version. This he does and bases this development on Popper's later work concerning the growth of knowledge. Here the crucial point is that theories should only be rejected if a better theory is available, and the criteria for what constitutes a better theory are that it should explain everything that the old one explained, it should make some novel predictions not made by the old theory and at least some of these novel predictions should be confirmed by experiment. These criteria ensure that the replacement of one theory by another is a 'progressive' move.[34]

Now, as the Duhem-Quine thesis reminds us, theories are appraised together with auxiliary hypotheses, initial conditions, and with other theories. So what is appraised is a 'series of theories' rather than isolated theories.

A new series of theories is 'theoretically progressive' if each new theory has some excess empirical content over its predecessor and it is 'empirically progressive' if some of this excess empirical content is also corroborated.[35] A 'problemshift' is 'progressive' if it is both theoretically and empirically progressive, and 'degenerating' if it is not.[36] For Lakatos problemshifts are 'scientific' only if they are at least theoretically progressive; those which are not can be designated as 'pseudo-scientific'. On this sophisticated methodological falsificationist position, "Contrary to naive falsificationism, no experiment, experimental report, observation, statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory"[37] (Here Lakatos is equating falsification with rejection). Because a new theory must be available for a falsified theory to be rejected, the notion of 'crucial experiments' can only be applied with hindsight. While an old theory may face a series of anomalies, only those anomalies which corroborate a successive theory while refuting the old theory can be seen as 'crucial experiments' and this can only be done when the old theory has been rejected and the new one has taken its place. Since experiments do not test single theories, but 'sets of theories', Lakatos concludes that "It is not that we propose a theory and Nature may shout NO; rather, we propose a maze of theories and Nature may shout INCONSISTENT."[38] To solve the problem of which of the mutually inconsistent theories to replace, the sophisticated falsificationist opts for retaining the theory which provides the biggest increase in corroborated content, while providing the most progressive problemshift.

As we have seen, Lakatos talks in terms of 'series of theories' and he argues that these theories are connected by a 'continuity' which makes them into 'research programmes'. The research programme consists of methodological rules which provide what Lakatos calls the 'negative' and 'positive' 'heuristics'.[39] The negative heuristic consists of methodological rules which tell the scientist what paths of research to avoid. At the heart of the research programme is a 'hard core' of theories which the negative heuristic protects by directing research and test results away from it. Around this hard core a series of 'auxiliary hypotheses' are built up which form a protective belt for the core. It is the auxiliary hypotheses in this protective belt which get rejected and adjusted to accommodate negative test results, while the hard core remains intact. To prevent this protection of the hard core from being completely ad.hoc.Lakatos employs the notion of progressive research programmes. If the adjustments made to auxiliary hypotheses,in the light of tests, continues to lead to a progressive problemshift, then the programme is successful. However, if this leads to a degenerating problemshift, then the programme is unsuccessful.

The positive heuristic sets out a research policy of problems to investigate. This research policy anticipates 'refutations' of the programme and suggests ways of developing or adjusting the protective belt of auxiliary hypotheses to save the hard core. Research is seen by Lakatos as consisting of a chain of models increasing in complexity with the positive heuristic providing the instruments for building these models. With a developed positive heuristic 'refutations' became irrelevant because they are fully anticipated and a strategy for coping with them is already laid out. Thus the real problems of a programme are mathematical rather than empirical. The positive heuristic is quite flexible according to Lakatos, so that even in a degenerating phase a creative shift in this heuristic can produce a progressive problemshift. In this model of scientific activity 'verifications' become important in keeping the research programme going.[40] Research programmes can be appraised, according to Lakatos, in terms of their 'heuristic power' which includes such elements as their ability to produce facts and to explain refutations.

It can be seen that, in Lakatos's model, research programmes are very robust activities with a great capacity for resisting and responding to criticism. The important question remaining is, when should a research programme be rejected? Here again Lakatos takes Popper's ideas on the growth of knowledge through the successive replacement of theories by better theories as a starting point. For Lakatos theoretical pluralism is vital for the growth of knowledge. A research programme should only be eliminated when there is a better rival programme to take its place. If one research programme can only be saved by a degenerating problemshift and a rival is available which represents a progressive problemshift, then the first should be rejected. If, however, the first programme would still be progressive, if not for the existence of its rival, then sufficient time should be allowed to elapse for the two programmes to compete. Here Lakatos stresses the need for methodological tolerance.[41]

Because of the need for tolerance, Lakatos rejects the notion of instant rationality, particularly as represented by the notion of crucial experiments. Since a 'falsified' research programme needs time to see if a progressive problemshift can be developed to save it, experiments do not lead to the instant rejection of research programmes. This means that experiments which are seen as crucial can only be seen as such with hindsight. Even then a research programme could make a comeback,in which case what was once seen as a crucial refutation could later be seen as significant supporting evidence. A more appropriate application of the term 'crucial experiments',according to Lakatos, would be the experiments which decide between different versions of theories within a particular research programme.

So Lakatos's view also centres on the notion of criticism, but emphasises the need for tolerance and a large degree of resistance to criticism. While his views are developed from Popper's work on the growth of knowledge the emphasis which he places on the role of research programmes in directing research, and their resistance to refutation bears some similarity to Kuhn's analysis of the notion of paradigms.

If Popper's view of science can be characterised by its emphasis on the central role of criticism combined with a recognition that criticism must, to some extent be moderated, Kuhn's view might be seen as giving a central role to the suspension of criticism combined with occasional instances of major critical activity during periods of crisis. To put it in Kuhn's own words: [42]

> "In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis, when the bases of the field are again in jeopardy."

Central to Kuhn's view is the notion of paradigms. A paradigm is shared by a community of scientists and serves to define the legitimate problems and methods of a research field. Two characteristics of a paradigm are that it must represent an unprecedented achievement so as to attract an enduring group of adherents, and it must be sufficiently open-ended to leave all sorts of problems to be solved. As Kuhn's views developed, he wished to emphasise the exemplary role of paradigms and, so, broke the original usage of the term 'paradigms' down into various components of what he called a 'disciplinary matrix'.[43] The members of a scientific community share a 'disciplinary matrix'. This matrix is composed of such elements as symbolic generalisations which express accepted laws in formal terms, or serve to define various symbols, metaphysical principles expressed in terms of models, values which help in judging predictions and theories, and exemplars. It is this latter category for which Kuhn sees the term 'paradigm' as being most appropriate. These are concrete problem-solutions which serve to show, by example, how the job of science is to be done. These accepted examples provide models of the type of laws, theories, applications and instruments to be used in scientific activity. The paradigm provides rules and standards of scientific practice. It is the differences between sets of exemplars that distinguish one scientific community from another. These exemplars are of prime importance for the maintenance of a scientific community. It is through exemplars that laws and theories acquire their empirical content and, through exposure to exemplars, that a student scientist is educated into the scientific community.

Until this paradigmatic stage is reached, that is, before a community of scientists share the elements of a disciplinary matrix, research is done in a number of disparate groups. These groups do not share a commitment to any body of belief and, so, each is forced to build the field from scratch. There may be little agreement between these groups, and poor communication between them. Furthermore, in the absence of a paradigm, all the facts which could possibly pertain to the development of a given science are likely to seem equally relevant. This means that early fact-gathering is nearly a random activity.[44] This pre-paradigm phase comes to an end when one of the pre-paradigm schools triumphs over the others, and achieves the status of a paradigm. When this occurs the field becomes more rigidly defined, as does the group of practitioners. The structure of this group changes in such a way that professionalisation occurs.[45] Effort is now concentrated on a limited number of problems. For Kuhn the acquisition of a paradigm is a sign of maturity in the development of a particular scientific field.

Once a paradigm is achieved, scientists become involved in what Kuhn calls 'normal science'.[46] This activity is concerned to elaborate the paradigm. It consists of developing and investigating the phenomenon which the paradigm is expected to explain. Normal science investigates problems in great detail and depth as the paradigm focusses attention on esoteric problems. Three types of experimental work which occur in normal science are described by Kuhn.[47] These are: investigating the class of facts that the paradigm has shown to be especially interesting; investigating those facts that can be compared directly with predictions from the paradigm theory; and empirical work undertaken to articulate the paradigm theory. This latter class is the most important according to Kuhn, and he identifies three activities within this class: the determination of physical constants; the discovery of quantitative laws; and experiments needed to chose among the alternative ways of applying the paradigm to a new area of interest where its application may be ambiguous.[48]

Normal science for Kuhn is a problem solving activity analagous to the solving of puzzles. This puzzles solving character of normal science follows from the fact that the paradigm identifies problems to be investigated and, to a large extent, the solution to these problems are anticipated in advance. Furthermore, in addition to the solutions being anticipated, the paradigm provides rules that limit both the nature of accepted solutions and the steps by which they are to be obtained. This means that normal science problems are puzzles in the sense that they test only the ingenuity and skill of the scientist in finding the appropriate solutions, in the same way that a jigsaw puzzle or crossword puzzle does. They do not test the paradigm or paradigm theory.[49]

While normal science problems do not test the paradigm 'anomalies' do begin to arise in the course of this activity. The emergence of experimental anomalies is followed by changes in theory or instrumentation, in an attempt to explain them. This process represents a scientific discovery and usually induces some changes in the paradigm.[50] Theoretical anomalies can lead to a breakdown in the normal puzzle solving ability of science. When this occurs, a situation of crisis has been reached, and only then will new theories emerge and be accepted.[51] Without this breakdown of the puzzle-solving activity anomalies can be accumulated and lived with without any challenges to the paradigm. Anomalies are not treated as counter instances.[52] A scientific theory which has achieved the status of a paradigm is not declared invalid unless there is an alternative candidate to take its place. This is shown, Kuhn claims, both by the history of science and by the fact that all theories face counter instances, so that if theories were rejected on the grounds of having counter instances, science would be empty. "The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves comparison of both paradigms with nature and with each other."[53] As a result of this scientists must, occasionally, be able to work in a situation of crisis as one paradigm is breaking down but before an altermative has emerged. This Kuhn refers to as, 'The essential tension', implicit in scientific research.[54] While paradigms are not immediately rejected in the face of counter instances, there is also no such thing as research without counter instances.

Not all anomalies lead to counter instances; some get resolved after a period of time. Those that do evoke crisis may do so because they call into question explicit and fundamental generalisations of the paradigm, or may inhibit applications which have a particular practical importance, or the development of normal science may transform an anomaly that had previously not been serious into a source of crisis. Often several circumstances will combine to make an anomaly, particularly significant and capable of evoking crisis. The situation which is brought about by these serious anomalies Kuhn refers to as 'extraordinary science'. In extraordinary science the anomaly becomes the centre of attention. If early attacks on the anomaly fail, eventually attacks will involve numerous different articulations of the paradigm. Through this proliferation of divergent articulations, the rules of normal science become increasingly blurred. According to

Kuhn two effects are universal in crisis situations: all crises begin with the blurring of a paradigm and consequent loosening of the rules of normal research; all crises close in one of three ways.[55] These three ways are, first, that normal science is able to handle the problem, second the problem resists but scientists decide to shelve it until better tools are developed for its solution, and third, a new candidate for paradigm emerges and a battle ensues over its acceptance. In this latter case, if a new paradigm eventually triumphs over the old one, then a 'scientific revolution' has taken place.[56]

When crisis leads to the emergence of a new candidate for paradigm, the two paradigms available will be conflicting because the new one will be able to explain the anomaly which has caused the breakdown of the old paradigm, which could not explain it. Furthermore, the two paradigms will be logically incompatible as one will make predictions that are different from those derived from its predecessor. As these paradigms are fundamentally incompatible, one can be accepted only by admitting that the other is wrong. One consequence of the paradigms' incompatability is that arguments in support of a particular paradigm can have persuasive force only. This is because any argument in support of a paradigm will presuppose the paradigm itself and will thus be circular. In a debate concerning the merits of two paradigms, the participants will talk past each other because they will disagree about what is a problem and what is a solution, as each paradigm sets its own criteria. Choice between competing paradigms is, "A choice between incompatible modes of community life".[57] Revolutions can be seen as changes of 'world view'.

For Kuhn these rival paradigms will be 'incommensurable' because: the proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve; the vocabulary and apparatus, both conceptual and manipulative, of the old paradigm become incorporated into the new one, but their relationships with each other change and they take on new meanings; the proponents of competing paradigms practice theirtrades in different worlds.[58] From this incommensurability a problem arises:[59]

> "Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the Gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all."

The question is how scientists come to make this switch and Kuhn points out that very often they do not. Many supporters of an old paradigm are never persuaded by a new one. The final triumph of a new paradigm may have to wait until the proponents of an older one have died.[60] Paradigm change cannot be justified by proof, so it is not surprising that many proponents of an old paradigm may never come to accept a new one. However, Kuhn does give some examples of arguments which can persuade scientists to adopt a new paradigm.

Some of the reasons which have converted particular scientists lie outside the apparent sphere of science.

The most effective ones Kuhn suggests are these: a new paradigm can solve the problems that have led the old one into crisis; the new paradigm permits the prediction of phenomena that had been entirely unsuspected while the old one presided and these are confirmed by experiment; the new paradigm may be seen as aesthetically superior to the old.[61] It is pointed out that even in the area of crisis, the balance of argument and counter argument can sometimes be very close. Choosing between two candidates for paradigm must be based less on past achievement than on future promise, since the successful candidate must act as a guide to future research on problems not yet encountered. The decision must be made on faith, although that faith must have some basis whether it is correct and rational or not.[63]

Science progresses, both in the normal situation and through revolutions. "In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define. Furthermore, the result of solving these problems must inevitably be progress".[63] Revolutions are seen as progress, partly because there is a certain amount of re-writing of history to present the new paradigm in a favourable light. Genuine progress is also made though. "The scientific community is a supremely efficient instrument for maximising the number and precision of the problems solved through paradigm change."[64] For Kuhn the unit of scientific achievement is the solved problem, so scientists will not adopt a new paradigm if it re-opens many of the problems which have previously been solved. Furthermore, for a new paradigm to be accepted two conditions will have to be met. The new candidate must be seen to resolve some outstanding, and generally recognised, problem that cannot be solved in any other way. The new paradigm must preserve a relatively large part of the concrete problem-solving ability that has accrued to science through its predecessors.[65] New paradigms tend to narrow the focus of research and, in this case, science grows in depth but not in breadth through revolutions. If it does grow in breadth, this is through the proliferation of scientific specialties, not through the growth of any single specialty. Finally, Kuhn rejects the notion of progress through paradigm change as leading closer and closer to the truth. Science progresses but not towards any particular goal. In this sense it is analogous to Darwinian evolution. In the view of science developed by Kuhn, we again see the two themes of criticism and moderation of criticism. Here the emphasis is on periods of science characterised by the suspension of criticism. However, these period are interspersed with intense critical activity. One of the criticisms of Kuhn's view is that he implies that revolutions are rare events, whereas it may be argued that they occur frequently.[66] Thus, even a view which accepts Kuhn's notions of 'normal' and 'extraordinary' science could give a major role for criticism. Another criticism is that he identifies two characteristics of science, criticism and dogmatism, with specific periods, whereas it may be argued that these characteristics are co-present in science.[67]

In his early writings, Feyerabend's position owes a lot to that of Popper.[68] He emphasises the need for criticism in science and advocates theoretical pluralism as an essential feature of the methodology of science. He argues that models which represent confirmation and testing in science as involving the comparison of a single theory with a set of facts are too simple. These models assume that facts are relatively autonomous from the theory being tested whereas, according to Feyerabend, facts and theories are intimately connected. The description of any single fact is dependant on some theory, and facts exist which cannot be unearthed without the help of alternatives to the theory being tested, in Feyerabend's view. Furthermore, the relevance and the refuting character of many very decisive facts can be established only with the help of other theories which, although factually adequate, are yet not in agreement with the view to be tested. Testing and assessment of the empirical content of a theory necessarily involves, "A whole set of partially overlapping, factually adequate, but mutually inconsistent theories."[69]

These theories must be inconsistent because an inconsistent alternative theory will provide a more efficient criticism of a theory under test. Feyerabend criticises two principles which he attributes to traditional empiricism, the 'consistency' principle and the principle of 'meaning invariance'.[70] Briefly, these state that when one theory is reduced to another, the two theories must be consistent and the meanings of terms in the theories must remain constant. According to Feyerabend, these principles will lead to a dogmatic metaphysics completely removed from any criticism. The alternative is to propose inconsistent theories which enable currently accepted ones to be rigorously criticised. As Feyerabend puts it, (in rather colourful language!),[71]

> "Unanimity of opinion may be fitting for a church, for those frightened victims of some (ancient, or modern) myth, or for the weak and willing followers of some tyrant; variety of opinion is a feature necessary for objective knowledge; and a method that encourages variety is also the only method that is compatible with a humanitarian outlook."

For Feyerabend, then, criticism is vital for objective knowledge and a plurality of theories is vital for effective criticism. In summary he describes 'How to be a good empiricist':[72]

> "In the last resort, therefore, being a good empiricist means being critical, and basing one's criticism not just on an abstract principle of scepticism but upon <u>concrete suggestions</u> which indicate in every single case how the accepted point of view might be further tested and further investigated, and which thereby prepare the next step in the development of our knowledge.
> (Emphasis in the original).

According to Feyerabend his own views on the need for alternatives in the criticism of a theory are consistent with those of Popper and Kuhn.[73] On the subject of the need for a certain amount of dogmatism or 'tenacity' with respect to theories, Feyerabend agrees with Lakatos that criticism and tenacity are always co-present in science. As we saw before, he criticises Kuhn's view as mistakenly postulating periods of tenacity, followed by periods of criticism. For Feyerabend, mature science unites two different traditions, one of pluralistic philosophic criticism and a more practical tradition which explores the potentiability of a given material without being deterred by the difficulties that might arise and without regard to alternative ways of thinking.[74]

While accepting that criticism and dogmatism are co-present in science, Feyerabend goes on to criticise Lakatos's view as not being an accurate account of science as it is practiced, and as being inadequate even as a 'rational reconstruction' of science, or an analysis of 'world 3'.[75] Ultimately, Feyerabend is led to argue that there is no methodology in science as it is practiced. Science, he argues, is an essentially anarchistic enterprise and theoretical anarchism is more humanitarian and more likely to encourage progress than a science built on methodological rules.[76] According to Feyerabend the history of science shows that there are no rules in science which are universally adhered to, and that violations of what have been taken to be rules have produced many of the most significant developments in science. In addition to this, ideas do not always precede action but can develop through action, so that problematic aspects of particular theories can come to be accepted as clear and reasonable only after they have been used for a long time.[77]

To illustrate his maxim that 'anything goes', Feyerabend attempts to show that hypotheses can be used which contradict well-confirmed theories or well-established experimental results. This is an extension of his theoretical pluralism. For Feryerabend knowledge is not a series of self-consistent theories gradually approaching the truth, but is an, "Ever increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives."[78] The importance of alternative theories, hypotheses and assumptions, for Feyerabend, stems from the need for an external standard of criticism. In Against Method he restates his criticism of the consistency condition, arguing that this pres erves old theories not necessarily better theories. Hypotheses contradicting well-confirmed theories, provide evidence which cannot be obtained in any other way. "Prolification of theories is beneficial for science, while uniformity impairs its critical power. Uniformity also endangers the free development of the individual."[79]

As an example of the need for alternative theories, Feyerabend argues that the alleged success of Quantum Mechanics is a man-made phenomenon reflecting not success but the elimination of alternatives and of problematic facts that can be discussed with their help. This means that the empirical content of the theory has decreased and it has become a rigid ideology protected from criticism, according to Feryerabend. Criticism is, then, essential in Feyerabend's view of science, and his earlier theoretical pluralism and later anarchistic philosophy is founded on the need for critical assessment of theories.

The important role given to criticism in these various philosophical views is, of course, in stark contrast to the view of science held by traditional logical empiricists. This view is important for, while few people today would admit to holding it, the view represents an ideology on which the authority of science in the policy arena is based. So, while no one explicitly advocates this view, assumptions about the usefulness of science and the authority of scientific experts rest on an implicit acceptance of aspects of the positivist model of science.

The foundation stone of the logical empiricists' view is the belief that sentences referring to experience are fundamental, so that experience provides the bedrock of all knowledge. The empiricists, concerned to distinguish science from metaphysics, proposed a demarcation criterion based on meaning. Science consisted of meaningful statements whereas metaphysical statements were meaningless. This distinction was, in turn, based on the principle of verifiability. A meaningful statement was verifiable on the basis of fundamental empirical statements. For example, in Wittgenstein's Tractatus all meaningful statements must be verifiable on the basis of 'atomic sentences' which picture elementary facts.[80]

For the empiricist the aim of science is the search for truth in the form of natural laws which are universal statements verified by observation, represented by observation statements. The two concerns of the empiricist are, then, that knowledge should be secure (it should be truth), and it should be justifiable (through empirical verification). The firm foundation which scientific knowledge rests on is experience.

This empirical base is represented by observation statements and the problem is what justification have we for accepting observation statements as true. If another statement is demanded to provide evidence for an observation statement then a problem of infinite regress arises because a further statement is required to provide evidence for that statement and so on. It is to solve this problem that the view that empirical statements are fundamental is proposed. Certain statements about our experience are said to exist which need no justification. The main problem with this view is that it seems impossible to characterise a sentence about experience which needs no justification in the form of evidence from other sentences. Simple empirical assertions such as, 'There is a table in this room', are not indubitably true; evidence for this type of statement can always be called for.

To avoid this problem Carnap and Neurath developed the idea of 'Protocol sentences'.[81] These are statements about assertions taking the form, 'A at time t and place P, B has perceived such and such', where A and B refer to specific people. But, even these statements are open to the above criticism; one can still ask how does A know that P perceived such and such? If the reply is that he made an observation to that effect, the whole problem of justifying the assertion arises. Another problem with fundamental empirical statements is that they speak only of subjective experiences. Strictly I can only justify a fundamental sentence about my experiences which I am having at the moment. I cannot accept other people's statements of experience as transparently true and neither can I trust my own memory. This leaves a very narrow base for knowledge.

For the empiricists the method of science is induction. Following Bacon's model, scientific activity consists in collecting observations and deriving universal laws from them. These universal laws are verified by the singular observation statements. The problem with this method is that inductive inferences are not logically valid; no finite number of singular observation statements can logically verify a universal statement. Recognising this problem of induction some empiricists, Carnap for instance, proposed a theory of logical probability.[82] They maintained that an observation statement which confirms a universal law, confers a degree of probability on that law. Carnap proposed that this probability was quantifiable, so that a theory T could be said to have a degree of probability P on the basis of evidence e. However, this notion of a degree of probability being conferred on a theory through induction still faces the same problem of how the inductive inference is justified.

For the empiricist science grows by the accumulation of observations from which new laws can be inferred and old laws verified. Science is thus seen as a cumulative process of collecting knowledge in the form of observations and laws. Scientific knowledge grows primarily in quantity, but the foundation of knowledge is always sure. It can be seen that there is little place for criticism in this view. Observation statements are fundamental and therefore not open to criticism. Laws are inferred from these observations with the help of theories and further observation verifies the validity of these laws. With a probability calculus to define the level of confirmation of a theory, there is little ground for criticism of theories. Finally, progress is through the cumulative growth of knowledge.

4.2 Models of Criticism.

While there are differences among the post-positivist philosophical views discussed above on the relative importance of criticism and dogmatism in science, they all give these two aspects a central role to play in the development of knowledge. For Popper scientific activity is critical activity. Criticism is what makes science rational and enables knowledge to grow. For Lakatos criticism is similarly important for the growth of knowledge, both in the form of critical re-adjustment of the protective belt and in the replacement of research programmes with better alternatives. For Kuhn criticism is necessary when normal science has broken down and a paradigm has ceased to perform its puzzle-solving function adequately. In this case criticism must come from a rival paradigm. Furthermore, paradigms emerge in a science initially through a critical battle with a range of alternative candidates. For Feyerabend criticism is essential in science and rival theories are necessary to make this criticism possible. Dogmatism is also essential in science. In Popper's view falsified theories must not be immediately rejected; this should only occur when a better theory is available as replacement. This is taken a stage further by Lakatos who argues that the core of research programmes must be protected from falsification by a protective belt of auxiliary hypotheses and initial conditions. Research programmes should be rejected only if they are no longer progressive and if a better programme is available as replacement. For Kuhn dogmatism is central. The main aspect of scientific activity presupposes the acceptance of a paradigm which is, once accepted, immune from criticism so long as it continues to enable puzzle-solving to continue. Criticism only occurs in pre-paradigmatic science and in extraordinary science.

The need for dogmatism in science, that is, the need to restrict or resist criticism, implies that there can be too much criticism. Since the empirical base of science is fallible, according to all of these philosophical viewpoints, unrestrained criticism of experimental results or 'basic statements' could lead to a problem of infinite regress. Furthermore, since all theories have at least some disconfirming instances, unrestrained criticism here would leave science empty of theories. Similarly, the need for criticism suggests that there can be too little criticism. If a paradigm, research programme, or theory was accepted without having been compared critically with alternatives, then it may not be the best available, it may not represent any advance or growth of knowledge, and according to Feyerabend, its empirical content would be low.

While too much criticism may lead to stagnation and the breakdown of the ability of science to solve problems, too little criticism may produce problem solutions of dubious value.

In science, then, there can be too-much or too-little criticism. We might divide 'over-criticism' into four categories, 'destructive criticism of theories' being the criticism of a theory/research programme core/paradigm theory without the proposition of an alternative. For Popper a theory should not be rejected without a successor being available, for Lakatos a research programme should not be rejected without a successor being available, and for Kuhn a paradigm should not be rejected without a successor being available. Characterising this activity as over-criticism would seem, therefore, to be compatible with the various philosophical models. 'Unresolved criticism of theories' occurs when a critical debate occurs between proponents of alternative theories but no one theory emerges as victor. This may have various results. The field may fragment with rival groups becoming clearly differentiated. Or both theories may be accepted even though they are incompatible. Finally, the result which allows the term 'over-criticism' to be applied to this category of phenomenon is when the critical debate becomes endless, with no resolution, so that problem solving breaks down and knowledge is not able to grow. 'Destructive criticism of experiment' occurs when experimental results are criticised and rejected without proposing either alternative experimentsor alternative theories to the one supported by the rejected experiment. 'Unresolved criticism of experiment' occurs when, even though alternative theories and experiments are proposed, no experimental results emerge as acceptable in a particular field. As we have seen, the empirical base of science is fallible and the specific details of any experiment can always be questioned. If no experimental results are accepted by a community of scientists, then again the problem solving ability of science will break down.

The problem which now has to be faced is that of identification of these four types of over-criticism. Destructive criticism of theories or experiments should be possible to identify. These occur when criticism is not accompanied by the proposal of an alternative. Unresolved criticism of theories or experiments will be more difficult to identify. The problem is that, to avoid stagnation or infinite regress, agreement must be reached at some stage on what theory or experimental result to accept. However, it is impossible to say at what point in a debate certain results or theories should be accepted. The Sociology of Science studies discussed earlier show that thisacceptance is negotiated and will therefore take different lengths of time to achieve in different cases. This means that, while a debate may appear to be taking a long time to reach a point where acceptance of a particular theory or experimental result is achieved, this does not mean that such a resolution will never be reached. All that can be said is that over-criticism has produced a long debate which appears to have stagnated, but which may resolve at some future date. Even with this formulation, the problem still remains of what is, and what is not, a long debate. In a sense the answer to this question depends on what hangs on the resolution of the debate. If no progress can be made in a field at all until the debate is resolved, then a period of more than a few years would be a long time to wait for resolution. However, if the particular experimental result or theory was peripheral to the major concerns of a field, then even twenty or more years may not be considered a long resolution time. This way of looking at the problem suggests that the way to assess whether a situation of unresolved criticism is present is to identify the effects of the lack of resolution.

Criticism is important for the assessment of claims to knowledge. If a particular theory or paradigm is accepted without being subjected to criticism, in the light of an alternative theory or paradigm, then its contribution to knowledge will be uncertain. On Kuhn's view a paradigm only emerges after winning a critical battle with either a group of other pre-paradigmatic schools or theories, or with an earlier paradigm. This battle, while not establishing the paradigm's adequacy and value on logical grounds, does serve the function of convincing scientists of which is the better paradigm candidate. Research programmes face criticism at two levels in Lakatos's view; experimental criticism of the protective belt and criticism of the hard core by comparison with alternatives. Without these two aspects of criticism the value of a research programme will be poorly assessed. For Feyerabend, alternative theories are crucial to the critical assessment of the empirical content of theories. For Popper empirical criticism is the important test of value, but when a theory has been falsified it must eventually face critical assessment in the light of alternative theories. Thus, for the value of a theory to be fully assessed, it must fact critical comparison with alternative theories, though, in Popper's view, some assessment is possible through straightforward empirical test.

The identification of a situation of under-criticism will therefore concentrate on the existence of alternative theories with which the accepted one can be critically compared. It was seen above that Feyerabend interprets the development of Quantum Mechanics (QM) as having led to a situation that is here described as 'under-critical'. He argues that, to the extent that QM has not faced criticism from rival theories, it has become a rigid ideology with low empirical content.[83] It was also seen earlier in the sociological study by Harvey, of a challenge to QM, that this theory had an 'easier ride' than it might have done had there been a rival group to vigorously pursue an alternative theory.[84]

While over-criticism will produce stagnation and an inability to solve even the problems which science sets for itself, under-criticism will result in poor assessment of the value or adequacy of claims to knowledge.

4.3 Industrialised Science and Practical Problems.

In addition to the specific problems associated with particular philosophical views of science, there are a number of problems common to all of the ones so far considered. The main problem is their rather narrow focus on a particular type of scientific activity. These models tend to focus on academic science and on the formation and development of theories. They also tend to take physics as their model of scientific activity. However, the scientific community is widely differentiated along both cognitive and occupational lines, such that the activities of scientists in different disciplines and specialties may be very unlike those of the physicist. While many scientists may work in an academic context there are many more who work in industry and government contexts.[85]

The role of science in industry and government contexts has become particularly significant in the period following the Second World War. Developments in this era have been characterised by Ravetz as the, 'Industrialisation of science'.[86] He contrasts 'industrialised science' with 'academic science' which existed in the period following the French Revolution and up to the Second World War. He argues that the technical character of scientific work has changed, and with this change has come changes in its social institutions and social practices. He warns that there is a danger of the ideology of academic science being preserved in, "A fossilised state for particular public-relations functions, while becoming less and less relevant to the experience of those who live in the world of industrialised science".[87] Academic science was performed by an autonomous community of gentlemen adhering to the ethic of searching for the truth, assuring diffuse social benefit and whose intellectual property was represented by the paper published in a refereed journal. Industrialised science has many different characteristics to this old ideology of academic science, as Ravetz points out.

The most significant change in the technical character of scientific work, according to Ravetz, is that research is now capital-intensive. This has meant the loss of independance of scientists who must now apply to an institution or agency that distributes funds for research before his work can begin. This has led to the existence of three roles which a scientist can fulfil: an employee working under the control of a supervisor; an individual outworker, working for investing agencies and existing on a succession of small grants; a contractor, managing a unit or an establishment which produces research on a large scale by contract with agencies. With the differentiation of positions of individual scientists has come a concentration of power to make decisions and the development of a formal administrative system for this function according to Ravetz. He argues that, under the new system, the location of intellectual property has changed from the published research report to the research contract. Accompanying this shift in property is a shift in the conception of a successful career. Ravetz describes as a 'scientific entrepreneur' someone whose career goal consists, not of a series of successful research projects made possible by a parallel series of adequate grants, but of a series of successful research contracts made possible by a parallel series of adequate projects.[88] In this new context the scientific community cannot survive in its old form, and with differences in wealth, prestige, power and material benefits, Ravetz suggests that it is possible to speak of classes in a society of science.[89]

Industrialised science has brought with it an adulteration of the products of research, according to Ravetz.[90] The 'information crisis' is a quantitative problem in that the number of journals is so large that no scientist can keep up with the literature, which can lead to the duplication of research. It is also a qualitative problem in that less formal channels of communication develop, which avoid the critical scrutiny of referees. Another phenomenon Ravetz describes as, 'shoddy science'. The majority of journals in many fields are full of papers which are never cited by an author, other than their own, and many of these papers are dull or bad. They get published because it is in the interest of the author, an editor and a publisher that they should be.[91]

Another phenomenon is the penetration of science by industry. The research and development function has become an industry in its own right, in which a large proportion of the 'scientific community' work, employed by both industry and the State. Furthermore, scientists can have a whole variety of roles working in university, the State and industry, and they can lead to conflicts of interest. With large quantities of funding available for work which is mission oriented even that performed in universities may have strong practical orientations. According to Ravetz, a number of pathological phenomena occur in industrialised science as a result of the close contact between science and industry being strongest in areas of what he calls 'runaway technology'.[92] These are the areas with the most modern, rapidly developing technologies, where innovation depends entirely on large-scale, sophisticated 'R & D'. In this R & D industry the work ethics of scientists derive from industry, private and state-supported, rather than from academic science. Here entrepreneurical science and shoddy science occur when a contractor develops a big enterprise which is performing mission oriented research in a field where money is plentiful and control is not rigid. 'Reckless science' occurs when the influence of runaway technology is to create a drive to produce new technical powers whose dangers are brushed aside or seen as the responsibility of others.[93] Finally, 'dirty science' occurs when scientists are engaged on projects, "Whose intended application lies beyond the pale of civilized practice and morality".[94] This occurs particularly in the development of military technologies such as atomic, biological and chemical weapons, according to Ravetz.

Accompanying all these problems of industrialised science are serious problems of both morale and morals which confront modern science, according to Ravetz. The traditional image of the noble scientist doing work of the highest moral status and offering the products of his work freely has become outdated and is being replaced by an image of scientists as scientific manpower. As Ravetz puts it, "But such manpower units cannot be considered as scholars, and any propaganda that projects the image of the typical scientist as an independant searcher, following his own path in the exploration of Nature, is now worse than false: it is a bore."[95]

This analysis by Ravetz of the context and problems of modern science serves as a useful reminder that the traditional philosophical views of science tend to have a narrow focus and frequently ignore the context of industrialised science. We have seen so far that, in apparently esoteric areas of academic science, controversies can arise. The sociology of science studies have shown the importance of tacit knowledge in the conduct and conclusion of these controversies. They have further shown that the mechanisms of closure of scientific controversies depend on a process of negotiation. Philosophical models of science have shown the importance of criticism as a part of scientific activity and the equal importance of mechanisms to moderate this criticism. Even within academic science it is possible that over-criticism and under-criticism may occur. What does this mean for science which is related to policy issues? Before proposing an application of the models so far developed in a context of esoteric science to the areas where science has policy implications, it will be useful to borrow from Ravetz some definitions which he makes of the various types of problem in which science can be involved.

In his analysis of modern science, Ravetz makes a threefold distinction between the types of problem in which science can be involved. These are 'scientific problems', 'technical problems' and 'practical problems'.[96] He makes the point that, in one sense, the activities aimed at solving any of these problems could be called 'science' in that their work consists of problem-solving on intellectually constructed objects and there is no clear demarcation between the methods used in any of these situations. The important distinction between these areas of modern science does not lie in the character of the activities themselves, but in the relation of these activities to their goals, functions, and purposes.[97] These categories are defined as follows: the task has a goal which is conditioned more or less strictly by the function which will be performed by the result of the accomplished task; this is in turn governed by the ultimate human purposes which are expected to be served by the performance of that function.[98] For the purposes of the distinction between the different types of problem Ravetz concentrates on 'objective' final causes, ignoring the 'subjective' ones which include the individual scientist's own personal purposes for performing the task. To make the distinction Ravetz identifies the task itself as the investigation of the problem and the goal of the task as the solution of the problem. He then asks to what extent are 'functions' and 'purposes' involved in the specification of the goals of this sort of task.

The goal of 'scientific' work is the establishment of new properties of the objects of inquiry, and its ultimate function is the achievement of knowledge in its field.[99] In this case higher final causes are not so influential. The function to be performed by the solved scientific problem, that is the contribution of new results for the advancement of the field, conditions the work only in a general way, through the controlling judgements of adequacy and value. The investigation of the problem may change course in mid-stream and, in this case, the solved problem will perform a different function. The ultimate purposes to be served by the 'scientific' problem are quite remote, diffuse and unpredictable in detail.[100]

For 'technical' problems the function to be performed specifies the problem itself.[101] The goal of the task is fulfilled, and the problem solved, if and only if the function can be adequately performed. However, the purposes to be served only condition the work on a technical problem in a general way. Technical problems may be concerned with producing a device or a commercial product, or they may represent subsidiary problems within scientific research itself, for example the making of tools. Technical problems have less freedom to evolve than scientific problems because the function which is externally assigned cannot easily be altered if some unexpected possibility or difficulty appears in the course of research. The function is also specified in a more detailed fashion with each of its aspects being assigned some standard of adequacy to perform. With the function and criteria of quality assigned externally, practitioners of technology do not have the freedom which scientists have to ignore aspects of a problem that do not suit their tastes.[102]

An example which Ravetz gives of a technical problem is the creation of a new model of aeroplane.[103] A particular function will be identified for the plane to perform and a technical problem exists when a model has been proposed and this is defined in detail in terms of the operating characteristics which the model is expected to have. It is emphasised by Ravetz that, while the ultimate purposes and the immediate functions of technical problems are very different from those of scientific problems, the goals of individual tasks can be similar, and the methods of work nearly identical.[104]

According to Ravetz, while scientific results can give rise to technical problems and technical problems can give rise to subsidiary scientific problems, it would be dangerous to the integrity and survival of science if it were forced to get its inspiration exclusively from the problems posed to it by technology.[105] This is because the creation of a new scientific problem cannot be managed in an institutional manner as neatly as the exploitation of results.

Quality control mechanisms for technical problems are different from those of science.[106] The adequacy of technical solutions is judged in relation to performance and the value of the work is assessed in terms of the benefits accruing to the person or organisation sponsoring the work. In contrast, scientific solutions are judged by criteria of adequacy set by the scientific community and aimed at judging the adequacy of the argument and of the evidence involved in a piece of scientific work. The criteria of value by which scientific problems are judged assess the contribution of the solved problem to the advancement of knowledge of the object of inquiry in the field, and its contribution to the solution of problems outside the given field. Additionally, the personal value which the problem solution represents to the individual scientist performing the work may be considered.

Practical problems are the most complex and difficult to solve of the three types of problem which Ravetz describes. They are also the ones of most interest here. In this class of problem, the ultimate purpose of the task determines the goal. They are defined as: "A statement of a purpose to be achieved, whose means are to be established as the conclusion of an argument, with a plan for its accomplishment".[107] The problem is brought into being by the recognition of a problem-situation that some aspect of human welfare should be improved, according to Ravetz. These problems involve intellectually constituted objects and can give rise to subsidiary technical and scientific problems. Their solution, Ravetz claims, will usually require a large-scale project. He identifies a cycle of investigation through which practical problems progress, consisting of five phases: definition; information and argument; conclusion and decision; execution; and control.[108] The information and argument phase will consist of an inquiry of a scientific sort whose results will form the basis for a decision on the best method for achieving the defined purpose. The crucial difference between this cycle of investigation and those of technical and scientific problems is the way that ultimate purposes determine the goal itself in a practical problem, while in science they remain remote and diffuse, and in technology become just part of the criteria for controlling judgements.[109]

The definition of the problem in the practical case has a significant effect on the later stages of the investigation and is different to problem definition for scientific and technical problems. In science setting the problem involves the tentative specification of a conclusion about, 'Artificial objects in a self-contained universe', according to Ravetz,[110] and, for a technical problem, it involves imagining a device to perform a pre-assigned function. For a practical problem, the specification is of a state of affairs in human society which does not yet exist.[111] All of the controlling judgements in a practical problem involve a multiplicity of factors which is difficult to assess. An important point which Ravetz makes is that the statement of the goal of the problem, and of the controlling judgements, presupposes a social and moral philosophy or an ideology. This ideology will then influence and limit all the later phases of investigation, argument and decision.[112]

As Ravetz points out, for a practical problem there is no consensus on the criteria by which quality or success are judged. In science the members of a small community associated with a field usually develop appropriate criteria for their work. This process is illustrated in the sociology of science studies discussed earlier. In technical problems the criteria are supplied by the objective test of performance of function, and of commercial success. Since practical problems affect a variety of people in a variety of ways, however, agreements on criteria for assessing the problem solution will be elusive. With radically different perspectives on the problem, different groups may even interpret the words used in the description of the problem in very different ways, and debates over practical problems will inevitably lead to participants talking at cross purposes. 'Facts' will be particularly elusive in this type of problem, according to Ravetz. He suggests that it would be naive to believe that 'scientific method' can be applied in a simple and straightforward way to this class of problem.[113] This is a conclusion which prefigures some of the argument which will be presented in the present study.

The role of various professionals in the solution of practical projects is discussed by Ravetz. These include 'technicians', 'practitioners', 'experts', and 'consultants'.[114] 'Technicians' and 'practitioners' perform routine tasks, the former being involved in detailed work of inspection and regulation and the latter serving individuals to improve their welfare, according to Ravetz's account. 'Experts', in Ravetz's terminology, are not restricted to routine tasks, but study genuine problems, involving the exercise of judgement and the drawing of a conclusion from an argument. Working for a client, they may suggest a decision and leave the execution to him, or they may undertake that work as well. The expert, for Ravetz, is distinguished from the 'true professional' in that he is an employee of a firm, accounting to his superiors there who have the power of penalty and reward over him. His answerability to his clients is very slight, and to his colleagues in his specialism not much greater.[115] This is the crucial difference for Ravetz between the 'expert' and the 'consultant' who is an independant agent ostensibly offering the same service as the expert.

As Ravetz points out, when disputes arise over the practical aspects of some types of technical problem, where a person or group consider their welfare to be neglected or impaired, experts are called in by both sides. In this case the role of the expert is like that of an advocate, according to Ravetz, although they do not have the sophisticated etiquette and ethic of the legal profession, where a man can argue for his client without losing his own integrity.[116] The expert tends to argue as a scientist, establishing his conclusion on supposedly known and irrefutable facts. A serious problem arises from this role of experts which Ravetz identifies as the need for decision makers to place some reliance on experts' conclusions and recommendations, when the reliability of these are subject to severe limitations.[117] A development which is highlighted by these situations is the way that problems, once seen as technical, have begun to be seen in their practical aspects. Examples of these, which Ravetz provides, are technical developments which produce pollution or which intrude on the human environment in other ways.

In comparing technical with practical problems, Ravetz contrasts the great successes achieved with technical problems with the failure at solving practical problems. An example of technical success which he gives is the space race. Here, the ultimate purpose, the enhancement of national prestige, was simple and established by decree of national leaders. From this purpose the choice of goals was also simple and work could proceed on purely technical problems. Practical problems are unlike this, however, in that they cannot be reduced to a matter of technique. Significantly, Ravetz argues that, "The deepest and most urgent practical problem-situations are not discovered or invented; they are presented to us, frequently against our desires, by the processes of human history acting through time up to the present."[118]

The solution of practical problems is fraught with a multiplicity of 'pitfalls', according to Ravetz, and the danger from these is that the purposes served by the solution can turn out to be quite different from those intended.[119] He gives some examples of pitfalls characteristic of this type of problem. A problem in the inquiry stage is that, if the conclusions of the inquiry are simple, they are likely to be over-simple and, if they are subtle and detailed they are likely to be inappropriate as the basis for a decision. Furthermore, the inquiry stage can be conducted within the framework of a particular ideology which may result in the conclusion being determined before the work even begins. Another pitfall in this early phase is the imposition of excessively high standards of adequacy on the work, so that a lengthy and expensive research programme is needed before any decisions can be made. In a large practical project one pitfall that can arise occurs when the project is broken down into a multitude of routine tasks, governed by a hierarchy of decision and control. The problem here is that the aggregation of tasks, as they are articulated and then controlled by a bureaucracy, may come to be governed by goals which are contrary to the original function.[120] Finally, Ravetz argues that there are some practical problems which are simply incapable of solution.[121] Practical problems, the role which science plays in them, and the effect which they have on that science, are the concern of this study.

4.4 Criticism in Policy Relevant Science.

When scientific work is closely related to policy issues, for example when concerned with the solution of practical problems, the effect that policy implications may have on the level of criticism involved in the science may be very significant. It has been pointed out, by Ezrahi, that there can be various degrees of agreement within the respective groups of scientists and policy makers.[122] Four possible situations are identified: agreement on policy objectives with scientific consensus; agreement on policy objectives without scientific consensus; scientific consensus and disagreement about objectives; disagreement about objectives coupled with scientific dissensus. The aim of Ezrahi's discussion of these various situations is to show that the roles and uses of scientific knowledge in public policy will be different in each of them. This point will be further discussed when considering the role of expert advice in the policy process (cf.Chapter 5). An important point which Ezrahi makes is that for any particular issue shifts can occur between these four possible situations of consensus and dissensus within the two groups. Thus it is argued that the issue of the use of intelligence tests in education policy in the USA shifted from a situation of agreement on both policy objectives and the scientific validity and interpretation of tests to a situation where the objectives of applying intelligence tests became controversial and, finally, to one where the consensus on educational objectives, as well as that concerning the validity and interpretation of tests, broke down.

The four situations which Ezrahi describes can be related to the concepts of over-criticism and under-criticism developed in this study. In a situation where there is dissensus over policy objectives and dissensus amongst scientists, experts will tend to become advocates for particular policy options, using technical arguments to defend their positions, as Collingridge's model describes.[123] (cf.Chapter 2 above). In this situation, experts with rival institutional backgrounds will be highly motivated to 'win' the debate. Furthermore, as 'practical problems' almost invariably transcend the boundaries of scientific specialties, all the problems of cross-specialty comparison and criticism described earlier will arise. With experts from different cognitive and institutional backgrounds supporting different policy options, and backed up by agencies with vested interests in these different policy options, a situation of over-criticism will arise. Theoretical and experimental aspects of each side's case will be subjected to an inordinately high level of criticism.

This criticism may be destructive in the sense that while one theory is vigorously criticised, no alternative theory is proposed. Or it may appear to be constructive, based on the advocacy of an alternative theory. Even in this case, however, over-criticism can lead to chaos and stagnation through the problem of 'unresolved criticism' as described above. With both sides equally highly motivated, both theories may be subjected to an excessive level of critical scrutiny and, since all theories have some counterinstances and all experimental results can be questioned, the debate may stagnate with neither theory reaching a state of acceptance by both sides. This problem may be further exaggerated by the proliferation of theories leading to chaos as they are all vigorously criticised.

As consideration of findings in the sociology of science and the philosophy of science shows, in esoteric areas of science some empirical data must be accepted and some theory taken as the best available to allow problems to be solved and science to progress. However, in policy relevant science these agreements are unlikely to be achieved without a prolonged and agonising battle, if they can be achieved at all. Since the mechanisms for controlling criticism are social, as the sociology of science studies discussed earlier show, they will be difficult to apply where scientists have different cognitive and institutional backgrounds. Add to this high levels of financial and other support for rival views by agencies with vested interests in rival policy options, and these arguments will be very elusive. See Figure 1.

Under-criticism will occur when there is a consensus on what policy options to adopt. This will happen in two of the situations which Ezrahi describes, where there is consensus on policy objectives combined with scientific consensus and where there is consensus on policy

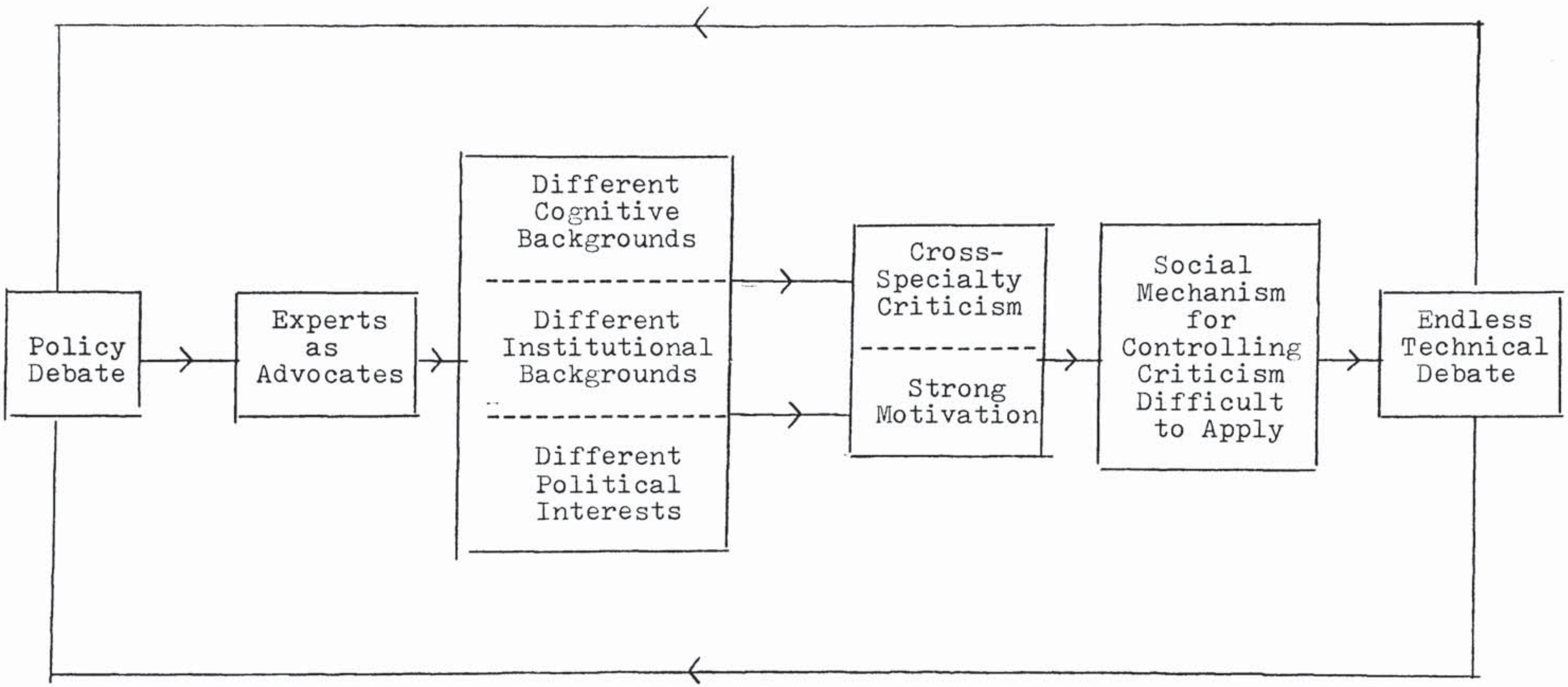


FIGURE 1. OVER-CRITICAL MODEL OF THE INTERACTION BETWEEN SCIENCE AND POLICY

objectives with scientific dissensus. Where policy consensus coincides with scientific consensus, providing expert opinion supports the chosen policy option, there will be no motivation in the political arena to seek out alternative expert views, or to fund critical research. Without alternative scientific advice, policy-makers will have a poor idea of the reliability of the scientific claims which are being made. This can have very serious implications if the advice which has been received also happens to have been based on work which was under-criticised in the scientific arena. In this case the advice may be of very dubious reliability. This need not always be the case, criticism may have occurred within the scientific arena and one theory may have emerged as the best available. However, policy-makers will not necessarily be aware of this, so they will have a poor idea of the reliability of claims which appear consensual, and furthermore these claims may in fact be of very dubious reliability.

The second situation where under-criticism may occur is where there is consensus in the policy arena but dissensus amongst scientists. In this situation dissenting scientific views which do not fit in with the chosen policy option will not get an airing in the policy arena, since it is not in the interests of any of the political groups involved to support or publicise these views. Thus, only the scientific advice which supports the policy consensus will be accepted and publicly supported in the policy arena. This will again lead to a poor assessment of the reliability of this advice as it is under-criticised in the policy arena and the advice may, in fact, represent the least accurate or valuable of the available alternatives. See Figure 2.

Just as particular issues or problems may undergo shifts in the situation of consensus or dissensus in the policy or scientific arenas, as Ezrahi has shown, so they may move between situations of under-criticism and over-criticism. For example, the second case of under-criticism may not exist for long, as dissenting scientists may force their views upon the policy-makers forming themselves into a pressure group advocating a rival policy option. (Some scientists are more willing to do this than others).[124] In this case, there will no longer be consensus in the policy arena and, depending on the strength of the dissenting group and their ability to attract allies and resources, an over-critical situation may develop.

Finally, the fourth situation which Ezrahi describes is, that where there is consensus amongst scientists but dissensus with respect to policy objectives. First of all this is a situation which is unlikely to occur very often since: "The sophisticated public servant knows not only that there is 'another' point of view on almost any scientific issue which bears on an important policy question, but he knows quite matter of factly to whom to turn to get it." [125]

This point is reinforced by Clark who, in explaining, 'How to exploit expert advice,' gives specific guidelines on how to, 'Choose the right expert'.[126] In the unlikely

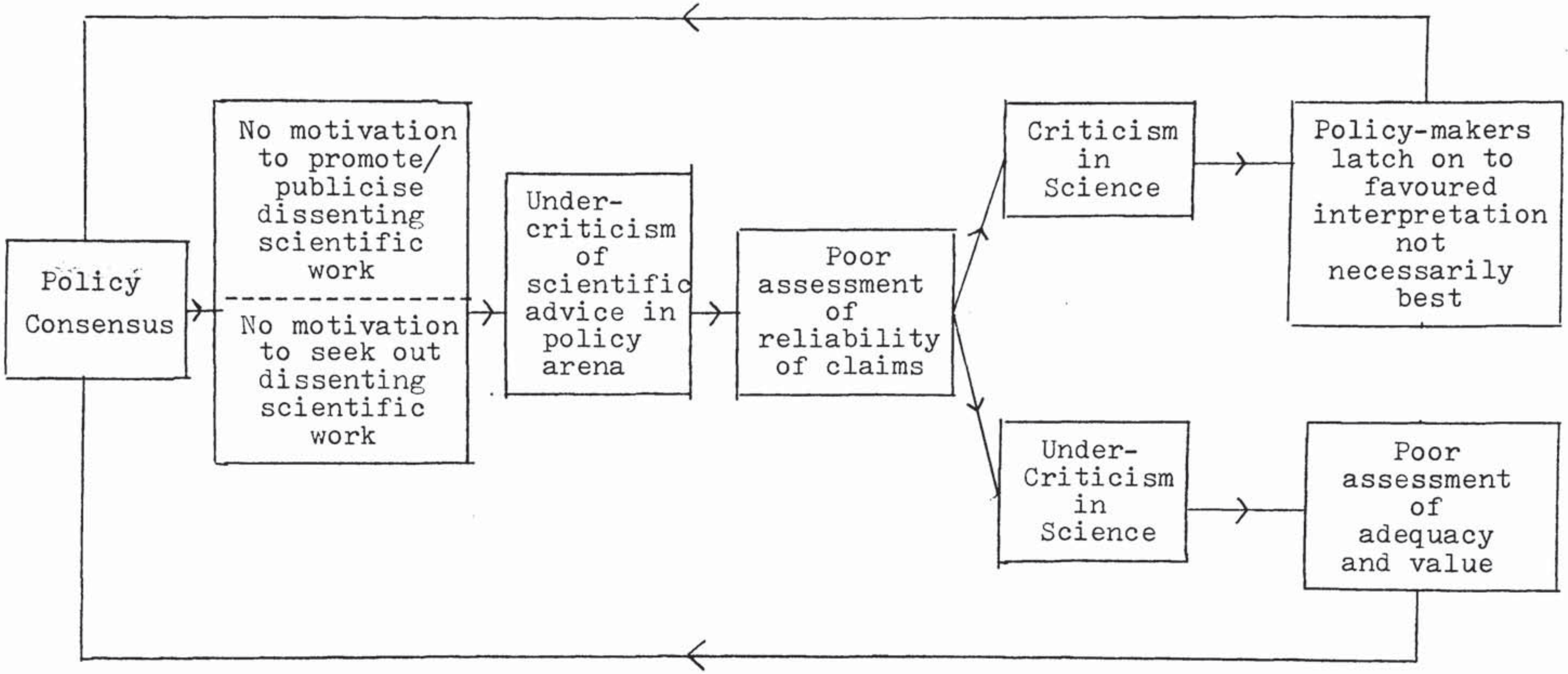


FIGURE 2. UNDER-CRITICAL MODEL OF THE INTERACTION BETWEEN SCIENCE AND POLICY

event that this situation should arise, and Ezrahi gives a number of examples which he interprets in this way, the scientific advice is unlikely to be of any use in the policy process since the political debate will not be over the scientific aspects of the issue. This point is made by Ezrahi and examples are provided to illustrate it.[127] In the fluoridation controversy, for example, while there may be consensus amongst scientists on the effects of fluoridation on the human body and on the effects of artificially adjusting the fluoride content of the public water supply, disagreement may occur at the political level over the right of an individual to choose what he consumes, and over the level at which this decision is taken. This is Ezrahi's own interpretation of the fluoridation controversy, though it seems unlikely that this situation of consensus in the scientific arena would persist for long. With powerful interests keen to challenge every aspect of their rivals' political argument, dissenting scientific advice would no doubt soon emerge.

One further point should be made about Ezrahi's examples of the situation where there is consensus in both the policy arena and amongst scientists. The examples given are those of the Manhatten project and the Apollo programme. As Ezrahi points out, these are usually given as illustrations of the success of scientific knowledge when applied to public policy. However, as Ezrahi admits, in these cases the problem can be defined as technological and they are rare occurrences.[128] These particular examples are also referred to by Ravetz who also categorises them as 'technical problems'.[129] The type of problem which we are more concerned with in this study are the more common and more problematic 'practical' problems. Problems only have this technical character when their objectives are clearly defined and agreed upon and when they are based on the application of established scientific or technical knowledge. In these cases the purposes to be served are defined simply and are agreed upon. The problem could then be defined in terms of a function to be performed and a technical project was undertaken to produce a device to perform that function. As will be seen from the analysis of case study material in this study, while agreement on policy objectives and amongst scientists is a necessary condition for a problem to have this technical character, it is not a sufficient one.

A point which Ravetz makes about practical problems is that many of the particular sciences relevant to their solution are 'immature'.[130] These sciences are, in early stages of their development, such that disagreement can still occur on the fundamentals of the field. Thus, part of the difficulty in solving practical problems stems from the immaturity of these sciences, enabling controversy to occur amongst experts. The proponents of the 'Finalisation thesis' also argue that there are stages in the development of a science when it is more amenable to the imposition of goals from the political arena and stages where this external goal setting is inappropriate.[131] In contrast to Ravetz though, they argue that the pre-paradigmatic stage in the development of a science is a time when it is amenable to the imposition of external goals, along with a post-paradigmatic stage. It is argued however, that if external goals are imposed on a science in the exploratory phase, then the result will be what they call 'Functional'research.[132] That is, research which cannot draw on causal theories and which provides no explanation of a problem. The development of therapy programmes in clinical cancer research and environmental protection programmes are given as examples of this type of research. It is argued that the 'paradigm articulation' phase of disciplinary development is characterised by an 'internal' research programme which determines the range of choice of problems and objectives and which is, therefore, incompatible with 'external' problem-orientation. A link between research and external problems only occurs in this phase if the internal research front coincides with external problem-orientation. Finally, it is argued that, in the 'post-paradigmatic' phase of disciplinary development, the 'internal' rules within a field are so weakly selective that theory developments can proceed in accordance with 'external' guidelines.[133] For these authors, then, this final phase is the most appropriate for the imposition of external goals on to a science. A further argument which they make is that the harnessing of a particular science to political goals will influence the development of that science. This thesis has been explored with respect to Toxicology by Coles,[134] who concludes that external policy goals have had a profound effect on the social organisation of the science and on aspects of its cognitive content, but this influence can work in a number of directions. It has been argued by Weingart that, when political goals are imposed on science, it is the problems which are adjusted to fit the existing organisation of science, rather than the reverse.[135]

While the stage of development of disciplines, and particularly the involvement of immature disciplines, may accentuate the problem of disagreement, one of the aims of the current study is to suggest that characteristics of 'hard', 'mature' sciences which the above discussions of work in the sociology and philosophy of science draw out, combined with characteristics of 'practical' problems, produce serious difficulties in attempting to apply science to policy. In the perspective presented in this study the problems associated with immature disciplines are an additional irritant to a more serious problem of mis-match between science and the needs of policy makers.

C H A P T E R 5

SCIENCE IN THE POLICY PROCESS

CHAPTER 5. SCIENCE IN THE POLICY PROCESS

5.1 Introduction

The discussion so far has concentrated on the implications for science of its involvement in policy questions. The present chapter will discuss the other aspect of the interaction between science and policy; namely the role which science plays in policy formation. If the 'over-critical' and 'under-critical' models are accurate descriptions of the way in which science has behaved and predictions of the way in which it will behave when faced with policy issues, then these models have serious implications for the role which science can play in policy-making. Before discussing the various roles which science may play it will be necessary to say something about the nature of the policy process.

5.2 The Policy Process

In discussing policy I shall follow Heclo in assuming that policy can consist of what is not being done as well as what is being done.[1]

The literature, or rather literatures, on the policy process and policy-making are enormous and I shall not attempt to review them here. The justification for this is that a number of reviews already exist and a lengthy discussion of those literatures here would not add anything to the thrust of the argument in this thesis.[2] What I shall do in this chapter is to make explicit the sort of model of the policy process which underlines the analysis of case study material presented here.

Researchers contributing to the long running debate over the various dimensions of power, have given a considerable amount of consideration to the question of the nature of the policy process. One model of this process which is particularly useful is that provided by Van der Eijk and Kok.[3] Their main concern is with the process of agenda formation and, particularly, with elucidating the concept of non-decisions first proposed by Bachrach and Baratz.[4] The policy process model which they employ is a refinement of the one presented by Bachrach and Baratz.[5] The model represents the policy process as a linear path consisting of various stages with barriers between them. These stages are agenda-formation, decision-making and implementation. The process starts with 'wants' which are "....opinions, interests, ideologies and similar ideas and attitudes which are cognised in a <u>non-political way</u>," (emphasis in original).[6] Wants become 'demands' when they are politicised, that is, when an individual or group perceive them as necessitating action by those with responsibility for making political decisions and when this perception is voiced publicly. 'Issues' are then demands which are recognised by the decision-makers as problems to be decided upon: "..they are demands which become part of the agenda for decision making ."[7] So the process by which wants become demands and demands become issues is the agenda-formation stage of policy-making. The next stage, the decision-making stage, produces decisions. These may be decisions to act, to delay action, or not to act at all.

Finally, there is the implementation stage from which the outputs of policy are produced. As we saw earlier in Ravetz's discussion of practical problems, these outputs may be quite different from those intended by the policy-makers or by those who made the initial demands on policy-makers.[8]

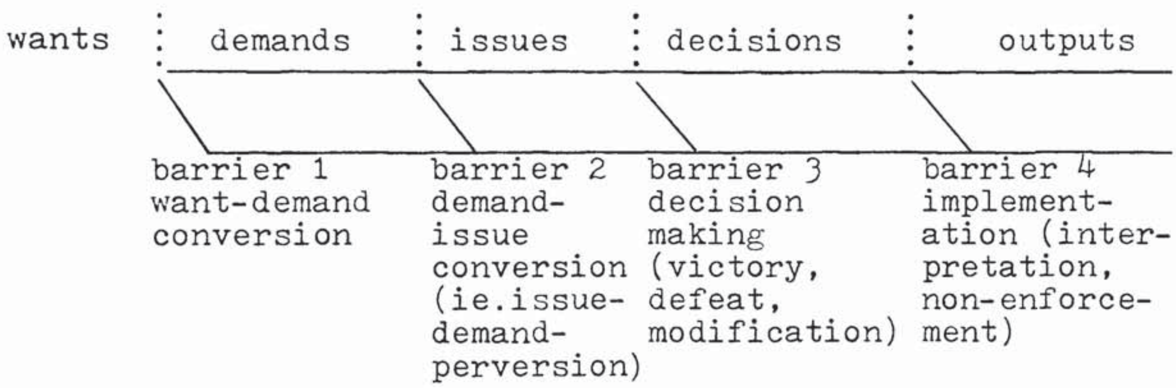
In Van der Eijk and Kok's model the various stages outlined above are separated by barriers. The first barrier is that which prevents wants from being converted into demands. Causes of this prevention can be,"....values, beliefs and myths that the dominant portion of the community embraces."[9] These act to suppress grievances which conflict with prevailing norms or which are at odds with the dominant view of what constitutes a politically legitimate issue.[10] That is, they make certain want-demand conversions legitimate and others not.[11] Other causes may be lack of knowledge, anticipated reactions of others, and actions by other actors to prevent the occurrence of organisational structures conducive for want-demand conversions.[12]

The second barrier is the prevention of issue formation and demand perversion. This is where procedures, customs and organisational devices select out only those of the many competing public demands which decision-makers are prepared or compelled to consider for decision.[13] At this stage demands may get converted into issues but they may lose part of their original character or become moderated by combination with other demands: this is demand perversion.

The third barrier is the decision-making process. Here, the outcome of the process may be victory or defeat for a particular issue, but again issue-perversion can occur for example when a 'rider' is attached to a proposal for decision.[14]

The fourth barrier is implementation, during which values, procedures and customs are all operative in restricting or re-shaping stated policy.[15]

Figure 3 Van der Eijk and Kok's Barrier Model of the Political Process.[16]



Van der Eijk and Kok define as nondecisions,"...all those instances where behaviours and/or social processes result in preventing a want from reaching issue-status, that is, agenda-status."[17] Now, because the present study deals with problems which have all reached issue status, the category of non-decisions is not of prime interest here. However, that is not to say that it is not relevant, and the processes by which the present issues have achieved agenda status may elucidate phenomena relevant to the consideration of non-decisions.

An important phenomenon to be aware of when employing a linear model such as the one described above is that of feedback. Both Bachrach and Baratz and Van der Eijk and Kok discuss this phenomenon, if briefly.[18] The former authors point out that a particular policy choice may result in the formation of new groups or the destruction of old ones, it can also affect values, beliefs and inputs. It can result in the modification of procedures and rituals. In all these ways policy choices can alter the structure and functioning of the political process. Van der Eijk and Kok point out that, in addition to these effects, the outcomes of policy choices can affect wants and, in fact, are one of the major determinants of wants. Furthermore, agendas are partly determined directly by policies and outputs. "Decision makers do not merely react to those wants that gain enough impact and support to develop into issues, they develop issues by themselves."[19] Thus, political decision-makers are not impartial arbiters of disputes but are active participants in agenda-building. With incremental decision-making successive agendas will overlap so that many new demands cannot achieve agenda status as other issues partially decided on still fill the agenda.[20] It may also be argued that the outcome of a particular policy may well represent an issue in itself which needs a decision and the development of a new policy.

Related to the phenomenon of feedback is the issue of sequence. The linear model suggests that each stage occurs in a strictly chronological sequence. However, since outcomes of the implementation process may feed back into any of the earlier stages , an iterative process may well occur. Here implementation may produce a new issue which is then decided on leading to a new phase of implementation which again feedsback into decision-making and so on. In this case implementation and decision-making or implementation and agenda-formation may well occur side by side and need not follow in a strictly linear chronological sequence.

The advantage of this model, and the justification for applying it, is that it recognises that policy-making involves more than just the decision-making phase. Some types of policy analysis tend to treat policy-making just in terms of the decision phase and they miss the important role in policy-formation which agenda-formation and implementation play.[21] In the context of the present study this is important because it raises the question of the role which science plays in these various phases.

5.3 The Role of Science

In an earlier chapter it was seen that a positivist model of the role of experts in policy-making must attribute disagreement amongst these experts to bias.[22] The model views experts as neutral providers of unproblematic information. This model underlines two views of the relation between science and policy which were once prevalent and which still underline much thinking about this relation. These views have been dubbed by Habermas the 'Technocratic Model' and the 'Decisionistic Model'.[23] The technocratic model holds that the decision-making power of the politician has been usurped by the scientific analysis and technical planning of the expert. According to Habermas, in this case political power has been reduced to rational administration and this can only occur, he argues, at the expense of democracy. This technocratic model has been described by Ezrahi as 'Utopian Rationalism'.[24] According to this interpretation of the model the policy-making process can, and should, be adjusted to ensure the maximum assimilation of relevant knowledge into the decisions made and actions taken to carry them out. Here political aims and considerations in the making of policy are regarded as obstacles to be overcome on the way to progressive rationalisations of decisions and actions.

The decisionistic model involves a clear distinction between the rational scientific approach of the expert and the necessarily irrational aspects of values, goals, and needs, among which politicians must decide. On this model experts can provide rational advice but decision-makers must ultimately decide between competing value orders and convictions which escape compelling argument and remain inaccessible to cogent discussion. Elsewhere this model has been described as the 'Democratic Paradigm'.[25] In 1931 Harold Laski, warning against technocratic tendencies, argued that questions of policy can be distinguished from technical questions and advocated that experts should provide only technical information, having no greater influence on the selection of social goals than any other citizens.[26] Similar warnings are still made by policy-makers who continue to expound the virtues of the democratic paradigm. For example, in 1977 U.S.Secretary of Defense, Harold Brown, warned against 'expert advocacy':[27]

> "If in the guise of analysis and exposition, an expert becomes an advocate for a particular decision, he sometimes may have his own way, but only by substituting his own judgement for that of people who have the responsibility for decisions and who might weigh values differently if given all the facts, and whose judgement may be better."

This model has been elucidated by Clark who emphasises the assumption that technical questions, for which in principle there are determinable answers, and questions of policy which involve value judgements and for which there can be no scientific answers, can be clearly separated.[28] The resolution of a technical question is the scientific answer, whereas the resolution of a question of policy is a political choice, according to this model. The decision-maker is left to make a decision between competing value orientations.

While some scientists, frustrated by the operations of the political system, may long for the technocratic model to become reality, and some social forecasters may see it paving the way for 'the end of ideology'[29], most policy-makers still appear to accept the 'democratic paradigm', at least according to Hadden.[30] Certainly, public pronouncements by policy-makers suggest that the positivist model of objective scientists providing pure information is alive and well. For example, during the American debate on the Anti Ballistic Missile in 1969, Assistant Secretary of Defense, David Packard, said of his sources of scientific judgement: "I do not consider that when you are involved with scientific matters it is important whether you have people outside the Defense Department or not. Scientists, to me, are objective about such matters." [31] Apparent acceptance of this model is not restricted to policy-makers. Policy analysts can be found comparing the rational character of science which is concerned with, "The discovery of general truths or the operation of general laws discerned over time through systematic empirical observation," with the, "irrational, non-objective and illogical," characteristics of politics so that there is, "Little wonder, therefore, that the language of politics seems so alien to the cool logic of the analyst or to the rigorous empiricism of the scientist".[32]

As Russell has shown the decisionistic or democratic paradigm model has been apparently operationalised in the field of risk assessment with the repeated assertion of a distinction between the 'estimation' of risk which is seen as an objective scientific activity and the 'evaluation' of risk acknowledged to be a matter of social and personal value judgement.[33] Here, the role of the expert is claimed to be restricted to estimating the level of risk and presenting this information to the decision-maker who must then decide on the acceptability of this level of risk. Applying the models of Habernas in this context, Russell argues that while the decisionistic model is explicitly endorsed in public statements on risk assessment, this represents a mystifying ideology while the reality of the risk assessment activity follows the technocratic model in the sense that outcomes are chosen and rationalised as objectively necessary outside the political sphere.[34] This example serves as a warning that, while policy-makers and experts may appear to accept the democratic paradigm in their public statements, the reality of practice in a given situation may not conform to this model at all.

Both the technocratic and the decisionistic models portray experts as neutral providers of information. In the former model the logic of this information is able to replace the irrational processes of political decision-making. In the latter model experts provide scientific information and policy-makers are left to make decisions between different sets of values in the light of this information. This view of experts roles has already been shown to be inaccurate by Collingridge whose own model characterises experts as advocates, as we have already seen.[35] The two models are rejected by Habermas on the grounds that they are not adequate descriptions of reality and that one denies rationality while the other denies democracy.[36] The democratic paradigm is also criticised by Clark and by Hadden.[37] They both argue that it does not represent an accurate description of reality, however Clark seems to advocate the democratic paradigm as a normative model by which to judge the quality of expert advice in specific cases. According to Hadden, who reviews a large amount of empirical work on the role of technical advice in policy-making, with respect to the democratic paradigm, "A large body of literature attests to its inapplicability."[38] While Habermas rejects the technocratic model as not adequately describing reality and as denying democracy

Ezrahi criticises the equivalent Utopian Rationalism as only being applicable to a situation which is very rare in the relation between science and politics.[39] That is when there is both agreement on political objectives and scientific consensus.

The technocratic and decisionistic models can be seen to be inadequate as descriptions of reality. In addition it can be seen that any model which sees the role of science in policy to be that of a provider of neutral information is inadequate. There are a whole host of other roles which science can play in the policy process. For example, Brewer provides a list of uses to which a policy analysis may be put in the policy process and these may equally well apply to a piece of scientific research.[40] The uses which he suggests are as follows: problem framing; attention directing; bounding and narrowing the options; option testing; posturing; eyewash; whitewash; destruction; postponement. Amongst the roles which have been observed in empirical investigations of the role of advice in policy-making, "Perhaps the most widely accepted proposition stresses the use of technical advice, either as a means of justifying decisions made on other grounds, or as a means of depoliticising or delaying a controversial decision," as Hadden shows.[41] The justificatory role of advice is observed, not just by Hadden and the authors she reviews, but also by Ezrahi [42], Nelkin[43], Weingart[44], Clark[45], Cahn [46], and others.

To replace these unsatisfactory models Habermas proposes a 'Pragmatistic model' in which the strict separation between the function of the expert and the politician is replaced by a critical interaction.[47] This model is intended not to deny rational input whilst being essentially linked to a democratic order. However, the model is not clearly worked out. In a similar way Ezrahi advocates 'Pragmatic Rationalism' as a replacement for the inadequate 'Utopian Rationalism' model.[48] The pragmatic rationalist is concerned with how knowledge can best be incorporated into political decisions and how the knowledge appropriate to the rational attainment of substantive ends can be combined with the objectives of maintaining and increasing political power. This model accepts the inevitability of political ingredients in the making of policies. It also acknowledges that political considerations are not confined to the substance of the policies chosen, but are present in the decisions about what problems or conditions are to be dealt with. Most significantly this model accepts that the level of agreement or disagreement within the respective groups of scientists and policy-makers is important in determining the roles and uses of scientific knowledge in public policy. When there is consensus in both the political and scientific arenas the role of scientists will be to assess the prospective efficacy of measures, according to Ezrahi. When there is consensus on political objectives but dissensus amongst scientists then the latter's role will be to explain the points of their agreement and disagreement providing that they confine themselves to what they really know. In a situation of consensus amongst scient scientists and dissensus on political objectives, the scientist should assist politicians to become aware of the costs and consequences of each of the policies proposed. In this situation, however, even what is put forward as uncontroversial scientific knowledge is likely to be affected by the climate of political partisanship. In other words the information may not be accepted by all parties involved. In a situation where there is dissensus in both political and scientific arenas, the political and scientific disputes will interpenetrate and aggravate the controversy. Scientists will become vulnerable and lose credit previously given to them for dispassionate objectivity. Here divergent views will acquire political significance, "...which inevitably invites political assessment of competing scientific positions."[49]

Since the situation of consensus and dissensus amongst scientists and politicians will determine the roles which science plays in the policy process, any changes in the extent of this consensus will alter the patterns of relations between science and policy-making, according to Ezrahi. An example of this effect is given in the case of the debate over IQ testing in the U.S.A.[50]. Here, according to Ezrahi, the situation changed from one of scientific and political consensus to scientific consensus combined with political dissensus, and finally to a situation of dissensus on both science and policy. With these shifts came a corresponding shift in the social definition of the role of the psychologists. Initially they were experts applying a valid technique, then they were defending and reinterpreting the use of tests and, finally, a split appeared in the scientific community which resulted in most participating experts being seen as partisan advocates.

The pragmatic rationalism which Ezrahi advocates makes the useful point that the role which science plays in policy-making will depend on the extent of consensus or dissensus amongst scientists and politicians. This will not be the only influence on the role of science however. As was seen above, the policy process can be viewed as consisting of a number of stages. It seems likely that the role which science playsin the policy process will be different in each of these stages. It may further be argued that the role which science can, or should, play is different in these different stages. Some investigation along these lines has been done by Weingart who assumed that the circumstances, under which the public administration perceives and defines certain problems and attempts to formulate programmes to cope with them, determine the function science is to have in the process.[51] He investigated the development of a particular government programme, the 'Programme for Environmental Protection' of the German Federal Government, to assess the relative role of scientific knowledge and political intent in the formulation and implementation of this programme. This he did by identifying the inputs from three institutional sectors; the government, the scientific community and the public. He concluded that in the 'Issue Formation' phase, while scientific information was important in identifying the initial problem, it was factors in the political system which determined the appearance of the issue. It was also found that the issue crystallised as a political one in the public and in the scientific community at roughly the same time and in the same way. This led him to conclude that, while a small part of the scientific community had an initiating function in the process of issue formation, it is at the same time subject to a bandwagon effect as the issue crystallised in the public and political sectors.[52] In the phase of programme formulation Weingart argues that the structure of political organisation and administration and the structure of scientific disciplinary organisation and specialisation act as selective filters which affect the way a problem is formulated and perceived. This means that if any scientific knowledge is used in this phase it will first be subject to the filter of administrative organisation such that only aspects of a problem which can be dealt with by existing organisational units will be considered. Then, only scientific knowledge which can be provided by existing scientific specialties will be proffered. This means that any scientific knowledge which is made use of will be attempting to answer a problem with quite different characteristics from the one from which the process was initiated.

Finally, Weingart argues that when programmes are implemented into scientific research processes, instead of their being visible effects of the external direction of science, what occurs is an "...anticipatory reduction of complexity".[53] This means that, through the process of problem perception and programme definition, the 'external' political and the 'internal' scientific criteria of relevance are not allowed to clash. It is also argued that this mechanism operates with respect to the formulation of political goals insofar as they imply a demand for scientific problem solutions. This means that programmatic ideas are reduced to relatively conservative goals that do not reach very far beyond the existing potential of science. This is a very interesting conclusion for it suggests that the limitations of the problem-solving capacity of science when applied to practical problems are allowed for by redefining the problem to fit these limitations.

The present study will attempt to address the question of the role which science plays at the different stages of the policy process, and to assess other factors which influence this role. The role which science plays in the different stages of the policy process will also be assessed in the light of the models of criticism developed earlier.[54] It will be important to remember that science may play a very minor role contrary to claims for its importance. For example, Hadden notes some empirical support for the proposition that, "Advisor scientists simply do not have much influence on policy."[55] In her study of the Anti-ballistic Missile (ABM) debate Cahn comes to this conclusion.[56] Some interesting phenomena along these lines are also noted by Clark in his study of the Supersonic Transport (SST) debate. He notes that: expert advice is usually exploited; expert advice is rarely useful; expert advice is seldom understood; expert advice seldom affects decisions; and the real experts are often not asked.[57]

Of particular interest will be the question of the extent to which scientific information influences the formation of policy. Two predictions can be made from the models of criticism developed earlier. First, in a situation of over-criticism the endless nature of the expert debate will mean that science will have little effect on the actual choice among policy options in the decision-making phase. In this case scientific disagreement may hinder the resolution of the political controversy. In a situation of under-criticism science will be used as a justification for particular policy options. With a policy consensus there will be no motivation to seek out critical scientific views, so that scientific views will only be aired in the political arena if they support the policy consensus. Again choices among policy options in the decision-making phase will not be influenced by science. So, on the basis of these models, the prediction is that, in the decision-making phase at least, science will have little influence on the policy debate and may even hinder its resolution. An area where the influence of science may be important is at the beginning of the policy process in identifying specific 'wants' and as a tool for making these into 'demands'. However, in the process of making these 'demands' into 'issues', that is getting them onto the political agends, the influence of science may again be limited by the processes of under- and over-criticism.

P A R T 2

CHAPTER 6

SMOKING AND HEALTH:

THE DEBATE AMONGST EXPERTS.

6. SMOKING AND HEALTH: THE DEBATE AMONGST EXPERTS.

6.1 Introduction.

In an earlier study of the smoking and health controversy in Britain Reeve[1] concentrated on the nature of the debate amongst experts and the influence of policy implications on this debate. That is to say, the earlier study concentrated on what I have called the contents and context of the debate. This chapter will present some of the findings from that study. This will enable further assessment of the content and context argument and will provide background information for the presentation of findings relating to the influence of technical experts on the formation of policy with respect to smoking. The findings on the influence of science and scientific experts on policy will be presented in Chapters 7 and 8.

As a full discussion of the debate amongst experts is given in the earlier study, the present treatment of the content and context of the smoking debate will be brief. Furthermore, this chapter will follow the previous study in concentrating on the issue of the relation between smoking and lung cancer. Some justification for concentrating on this issue is provided by the fact that it holds a dominant position in the history of the smoking and health controversy. Further justification is provided by the similarity of the content of the debate over this issue and that of the debates over other diseases, as well as the similar contexts of these various debates. Further discussion of the relation of the lung cancer issue to the debates over other diseases associated with smoking will be given in the next chapter.

The scientific debate over the relationship between smoking and lung cancer can conveniently be seen as starting in the early 1950s with the publication of the first results from a retrospective study of the smoking habits of lung cancer victims by Doll and Hill[2]. In 1957 the Medical Research Council (MRC), noting an increase in lung cancer, looked for causes of this increase[3]. They argued that the evidence pointed to a close association between smoking and lung cancer and that the most reasonable interpretation of this relationship was that it was causal. The evidence was the retrospective studies mentioned above, comparing past histories of lung cancer sufferers and non-sufferers, prospective studies looking at the smoking habits of defined groups in the population and studying the causes of death occurring subsequently in the group, and laboratory experiments which had identified carcinogens in tobacco smoke. The MRC statement was endorsed by the British Medical Journal (BMJ), the official organ of the British Medical Association (BMA), which went on to propose measures to reduce the smoking habit[4]. Since then there have been three reports from the Royal College of Physicians (RCP)[5] which represent milestones in the medical profession's case against smoking.

A rival hypothesis accounting for the accepted association between smoking and lung cancer was proposed by the eminent statistician, geneticist and biometrician, Sir Ronald Fisher FRS[6]. He suggested that individual genotype may cause both lung cancer and smoking. Fisher's argument was initially that the case for the causal hypothesis was unproven and the constitutional hypothesis represented a plausible alternative. From this point of view, action to reduce smoking was unjustified, and there was a danger in accepting the causal hypothesis that research into alternatives might be neglected.[7] As his views developed, Fisher went on to argue that the causal hypothesis was seriously discredited and the constitutional hypothesis was supported by new evidence.[8]

This early dispute set the tone of the whole debate. The question of contention has not been the association between smoking and lung cancer, but the interpretation of this association. On one side are members of the medical profession arguing that the best interpretation is that smoking is a major cause of lung cancer and urging Government and professional action to control the habit. On the other side are a collection of mostly statisticians and psychologists arguing for an interpretation of the association based on genetic predisposition and for whom action to curb smoking is premature. The tobacco industry, with its own research organisation, naturally supported the dissenters. For example, Fisher became a consultant for the Tobacco Manufacturers' Standing Committee (TMSC), which later became the Tobacco Research Council (TRC), and Eysenck's work was supported by the TMSC/TRC. It should be noted, however, that they also funded some prominent causal theorists.[9] In addition, the TRC did a large amount of research themselves at their own laboratories, mostly in the form of animal experiments and concentrating particularly on the identification of harmful substances in tobacco smoke with the aim of removing or reducing them through product modification. However, they continually emphasised the role of other environmental factors, such as air pollution, in the causation of lung cancer, and a number of TRC funded projects were concerned with research in this area.

As the debate between those supporting a causal theory and those supporting a constitutional one developed, emphasis was placed by each side on evidence which appeared decisive in supporting one theory and conflicting with the other. In this way a number of crucial issues emerged, on which the debate centred. Most important among these were the following:

1. The critics of the causal theory placed emphasis on the early epidemiological studies which had shown that smokers who inhaled had a lower risk of lung cancer than those who did not.
2. The critics of the constitutional theory emphasised the observation that, when male British doctors reduced their cigarette consumption, their lung cancer mortality rate fell relative to the rest of the male population and corresponding to their reduced cigarette consumption.
3. The critics of the causal theory emphasised the need for studies of monozygotic and dizygotic twins to test the genetic component in smoking and in lung cancer, and some support for the constitutional theory came from these studies.

4. The critics of the constitutional theory emphasised the observation of a large recorded increase in mortality from lung cancer.

Rather than cover the whole debate, the present study will confine itself to a discussion of the development of two of these crucial issues. For detailed coverage of the whole debate, see Reeve.[10]

6.2 The Inhalation Problem.

In the Doll and Hill study of 1950[11] the data showed a negative correlation between inhaling and lung cancer. Fisher concentrated on this point as it can be dealt with by the genetic hypothesis, but seems to conflict with the causal hypothesis. He managed to get the collected data from the study from Bradford Hill and analysed it closely in a pamphlet published in 1959.[12] Fisher concluded that the data on inhaling provided a significant 'knock-out' for the theory that smoking is a cause of lung cancer. Following this analysis of the data on inhaling, Fisher questioned the conclusions which the MRC had come to in their statement on smoking and lung cancer. He argued that they were jumping from the observation of an association to the conclusion of a causal relation. To emphasise this point he argued that, if the MRC make this jump in the case of cigarette smoking, they should also make it in the case of inhaling, and this would lead to the conclusion that cigarette smoking causes lung cancer but inhaling cigarette smoke prevents it.[13]

This apparent problem for the causal theory became a key issue in the debate, being repeatedly referred to by the opponents of the causal theory. In 1960 Eysenck and collaborators presented some evidence in support of the genetic hypothesis.[14] In criticising the causal hypothesis they refer to the problem that inhaling appears to have an ameliorating effect with respect to lung cancer. In an exchange following this paper, between Eysenck and 'Geminus' of New Scientist[15], Eysenck referred again to the inhalation problem.[16] To this 'Geminus' replied that at least three studies, other than the early Doll and Hill one, had shown a greater risk of lung cancer among inhalers[17]. In the first report from the RCP[18], published in 1962, it was admitted that there was some conflicting evidence on the effects of inhalation of smoke, but by the time of the publication of the second report, 1971, the Royal College concluded that inhaling increases the risk of lung cancer[19]. The issue arose again however when Professor P.R.J.Burch published a paper in New Scientist describing his views on the smoking and lung cancer debate[20]. Here, he refers to Fisher's findings on inhaling, claiming them as a serious problem for the causal theory. In a reply to this paper Sir Richard Doll[21] suggested that the figure on inhalation used by Burch was atypical. He further argued that an explanation for the apparent anomaly had been provided nearly twenty years previously by Dr.C.N.Davies. This was that deep inhalation does not deposit smoke droplets on the walls of the bronchii which is where lung cancer usually occurs. Doll concludes that until enough is known about the physical distribution of tobacco droplets in the respiratory tract of different types of smokers, the data on inhaling remains open to debate.

In 1976 Doll and Peto published the final results of the twenty year investigation of the smoking habits of male British doctors[22]. Concerning the inhaling issues, they again admit that some conflicting evidence exists, but they point out that some studies suffer from lack of standardisation for amount smoked and that the distinction between inhaling and not-inhaling is not absolute. They conclude that it is impossible to interpret the results on inhaling until more knowledge is available on the fate of smoke droplets.

The inhaling issue is discussed in the third report from the RCP published in 1977[23]. Acknowledging that several studies have shown that, among heavy smokers, inhalers have a slightly lower risk of lung cancer than non-inhalers, the report suggests two explanations for this. Statements about inhaling are inaccurate, or deep inhalation deposits smoke in parts of the lungs that are less susceptible to cancer. The issue crops up again, however, when Eysenck uses the inhalation problem to criticise the causal theory in a book published in 1980[24]. In criticising this book Peto[25] argues that Eysenck uses out-dated evidence on the inhalation issue, ignoring more recent evidence which provides an explanation for the apparent anomaly.
Evidence published in 1978[26] was purported to show that heavy smokers who say they do not inhale are actually

'slow' inhalers, and those who say they do inhale are 'fast' inhalers. Fast inhalation may cause less tar to be deposited in the upper airways most susceptible to cancer.

The history of this issue shows how it was first highlighted by the opponents of the causal theory who saw it as a serious problem for that theory. But, as these critics repeatedly emphasised this issue, the causal theorists developed various explanations for the apparent anomaly and placed emphasis on the inconclusive nature of the evidence. These explanations finally led to new types of research into the accuracy of statements about inhaling.

6.3 The Effect of Giving up Smoking

In 1964 Doll and Hill published an influential paper in the BMJ presenting results of their long running study of the smoking habits and deaths from lung cancer among British doctors[27]. They interpreted the results as showing, among other things, that in men who had given up cigarette smoking the death rate from lung cancer had fallen. The authors maintain that this can only be explained in terms of the causal and not by the genetic hypothesis. The second report of the RCP in 1971[28] placed emphasis on this evidence as a serious problem for the genetic hypothesis. The report argues that, on the genetic hypothesis, the inborn liability and hence incidence of lung cancer should be unaffected by persuading people to resist their own desires and stop smoking, and the British doctor's evidence conflicts with this. Following the RCP's second report an editorial in The Lancet[29] placed emphasis on the evidence that stopping smoking reduced the risk of lung cancer, seeing it as important in that it conflicts with the genetic hypothesis and with the argument that there is no point in stopping after essential damage is done.

Following the presentation of this evidence in the second RCP report, it was subject to a close examination and criticial assessment by Carl C.Seltzer of the Department of Nutrition, Harvard School of Public Health[30]. Among Seltzer's criticisms were that certain difficulties with the interpretation of secular trends were ignored by the RCP and that they grouped diseases into the categories 'related' and 'unrelated' to smoking in a different way from the Doll and Hill study, on which their analysis heavily relied, without any explanation for the change. More seriously Seltzer pointed out that the RCP report had used data for only two of the three time periods in the original study and for only one of the two age groups. He also argued that the data did not support the RCP's claim that there was little change in the smoking habits of the general population comparable to the change observed among doctors. After a re-examination of the data, including both age groups and all three time periods, plus further data on the smoking habits of the general population, Seltzer concluded that the statements and claims of the RCP, based on the particular set of data which they cite, cannot be supported.

In an editorial accompanying this article, <u>The Lancet</u>[31] suggested that several of Seltzer's criticisms are unimportant and the more serious ones it calls into question. They attempt to provide some justification for the RCP's omissions of data and accuse Seltzer of missing out the most crucial time period. Finally, they admit that Seltzer has confused the issue, but deny that he has thrown any doubt on the main conclusion that the data on British doctors provides strong evidence that stopping smoking increases the expectation of life. The article by Seltzer sparked off a series of correspondence in the columns of The Lancet including letters from Richard Doll[32] and C.M.Fletcher[33] arguing that The Lancet editorial had successfully shown the weakness of Seltzer's arguments. Seltzer himself[34] provides a detailed reply to the editorial, reiterating his criticism that the RCP report's claim rests on an inaccurate statement, namely, that during the cited time periods cigaratte smoking declined among British doctors but did not in the general population.

The issue arises again when Burch[35] criticises Doll and Hill's findings along the same lines as Seltzer had done. Burch found that, over a particular time period, the upper age group actually showed a considerable increase in lung cancer mortality and he suggests, with tongue in cheek, that retired doctors should have been warned of the dangers of giving up smoking. He attributes the various fluctuations in the trends involved for different age groups to diagnostic errors producing spurious trends.

In reply, Doll[36] maintains that Burch has quoted only a subset of the figures which gives a different picture from the whole. He argued that, although there are some irregularities in the figures, the contrast between doctors and all men is marked and the evidence weighs against the constitutional hypothesis. Burch replied by emphasising the inconsistencies in the evidence for doctors, arguing that the data shows that a large increase in recorded death rates accompanied an increase in the proportion of ex-smokers. He concludes that a comparison between the different populations of doctors and 'all men' (as was the Doll and Hill comparison) is fraught with dangers. In a correspondence following these articles Seltzer[37] criticised Doll using the same arguments as he had presented earlier in The Lancet. He argued that Doll's use of facts and his comparison between British doctors and the general population are both faulty. The facts, according to Seltzer, contradict the causal hypothesis and this calls for 'dispassionate examination' rather than 'unsubstantiated rhetoric'.

In Doll and Peto's presentation of the final results from the twenty year study of British doctors in 1976[38], they make the observation that lung cancer grew relatively less common as the study progressed, while other cancers did not, corresponding with a reduction in cigarette consumption during the period of observation, and this they claim illustrates the causal nature of the association between smoking and lung cancer. An editorial in the BMJ[39] commenting on this paper, mentions this observation as a significant one. In a letter to the BMJ Burch again argues that comparisons between temporal trends of mortality from lung cancer in doctors and the dissimilar population of England and Wales, with dissimilar standards of diagnosis, is inadmissible.[40] He further argues that Doll and Peto's own analysis only shows an average reduction in 'relative' mortality of doctors versus men, whereas the data suggests that the recorded and verified 'absolute' death rates from lung cancer in British male doctors have shown no significant temporal trend up or down over the period 1965-71. He argues that, since the consumption of cigarettes by male doctors fell by more than fifty percent in this period, it appears that doctors have derived little or no benefit with respect to lung cancer or to all causes of death by giving up cigarettes. The third RCP report, published in 1977[41], reviewed the evidence related to the effects of stopping smoking, particularly that presented by Doll and Peto. The report gives as one of the main reasons for rejecting the genetic hypothesis that it fails to account for the relative decline in mortality from lung cancer among doctors.

In the discussion of a paper given by Burch to the Royal Statistical Society in 1978[42], in which he presents further criticisms of the causal theory, Seltzer[43] provides another criticism of the studies involving ex-smokers. These, he claims, are all flawed because the groups they involved were self-selected. Following this line of thinking, Eysenck presents the results of studies in his book <u>The Causes and Effects of Smoking</u>[44], published in 1980, into the characteristics of smokers, non-smokers, successful and unsuccessful givers-up. It was found, Eysenck argues, that those who give up successfully are constitutionally more like non-smokers, while those who fail to give up smoking are constitutionally more like smokers. This, he claims, is an important finding since it shows that smokers and former smokers are not comparable groups. He concludes that genetic factors not only cause us to smoke, but also cause us to give up smoking.[45] In criticising Eysenck's book, Peto argues that he has misinterpreted the results of the Doll and Hill study concerning doctors' smoking habits and his conclusion that the effects of giving up smoking did not support the view that giving up protects against lung cancer is in direct opposition to the facts.[46]

The development of this issue illustrates some of the same points as the inhaling issue. A piece of evidence was interpreted by one side in such a way that it appeared to be decisive in supporting one theory, in this case the causal, and conflicting with the other. As this evidence was publicised as a decisive confirmation of the causal theory, the critics of that theory immediately called that interpretation into question. For the most part, the data itself was accepted, but the use of that data and its interpretation were questioned. In fact, in this case, it was suggested that the interpretation could be turned completely on its head; where the RCP had argued that the data showed that giving up smoking reduced the risk of lung cancer, Burch argued that they appeared to show that giving up smoking increased the risk of lung cancer. The debate continued with re-examination of the evidence and with further evidence being considered. Then, with the appearance of a possible explanation of the evidence in terms of the genetic theory, a new type of study was conducted whose results appear to support this explanation.

The history of both these issues illustrate this pattern: the identification of some evidence by one side with a decisive interpretation; the presentation of an alternative interpretation of the same evidence by the other side; a long debate on the merits and validity of each interpretation calling on extra evidence; the development of new types of research to test the various interpretations. This pattern is not unique to the two cases described above, but is common to all the crucial issues which emerged in the smoking and lung cancer debate. However, also common to all of these issues is the fact that, as far as the dissenters are concerned, they have not been resolved. Both sides can still call on evidence which supports their position.

CHAPTER 7

SMOKING AND HEALTH:

THE PROCESS OF AGENDA FORMATION

7. SMOKING AND HEALTH: The Process of Agenda Formation.

7.1 Introduction.

In the previous chapter the controversy over smoking and health was introduced by concentrating on the content and context of the debate amongst experts on the lung cancer issue. The next two chapters will be concerned with the role which this debate and its participants have had on the process of policy formation. Following the linear model of the policy process described in Chapter 5 the development of the smoking and health issue in the policy arena can be seen as falling quite neatly into phases of agenda formation and decision-making, or policy development as I shall call it. The present chapter will discuss the agenda formation phase of the smoking and health controversy. The policy development phase will be discussed in the following chapter.

7.2 Smoking, Lung Cancer and Other Diseases.

While the previous chapter concentrated on the issue of smoking and lung cancer, to discuss the whole smoking and health debate reference must be made to the other conditions involved. There are three major diseases with which smoking is associated. These are: lung cancer, chronic bronchitis and coronary heart disease. Other conditions associated with smoking include: emphysema, gastric and duodenal ulcers, cancers of the mouth, pharynx, larynx and oesophagus, cancer of the bladder, pulmonary tuberculosis, heartburn, ill effects on babies if mothers smoke during pregnancy, and ill effects on non-smokers through passive smoking.

The lung cancer issue was the first to receive attention from scientific research and analysis and, for a long time, dominated the controversy over smoking and health. At an early stage the medical profession were convinced that the evidence conclusively showed that lung cancer and chronic bronchitis were causally related to smoking and that this provided the rationale for action against smoking. The issue of coronary heart disease emerged somewhat later. The first Royal College report[1] concentrated on lung cancer and bronchitis, mentioning that smoking probably contributed to the development of coronary heart disease. By the time of the publication of the second report[2] they were more convinced. Here it was argued that cigarette smoking is an important factor in the causation of coronary heart disease and that the general avoidence of cigarettes would greatly diminish the number of deaths from this condition. This issue was more prominent in the later part of the expert debate. Concerning bronchitis the 'dissenters', notably Eysenck and Burch, have admitted that smoking causes some morbidity through this condition.[3]

It is interesting to note that, to a large extent, the arguments used by the experts on both sides of the controversy concerning lung cancer are the same as those used in relation to other conditions. For example, similar arguments to those used in the lung cancer debate were used in the debate over the role of smoking in relation to coronary heart disease. The Royal College of Physicians (RCP), argued that epidemiological studies show a correlation between smoking and coronary heart disease. This correlation is strong and consistent, is independent of other risk factors, is enhanced in smokers who inhale, and the risk shows a progressive reduction in those who give up smoking. This, combined with studies which have demonstrated injurious effects in animals, leads the RCP to conclude that the relationship between smoking and coronary heart disease is causal. The argument from the other side of the debate is that, although there is an association between smoking and coronary heart disease, it is not causal and is best explained in terms of constitutional predisposition.[4] Furthermore, the types of evidence employed in the lung cancer debate are also similar to those used in relation to other conditions. In fact, a number of the most important studies in the lung cancer debate appear as key pieces of evidence in the debates relating to other diseases and ill effects associated with smoking. For example, in the issue of the relation between smoking and coronary heart disease, those favouring a causal explanation of the association present data from epidemiological studies, notably the Doll and Peto study of British doctors.[5] Those favouring a constitutional explanation present data from twin studies, notably the Swedish twin studies of Friberg et.al.[6] Both of these studies are central to the debate over smoking and lung cancer.

Thus, when the whole expert debate is examined, its character and content is found to be very similar for all the diseases and conditions associated with smoking. Lung cancer and bronchitis dominated the debate in its early phases and have remained central to it. Coronary heart disease became an issue rather later, but was the subject of familiar rival arguments. These three issues have been the most prominent in the broader-policy context with the emergence of the smoking during pregnancy issue at around the same time as the coronary heart disease issue and with the more recent emergence of the issue of effects on non-smokers through passive smoking.

7.3 The Policy Agenda

The problem of getting smoking and health on to the policy agenda is of particular interest, when considering the roles of experts and expert controversy. The three reports of the RCP concerning smoking and health have been landmarks in the medical profession's case for action concerning smoking.[7] It is clear that the first RCP report played a very significant role in influencing public opinion, as other commentators have observed.[8] However, the issue seems to have emerged somewhat earlier than this date.

As argued in the previous chapter, a convenient date to set as the beginning of the scientific debate on smoking and health in Britain is 1950 with the publication of the first results of a retrospective study by Doll and Hill.[9] Throughout the 1950's further evidence accumulated, implicating smoking in the causation of lung cancer, and the alternative genetic hypothesis was proposed. During this early period of scientific controversy governments developed particular views concerning the scientific arguments and the question of policy and it is interesting to look at the roles of the medical profession and the tobacco industry in this process.

7.4 Agenda Formation: The Government

During the 1950's the position of governments on the scientific issue was revealed in statements and answers to questions made in the House of Commons by a series of different Ministers of Health. In 1953 a panel was set up under the auspices of the Standing Advisory Committee on Cancer and Radiotherapy, and chaired by the Government Actuary, to assess the results from Doll and Hill's early studies at the Medical Research Council's (MRC) statistical research unit.[10] In 1954 the Standing Advisory Committee reported its findings to the Minister of Health, who made an official statement concerning the results. In a parliamentary written answer Mr.Macleod, the Minister of Health, announced that a relationship had been established between smoking and cancer of the lung.[11] He pointed out some reservations concerning the findings, emphasising that there was no firm evidence of the way in which smoking may cause lung cancer or the extent to which it does.. In a press conference following the announcement he emphasised that uninformed and alarmist conclusions should not be drawn.[12]

In terms of the technical debate previously described the statement amounts to an acceptance of the association or correlation between smoking and lung cancer but not necessarily to the existence of a causal link. The committee's report did suggest, however, "...a strong presumption that the relationship is causal".[13] It also stated that, although reliable quantitative estimates of the effect of smoking on lung cancer could not be made, "...it is desirable that young people should be warned of the risks apparently attendant on excessive smoking".[14] At the press conference the Minister concluded that it was not for him to speculate to what extent the report constituted proof, but he thought it right to release the information at once. He made an assurance that research into the question would be vigorously pursued.

Following this statement, and despite calls for appropriate action to inform the public of the supposed risks, from various sources including M.P.s and the Standing Medical Advisory Committee,[15] the Government did nothing. In 1956 Mr.Turton, then Minister of Health, clarified his position on the smoking issue when pressed on a number of occasions in parliamentary question time as to his reasons for inaction and on the results of recent research. At this stage what he said was that, "...what has been shown was that there was a causal connection between smoking and lung cancer. That we know." [16] This suggests that the accepted statistical association was being accepted as representing a causal connection, but the Government still resisted calls for action. Turton claimed that he was waiting for further advice from the Central Health Services Council on the issue before considering government action.[17]

In March 1956 the Standing Medical Advisory Committee and the Central Health Services Council advised the Minister of Health that appropriate action should be taken to inform the public of knowledge on smoking and lung cancer and of the risks involved in heavy smoking.[18] The Minister announced in a parliamentary written reply, that he would consider what action would be appropriate.[19] Following this statement there were further calls for a national campaign to inform the public of the supposed hazards of smoking,[20] and questions concerning tobacco revenue.[21] In May 1956 the Minister announced that two cancer producing agents had been identified in tobacco smoke and emphasised that the chairman of a committee of the MRC which had been investigating the subject considered that the fact that a causal agent had not yet been recognised should not be allowed to obscure the fact that there was, statistically, an incontrovertible association between cigarette smoking and lung cancer.[22] But, although the Minister claimed that the Government would ensure that the public were kept informed of all the relevant information, he denied that a national publicity campaign was appropriate "...at the present state of scientific knowledge."[23]

Again there were continual calls for a national publicity campaign to publicise the supposed hazards of smoking,[24] and some suggestions for such action as the restriction of tobacco advertisements on television,[25] and questions about the morality of the Government's tobacco revenue.[26] As new evidence appeared from Doll and Hill confirming the statistical association between smoking and lung cancer[27], the Minister of Health continued to promise that the public would be kept informed, but also continued to resist calls for a national publicity campaign.[28] He reiterated his position on the scientific evidence, saying that no scientific proof of the connection had yet been found but that statistical evidence of a connection between lung cancer and heavy cigarette smoking was building up. The Ministry of Health's position on the evidence at this stage is further illustrated in a reply, during a debate, by Mr.Vaughan-Morgan, Parliamentary Secretary to the Minister of Health, to further questions on government inaction concerning smoking:-[29]

> "As the House will appreciate, there are many ways of interpreting the statistical evidence available and it is difficult to reduce the evidence to average figures, but without doubt the findings which I have just summarised constitute _prima facie_ statistical evidence that smoking carries a risk of lung cancer in human beings."

This risk, he stated, must be viewed with a due sense of proportion.

The report of the Central Health Services Council for 1956, published in 1957, included the Standing Medical Advisory Committee's comments on the evidence for a correlation between smoking and lung cancer and their advice that action should be taken constantly to inform the public of this connection and of the risk involved in heavy smoking.[30] In a foreword to the report the Minister of Health, Mr.Vosper, announced that the problem of smoking and lung cancer was under continuous review and he was ready to take full account of any new development there may be in framing policy.[31]

A clear picture emerges then, of the Ministry of Health's position, under different Ministers, on the smoking and lung cancer issue up until mid-1957, with regard to both the technical issue and the question of policy. On the technical issue they accepted that a statistical correlation had been demonstrated between smoking and lung cancer, and that, although there was some presumption that the best explanation for this was in causal terms, the interpretation of this correlation was still an open question. Regarding policy, the scientific evidence did not justify a national publicity campaign but the issue would be continually monitored.

In June 1957 a significant shift in this position occurred with the publication of a report on smoking and lung cancer by the MRC.[32] The report, after reviewing the available evidence including epidemiological studies and some laboratory tests, concluded that the most reasonable interpretation of the evidence concerning the large observed increase in deaths from lung cancer was that a major part of it was caused by smoking tobacco, particularly heavy cigarette smoking. Mr.Vaughan-Morgan, Parliamentary Secretary to the Minister of Health, speaking for the Minister, made a statement in the House of Commons as soon as this review by the MRC was issued. He said that the Government felt that this, "...latest authoritative opinion", should be brought to public notice, "...so that everyone may know the risks involved in smoking".[33] Concerning government action he announced that the Government would make these facts known to all those with responsibility for health education. In line with this, the Minister of Education had circulated information about the dangers of smoking to local education authorities and the Government proposed to bring the MRC's views to the notice of local health authorities who, with the assistance of the Central and Scottish Councils for Health Education, would be asked to take appropriate steps to inform the general public. The Parliamentary Secretary pointed out that, under statute, it was the local health authorities who were concerned with the prevention of illness and were responsible for health education as a means of prevention. The Government's role was seen strictly as one of helping with health education, not to interfere directly with smokers' habits. "Once the risks are known everyone who smokes will have to measure them and make up his or her own mind, and must be relied upon, as a responsible person, to act as seems best." [34] The help which the Government were to provide was to be in the form of an Exchequer grant for 50% of local health authorities' expenditure and publicity material supplied by the Central Council for Health Education.

This statement reveals the first formal acceptance by the Ministry of Health of smoking being a major cause of the increase in deaths from lung cancer. The statement was followed by a whole series of suggestions for, and questions about, possible government action, including the banning, or restriction, of smoking in such places as cinemas, buses, trains and aircraft,[35] the prohibition of tobacco advertising,[36] alternative sources of tax revenue.[37] Many of these were contained in M.P's questions to various

Ministers including the Minister of Health, Minister of Transport and Civil Aviation, Home Secretary and the Chancellor of the Exchequer. The position of government on the scientific issue, and on the question of policy was unchanged however, at least until the publication of the first report of the RCP in 1962. Until 1962 action concerning smoking was delegated by central government to local authorities, and it is at the local level that a number of types of action were attempted. The main strand of local action was the health education prompted by the central government, but a number of areas attempted other types of action. For example, in 1958 the first health authority smoking withdrawal clinic was established in Salford[38], and there were other examples of local action from local authorities and from private companies and organisations. These included, for example, experiments with smoking bans in cinemas and theatres,[39] on the lower decks of buses,[40] and further no-smoking accommodation on trains.[41] There were also smoking withdrawal clinics established by independent societies, for example the National Society of Non-Smokers and Smokers Anonymous.[42]

In 1962 the RCP report Smoking and Health[43] was published. This reviewed all the evidence on smoking and health and concluded that smoking was a cause of lung cancer, and bronchitis, and probably contributed to the development of coronary heart disease and various other less common diseases. On the basis of this conclusion the report recommended various preventive measures to reduce the risks to health from smoking, by both reducing the harmful effects of smoking through product modification and by general discouragement of smoking. It further recommended a number of specific measures to be taken by Government to curb the rising consumption of tobacco including: public education of the hazards, more effective restrictions on the sale of tobacco to children, restriction of tobacco advertising, wider restrictions of smoking in public places, higher taxation on cigarettes particularly, informing purchasers of the tar and nicotine content of the smoke of cigarettes, and organisation of anti-smoking clinics. The report received massive publicity and led to a large number of questions being asked in the House of Commons and a statement being made by the Minister of Health, then Enoch Powell. In reply to questions Powell said: "The Government certainly accept that the report demonstrates authoritatively and crushingly the causal connection between smoking and lung cancer and the more general hazards to health of smoking."[44] But, as regards action by the Government, it was announced that the task would fall on local health authorities to make the conclusions of the report widely known and to make clear to the public the dangers to health of smoking. The Government promised guidance and publicity material for the local health authorities and provided central funds for the health education campaign. This action did not represent any change from the policy initiated in 1957, however, the Minister did announce that he was considering the other recommendations made in the report, including anti-smoking clinics and television advertising restrictions. In the years following the publication of the RCP's first report there was much discussion of the smoking issue and some developments in policy leading up to the banning of cigarette advertisements on television in 1965, after attempts to get the tobacco industry to implement voluntary restrictions on television advertising failed.[45]

7.5 Agenda Formation: Medical Experts

The epidemiological studies of Doll and Hill at the MRC's statistical research unit, of which Bradford Hill was the Director, were vital pieces of evidence in the case against smoking.[46] This, and other evidence, was reviewed by a number of expert committees who gave advice to the Minister of Health, or provided recommendations to the Government generally. As seen above, the Standing Advisory Committee on Cancer and Radiotherapy advised the Minister in 1954 that an association had been established between smoking and lung cancer and this he accepted. In 1956 the Minister was advised by the Standing Medical Advisory Committee and the Central Health Services Council that appropriate action should be taken to inform the public of the risks involved in heavy smoking and, in 1957, the MRC provided a comprehensive review of the accumulated evidence on smoking and health, which prompted the Minister to take some action in the health education field. These, then, were some of the official medical advisory channels through which the Minister of Health was pressed to accept the view developing within the medical profession that smoking caused lung cancer.

It may be interesting at this point to mention some of the personalities involved in this advisory process. A key figure involved in the primary research connecting smoking with lung cancer was Sir Austin Bradford Hill, CBE, FRS, a Professor of Medical Statistics at the London School of Hygiene and Tropical Medicine and, later on, honorary Fellow of the RCP. His role, however, was not simply that of researcher. He was, in 1957, a member of the Council of the MRC, honorary director of the MRC's statistical research unit, and chairman of the MRC's statistical committee as well as being a member of the newly formed Aetiology of Lung Cancer Committee. Later, Richard Doll became director of the statistical research unit, also a member of the Aetiology of Lung Cancer Committee. Through his research work Bradford Hill clearly had an anti-smoking perspective and his position within the MRC gave ample opportunity for shaping the advice given to the Government. Another interesting personality is Dr.Horace Joules, MD.Lond, FRCP, Physician and Medical Director of the Central Middlesex Hospital. He was a member of the Standing Medical Advisory Committee and the Central Health Services Council and an outspoken anti-smoking campaigner.[47]

As something of a consensus began to develop within the medical profession, other medical bodies began to take up the anti-smoking cause. Following the MRC statement the British Medical Association (BMA) through it s official organ the <u>British Medical Journal</u> (<u>BMJ</u>) joined in with calls for action against smoking. In an editorial endorsing the MRC statement, the BMJ proposed action to reduce the smoking habit such as the prohibition of smoking in such public places as cinemas and theatres, and the use of, "...all the modern devices of publicity...", to inform the public of the dangers.[48] They also emphasised the role of doctors in persuading young people to avoid starting to smoke.

By 1957 then, something of a consensus had developed within the medical profession with regard both to the technical issue of the interpretation of the relation between smoking and lung cancer and on the need for action. At this stage the MRC, the BMA and the Ministry of Health all accepted that smoking was a major cause of lung cancer and that action of some sort was called for.

The anti-smoking campaign of the medical profession continued with discussions of the evidence and calls for action in editorial comments and letters to journals, as well as in speeches and lectures.[49] Some of these reports and speeches from the medical profession were used as amunition in Parliament to argue for government action.[50] Pressure from the medical profession not only came from professional organisations and individual doctors outside the Government, but also from within the Government bureaucracy. Sir George Godber, the Chief Medical Officer of the Ministry of Health actively supported the anti-smoking campaign, both in his annual reports on the state of the public health and later at conferences and elsewhere.[51]

This pressure for action on smoking culminated in 1962 with the report of the RCP, Smoking and Health.[52] This report has been interpreted, by Friedman, as the single most important document in the smoking and health controversy. As he puts it, the report provided "...a manifesto for action for the anti-smoking campaigners."[53] It also reinforced the growing consensus within the medical profession concerning the interpretation of the scientific evidence. The introduction to the report provides an interesting assessment of the role of physicians in the smoking issue and in preventive medicine generally. The report argues that physicians need to know the facts, to interpret their meaning and then to decide what action they should take themselves, what advice they should give their patients and what policy they should advocate in the field of public health in relation to tobacco smoking. The report also mentions a precedent to the RCP pronouncing on a question of public health when action was required, this was in 1725 when the College made representation to the House of Commons concerning the health consequences of rising consumption of cheap gin.

The RCP report was widely publicised, endorsed by the Minister of Health and accepted as an authoritative statement on the smoking and health issue. Again, the report became ammunition in Parliament for those pressing for more government action against smoking.[54] It is interesting to note that, through the report, the medical experts provided not just an interpretation of the available evidence but specific recommendations for action by the Government and that these recommendations were taken seriously. They provided the basis for calls for action by MPs in Parliament and were put under review by the Ministry of Health.[55]

Sir George Godber, in his annual report for 1962, called the publication of the RCP report, "The outstanding event of the year,"[56] and concerning the interpretation of the medical evidence by the report he said: "This reviewed the evidence dispassionately and, with scrupulous fairness, stated conclusions which were wholly damning against cigarette smoking. There is surely no longer need to argue whether, but only how, the habit should be avoided or broken."[57]

The report prompted much debate in Parliament and began an anti-smoking campaign by the medical profession. The BMJ, for example, joined in the call for action to be taken against smoking by doctors, the Government and local authorities.[58]

7.6 Agenda Formation: The Tobacco Industry

It was in the interests of the tobacco industry to keep the smoking and health issue off the policy agenda and, in this early period, their main tactics in this attempt were to repeatedly call for more research and to criticise the medical arguments. This defence was aided by the economic importance of smoking to Government through tobacco tax revenue.

In 1953 the Imperial Tobacco Company started research into smoking and health in response to the publication of the first results from Doll and Hill's studies.[59] Initially the object of the company's research was to analyse the constituents of its products, to examine the epidemiological evidence for health risks and to provide factual evidence for government and smokers. They claimed that it was not their object either to make medical judgements or to undertake basic medical research.[60] In 1956 the tobacco manufacturers as a group agreed that all information on health should be shared and the Tobacco Manufacturers' Standing Committee (TMSC) was established.

The aim of the TMSC was to promote research into smoking and health and to disseminate information and money to outside researchers. They also gave a sum of money (£250,000) to the MRC to 'solve' the issue of a causal link between smoking and lung cancer.[61] Until 1960 the TMSC concentrated its results on the chemical analysis of tobacco and tobacco smoke, but it was then decided that this chemical information needed to be related to the biological effects. This decision led to the setting up of their own research laboratories in Harrogate in 1962,[62] and in 1963 the Standing Committee changed its name to the Tobacco Research Council, a noticeably more 'neutral' name omitting the word 'manufacturers'. This research organisation became the focus for the industry's criticisms of the medical arguments.

With the announcement in 1954 by the Minister of Health that a relationship had been established between smoking and lung cancer, a group of tobacco manufacturers issued a statement, arguing that there was no proof of any causal connection, and they pointed out a number of problems with a causal interpretation emphasising the role of air pollution in the disease.[63] They pressed the need for more research and, in this context, mentioned their donation of £250,000, which it was later decided should go to the MRC. The £250,000, although offered before the 1954 statement by the Minister of Health, was widely publicised in response to the statement.[64] The sum was to be donated over a period of seven years.[65] To get this sum in perspective it can be compared with some other relevant figures. In the financial year ending 1957 the MRC were spending about £29,000 on the smoking and lung cancer question, most of which came from the tobacco manufacturers' fund,[66] and were spending £327,000 from public funds on cancer research in general. Also in 1955-56 the total net receipts from tobacco duty were estimated at £660m. and this was said to be approximately equal to the cost to the Exchequer of the Health Service national assistance and welfare milk and foods.[67] Between 1956 and 1960 the manufacturers spent £38m. on advertising their goods.[68] In terms of research funding the £250,000 clearly provided the MRC with a major source of revenue for its smoking and lung cancer investigations. However, when compared with the amount spent on advertising, or the Government's tobacco revenue it fades into insignificance.

This was a major tactic for the industry, they questioned the medical evidence, emphasised the need for more research and presented an image of responsible awareness by providing financial support for medical research and by pursuing their own research.

This position was maintained and reiterated in public statements, speeches and through publication of research results over the following years. For example, in a statement to shareholders in 1954, Sir Robert Sinclair, Chairman of Imperial Tobacco, emphasised that there was no proof that smoking is a cause of lung cancer and argued that there had been a lot of exaggerated comment, given the state of scientific knowledge. He mentioned the financial donation of the manufacturers to research to help medical science to discover the true cause or causes of lung cancer.[69]

Again in 1956 at the annual meeting of the Imperial Tobacco Company, Sinclair's statement, read in his absence, pointed out that neither tobacco nor atmospheric pollution could be entirely free from suspicion until medical science had established how lung cancer was caused. He called for more research and fewer premature conclusions, a familiar argument from the scientific controversy. He also mentioned the financial donation from the tobacco manufacturers to the MRC and the fact that Imperial Tobacco, itself, had continued and expanded its own chemical research into the constituents of tobacco and tobacco smoke.[70]

In May 1956, following the Minister of Health's announcement that two cancer-producing agents had been identified in tobacco smoke, the leading tobacco manufacturers issued a statement arguing that the evidence concerning smoking and lung cancer was conflicting and incomplete and that more scientific and medical evidence was needed before the true causes of lung cancer would be established.[71] They emphasised that most of the evidence was statistical and this did not constitute proof. They pointed out various specific problems with the medical evidence and argued that tobacco is a boon to millions of people with both psychological and physiological benefits which needed to be investigated more. They again portrayed a responsible image by referring to their own research and their support for other medical research, and mentioned the "...full sense of our duty to the public".[72]

The tobacco manufacturers were not the only organisations with an interest in maintaining smoking, another group were the thousands of small newsagents and tobacconists, who distribute the manufacturers' products. At the annual conference of the National Union of Retail Tobacconists in 1956 the president also commented on the Minister's statement, saying that what was needed was more research and fewer premature statements.[73] In 1957, at the next annual conference, the president argued that tobacco from contaminated sources was available in Britain and that the medical profession should investigate these contaminents as a possible explanation of the supposed connection with cancer.[74]

With the setting up of the TMSC in 1956, Sir Alfred Egerton,FRS., and Sir Ronald Fisher,FRS., were appointed as scientific consultants in the field of physical chemistry and statistics respectively. Fisher, who was Arthur Balfour Professor of Genetics at the University of

Cambridge and a past president of the Royal Statistical Society, proposed an alternative interpretation of the medical evidence to the causal one. This was the 'constitutional' or 'genetic' hypothesis. These two hypotheses, the causal and the constitutional, have formed the basis of the long running technical controversy identified in the previous chapter and more fully in Reeve.[75]

In the TMSC's first annual report in June 1957,[76] just before the release of the MRC's statement, the committee argues that the two carcinogenic agents discovered in tobacco smoke are present in such small quantities as to pose no threat to health. Mr.G.F.Todd, the committee's statistical advisor, pointed out contradictions and limitations of the evidence in the statistical material relating smoking and cancer. He argues that no-one was in a position to know how far the statistics were applicable to the whole population.

In response to the statements by the MRC and the Minister of Health in 1957, the TMSC argued that, at that stage, any conclusions concerning the existence and extent of a causal connection between smoking and lung cancer were simply a matter of opinion.[77] They pointed to problems with the evidence and suggested that the assumptions underlying the MRC's estimates of relative lung cancer rates of heavy smokers and non-smokers were open to criticism. They also argued that the possibility that the statistical association between smoking and lung cancer might be of a non-causal nature had not been adequately investigated. Later in the same year the TMSC published six grounds for doubting whether smoking is a significant factor in lung cancer in an interim report.[78] These were familiar arguments from the scientific debate. In their 1958 annual report, the TMSC argued that the traces of benzpyrene which had been detected in cigarette smoke were so small as to be very unlikely to affect health.[79] However they were conducting experiments to discover means of reducing the benzpyrene traces.

In 1959 the TMSC donated £15,000 to the Royal College of Surgeons of England to promote research into lung cancer.[80] In their annual report of that year, they argued that considerable attention had been paid to the possible harmful effects of smoking, but much less had been paid to the benefits it may confer.[81] They suggested that the acceptance of tobacco may reflect the fact that it aids in contending with stress and, furthermore, the reasons for smoking needed elucidation. The committee established a research post for the study of the relationship of emotional factors to physical diseases and Dr.D.M.Kissen, research associate in psychosomatic medicine in Glasgow Universitity, was appointed to the post. Among other work they had financed were investigations into the possible role of genetic factors in influencing smoking habits and they reported findings consistent with the view that genetic factors influence smoking habits.

During this period then, the manufacturers, through their research organisation, were criticising the medical arguments, supporting research relating to the genetic hypothesis, as well as that relating to possible product modification, and providing funds for general medical research on the smoking and lung cancer issue, and constantly calling for more research. At the same time Sir Ronald Fisher, the TMSC's scientific consultant, was vigorously defending the genetic hypothesis in medical journals and in wider media, while attacking the causal hypothesis and calling for more research and fewer premature conclusions.[82]

Alongside this use of scientific arguments concerning the health issue, the industry also began to argue against government action on other grounds. While the TMSC/TRC concentrated on the scientific arguments, the Tobacco Advisory Council (TAC), an industry organisation dealing with trade matters, concentrated on defending the industry against possible government action, using both factual and moral arguments relating to the possible effects of different types of action. For example, the RCP published, as an appendix to its first report, some arguments which the TAC had sent to them along with some data which they had been collecting.[83] These arguments primarily concerned tobacco advertising and were aimed at defending the amount and nature of this advertising against calls for its restriction. The arguments mainly dealt with the effects of advertising.

The industry's reaction to the RCP report reflects the use of these two types of argument, the scientific one questioning the medical case for action by disputing its interpretation of medical evidence, and the economic and moral one arguing against government action as unnecessary or unjustified. The TMSC issued a statement on the RCP report criticising its conclusions.[84] The Standing Committee, after mentioning the contribution of the tobacco manufacturers to research on smoking and health through the TMSC, goes on to criticise the RCP report on a number of grounds. Their first criticism is that the report lacks novelty. They maintain that it adds little evidence to that quoted by the MRC in 1957. They also argue that, while the RCP committee was set up to consider the effects on health of both smoking and air pollution, by dealing with the two issues in separate reports they have produced an incomplete assessment of the problems. The Standing Committee further argues that, "There is a growing body of evidence that smoking has pharmacological and psychological effects that are of real value to smokers."[85] Finally, they conclude that more research is needed, "The main unspoken lesson of the report is the need for far more intensive research."[86] They go on to describe three areas of research which they see as "...both practical and essential."[87] These are: studies of environment and personal characteristics, as well as past medical histories of lung cancer and bronchitis sufferers; studies into the constituents and effects of air pollution since there is much evidence to suggest striking differences in the incidence of lung cancer and bronchitis between urban and rural areas, and between different countries reflecting to some extent the effect of air pollution; and finally they argue that further investigation is needed into the chemistry and biological effects of tobacco smoke. The aim of this latter type of research would, according to the TMSC be to attempt to identify any substances in tobacco which may be injurious to health so that they can be eliminated or minimised. This research would be one of the primary aims of the laboratories at Harrogate when they came into operation. The statement concludes by saying that the TMSC did not feel that a general condemnation of cigarette smoking was a constructive approach to the problems, and they quote from the RCP report that, "...most smokers suffer no serious impairment of health or shortening of life as a result of their habit."[88]

Some of the recommendations of the report were criticised by the Tobacco Advisory Committee (TAC) who represent the industry in general trade matters. They argued that a further increase in tax on cigarettes in Britain would be inequitable and they criticise the proposal to put figures on cigarette packets showing the constituents of smoke on the grounds that the public would be misled into thinking that brands with less of the substances were somehow 'safer' than others. They said that, "In the present state of scientific knowledge the manufacturers would be strongly opposed to the adoption of this proposal."[89]

In this early period of the smoking controversy, the interest of the tobacco industry in keeping smoking off the policy agenda must have been considerably aided by government's own interest in maintaining smoking. With such a large tax revenue from tobacco, the Treasury at least would be very reluctant to see cigarette smoking reduced. This was illustrated in a comment made by the Chancellor of the

Exchequer in 1957 which caused some controversy in Parliament. He had said, in a public speech, that the Treasury did not want too many people to stop smoking.[90]

7.7 Summary

For the controversy over smoking and health, the period 1950 to 1962 can be seen as the stage of agenda formation. During this period members of the medical profession, particularly epidemiologists, were using technical arguments in an attempt to get smoking onto the political agenda. In the terminology of Van der Eijk and Kok smoking was, for them, a 'demand'.[91] On the other hand a statistician and geneticist and a psychologist with a predilection for genetic interpretations, who both had links with the tobacco industry, were using technical arguments in an attempt to keep smoking off the political agenda. At this stage there was dissensus both in the policy arena and in the scientific arena. Furthermore rival policy options, that smoking was an 'issue' to be acted upon or that smoking was not an 'issue', were tied directly to rival interpretations of data or rival theories. Thus, at this stage, the policy debate was equivalent to the technical debate.

In the process of influencing the government during the agenda stage, the medical profession were at an advantage with direct access to the Ministry of Health through formal advisory channels.

While the medical experts had the advantage of formal advisory channels to the Ministry of Health, the tobacco industry had the advantage of the Government's, and especially the Treasury's, own considerable interest in keeping smoking off the agenda.

In addition to these formal routes for advice, members of the medical profession and medical institutions involved themselves in public campaigns against smoking. The question of a link between smoking and lung cancer was initiated by epidemiological research and the medical profession pursued this question as a 'demand'. Through the recommendations of medical advisory committees, the demand finally reached issue status as the Government formally accepted the medics' causal interpretation of the smoking/lung cancer link.

CHAPTER 8

SMOKING AND HEALTH:

THE DEVELOPMENT OF POLICY

8. SMOKING AND HEALTH: THE DEVELOPMENT OF POLICY

8.1 Introduction

With smoking on the policy agenda, what influence has the scientific controversy and experts had on the development of policy and in keeping the issue on the agenda? This is a very complex problem since the history of the development of smoking policy involves a range of issues, actors and influences.

Some general comments on the style of British policy-making in this field may be useful. The observed tradition in Britain of favouring government-industry negotiations to the use of legislation [1], has been a significant factor in policy-making on smoking. Furthermore, the structure of government decision-making has probably influenced both the outputs of policy-making and the tactics of the various pressure groups involved in the issue.[2] In Britain there are only two pieces of legislation concerning smoking, and one of these, the law preventing children under sixteen years of age from buying cigarettes, is of historical interest only, not related to the scientific debate on the health effects of smoking,[3] while some other countries, Norway for example, have comprehensive legislation attacking the problem.[4] One interpretation of this difference is that the countries with comprehensive legislation have shown a marked degree of co-operation and co-ordination between different government departments.[5] An important factor to bear in mind, then, is the question of co-ordination within the Government on the smoking issue.

Another general point concerning smoking policy is the international nature of the whole issue. Policy in Britain must, to some extent, have been influenced by policy developments in other countries, by the international nature of the tobacco economy and tobacco companies, and through the international co-operation of medical authorities both in research and in pressing for action on smoking.

It is clear that the development of smoking policy in Britain has not been a simple matter of accepting the medical evidence, heeding the advice of medical experts and curbing cigarette smoking, using legislation and health education in a co-ordinated attack on tobacco advertising, promotion and sponsorship, smoking in public places and by young people, as the 'Utopian Rationalism', which Ezrahi criticises,[6] might lead us to expect. The development of smoking policy has, instead, been pragmatic, concentrating on voluntary agreements between the Government and the tobacco industry, and determined more by the economic importance of tobacco for the Government and the industry than by the medical arguments. The way that policy has developed has clearly been influenced by the fact that, though the smoking and health issue is of primary concern to the Department of Health and Social Security (DHSS), a number of other government departments have strong interests in the issue, including the Treasury, Customs and Excise and the Department of Trade, and these departments have very different perspectives on the issue.

An analysis of the role of the various pressure groups involved in the issue, including Action on Smoking and Health (ASH), in the development of smoking policy has been provided by Popham.[7] His study demonstrates the primacy of economic issues in the debate and the importance of the relative influence of the various groups. He notes that the tobacco industry, through its regular contact with the Government for negotiating the voluntary agreements, has a major political advantage. To avoid covering the same ground again, this study will concentrate particularly on the types of argument used in the various forums which influence policy. But the relative effect of these different forums and the power of different groups involved both in influencing and in formulating policy may be of crucial significance in discussing the role of experts and scientific advice. Frequent references to Popham's work will therefore be necessary.

A key question of interest to the general study of experts in policy is the effect which the nature of the scientific or technical problem has on policy. The issue of the health effects of smoking may be described as a trans-scientific problem, following Weinberg.[8] As many of the participants in the scientific debate have admitted, to conclusively prove a causal relationship between cigarette smoking and lung cancer, chronic bronchitis and coronary heart disease, what is neededis a long term and large scale experiment on humans.[9] However, for ethical and practical reasons, this experiment would not be acceptable, since large numbers of people, and randomly selected, would be required to smoke various amounts and, if it were conclusively shown that smoking causes disease and premature death, this would amount to killing large numbers of people. Of course the experiment would be practically impossible anyway since a very large number of people would need to be used as subjects and it would be impossible to get this many people to co-operate in such an experiment. Furthermore, the experiment would need to run for something of the order of fifty years at least because of the long latent period which cancer can have. Finally, the experiment would be prohibitively expensive in both financial and human resources. It should also be pointed out that in a philosophical sense no experiment can conclusively prove a causal relationship. This need not cause any problem for the concept of Trans-Science though, since the concept of a conclusive experiment can be, and is, usefully employed in an instrumental way within science. Of course, with strong enough motivation to do so, even the conclusions drawn from an experiment such as the one described above could be disputed. The point here is that this type of experiment is what would 'normally' be regarded as the strongest evidence for this type of problem, and in the smoking and health case it is ethically and practically impossible to provide this type of evidence.

The smoking problem is a complex issue involving various diseases and different theories of the aetiology of disease. It is a trans-scientific question and must depend heavily on statistical evidence. This reliance on statistics has meant that the work of the epidemiologists on smoking was open to criticism from statisticians.

The issue is many facetted as well, in that there are questions of association, of causation and of mechanism of causation involved.

Given the complexity of the smoking question, and the reliance on statistics, it is not surprising that there may be some scientific controversy over the issue. It may further be expected that this controversy may directly affect the development of policy concerning smoking. We have seen something of the effect of the early controversy in getting smoking onto the policy agenda, but how has it affected the actual development of policy?

8.2 Policy Development: The Government

From the publication of the first report of the Royal College of Physicians (RCP)[10] onwards a new phase in the development of the smoking issue was entered. In this period there has been very little debate in the policy arena over the scientific question of a causal link between smoking and lung cancer and various other diseases. Governments have, publicly at least, accepted the interpretation of the evidence given by the medical profession, that smoking is a major cause of lung cancer and other diseases. Alongside this acceptance of the medical case has been a broadening of policy to include more than just health education by local authorities. A further point concerning the development of smoking policy is that it has not been a party political issue. Anti-smoking campaigners have received both support and opposition from

Members of Parliament from both the major parties.

The public acceptance of the medical argument is illustrated both in Parliament through statements from Government officials and during debates and, as far as can be gathered, in the course of the negotiations between the Government and the tobacco industry. For example, during a parliamentary debate in 1966 in which Dr.John Dunwoody, a Labour MP and later Director-General of ASH and a Parliamentary Under-Secretary at the DHSS, presented the medical evidence for a causal link between lung cancer and cigarette smoking urging Government action to reduce cigarette consumption.[11] The arguments which came out during the debate centred more around the legitimacy of Government action and the specific types of action than around the medical evidence. One argument which came close to questioning the medical evidence came from W.A.Wilkins, the Member for Bristol South, with a large constituency interest in smoking with a cigarette factory in his constituency. Briefed by the Imperial Tobacco Company, Wilkins argued that, although smoking may cause lung cancer, there are many other factors involved. His main argument was, though, that the tobacco industry was being victimised by an obsessive anti-smoking lobby.

In the same debate the Minister of Health, Kenneth Robinson, said:[12]

> "I think we must take this relationship between cigarette smoking and lung cancer and these other diseases as accepted scientific facts, even though we do not as yet know exactly how the effects are produced. Our concern is to consider what action the Government and the public at large can, and should, take in the light of these facts."

The debate concluded with the resolution: "That this House, believing that the great majority of deaths from lung cancer are caused by cigarette smoking, urges Her Majesty's Government to take all possible steps to reduce cigarette consumption..."[13]. Throughout the debate the RCP and the Government's Chief Medical Officer were held up as authorities on the scientific evidence.

Statements made in the House of Commons following the publication of the RCP's second report concentrated on the nature of government action and, among other things, revealed that an inter-departmental committee had been set up to discuss the implications of the report.[14] While giving an account of the progress of the latest voluntary agreement, the Secretary of State for Social Services, Sir Keith Joseph, re-emphasised that the Government's concern was to inform the public of the dangers of cigarette smoking.[15]

In 1976 there was a debate in the House of Commons on a Private Member's Bill in which Kilroy-Silk, a Labour backbencher, proposed that tobacco and smoking substances should be classified under the Medicines Act 1968 to allow control of them as hazards to health.[16] The arguments centred on the appropriateness of government legislation and of the particular Act in question as the one for the job. Kilroy-Silk suggested that the medical case was not in dispute and quoted the RCP reports and those of the World Health Organisation (WHO) as authoritative statements on the health hazards of smoking, also mentioning the Government's own estimates of the number of deaths attributable to smoking. Dr.David Owen, then Minister of State for the DHSS, referred to the work of Doll and Hill, the two RCP reports and the reports of the U.S.Surgeon General and the WHO, and called cigarette smoking, "...a major public health hazard".[17] None of the opponents to government legislation seriously questioned the medical evidence. Although they tried to suggest that smoking in moderation may be all right, they did not support it with scientific evidence and still admitted that smoking is hazardous. Their main argument was that legislation is not the way to deal with the problem. In the debate one Member referred to the "...original rather feeble attempts to dispute the scientific evidence," which he said, "...have now petered out".[18]

In 1980 a Parliamentary Debate resolved that: "This House recognises the cost in personal suffering which arises from cigarette smoking and calls on Her Majesty's Government to make new initiatives..."[19]. Again this debate centred on the issue of the type of action to be taken by Government, particularly avoiding the use of legislation.

In Parliament, debate has centred on the proper role of Government and the particular types of action that should be taken. Many Members have claimed that the medical case is proven and quote as their authorities the RCP, the Chief Medical Officer of the DHSS, the U.S.Surgeon General and the WHO. Officials from the Health Department have confirmed successive Governments' acceptance of the medical arguments. Members opposing various actions by the Government, who have constituency interests in tobacco or are briefed by the industry, have not strongly argued against the medical evidence but have opposed legislation and other action on the grounds of individual liberty, the effect on Government revenue and the responsible attitude taken by the industry.

The impact of Parliament on policy is very difficult to assess, but it clearly has some effect and, as Popham has pointed out,[20] by making statements and calling for debates in Parliament, Government officials can influence the progress of the negotiations with the industry and, in this context, some influence may be felt.

In Parliament there have been a number of Private Member's Bills introduced concerning the control of cigarette smoking.[21] For example in 1964 Mr.L.Pavitt, Labour Member for Willesden West, brought in a Bill which proposed that cigarette packets should carry a warning of the health hazards arising from smoking.[22] In the debate following the first reading of the Bill, Mr.Hogg the Lord President of the Council and Minister for Science emphasised that the problem was not a scientific one but was political. Any decision, he said, revolved around the question of how far action should go in a free society, "...but I am confident that we know enough about the scientific and social basis of this thing to realise that the issues of policy we have to face are not scientific problems."[23] These Bills have all been rejected and in this sense have failed to bring action against smoking. However, as a forum for keeping the smoking issue alive, they may have had some significant effect and threats of legislation such as these may have provided a significant input to the negotiations with the industry. The fate of these Bills has been determined, not by disagreement over the scientific rationale for their proposals, that is the medical case against smoking, but through the power of the tobacco lobby, attitudes of Ministers, the progress of voluntary agreement negotiations[24], and through a general attempt to avoid legislation.

The department with primary responsibility for the smoking and health issue is the DHSS,[25] though other departments also have interests in the issue. The DHSS have been involved in negotiations with the tobacco industry to produce voluntary agreements concerning various aspects of cigarette sales. These negotiations and agreements have been the central plank of Government policy towards smoking and this makes it very difficult to assess the role of scientific knowledge and experts in policy since the negotiations are relatively inaccessible. However, as Chief Medical Officer, the DHSS's senior adviser on health, Sir George Godber was a vigorous opponent of cigarette smoking both through statements in his annual reports and through active participation in international conferences on smoking and health.[26] Furthermore, successive Ministers of Health and Secretaries of State have confirmed the Health Ministry's acceptance of the medical case. It can be assumed then, that the negotiations with the industry, through its trade organisation the Tobacco Advisory Committee, have centred on appropriate and acceptable ways of informing the public of the health hazards of smoking, reducing cigarette advertising and restricting its content, providing information about the contents of cigarettes, and developing tobacco substitutes and additives. This assumption is further supported by the content of the agreements which have come out of these negotiations and further evidence on the type of arguments used during the negotiations will be provided in the discussion of the industry's response.

Concerning the content of the agreements and of Government policy concerning smoking generally, it is interesting to discuss the influence of medical experts on the types of policy adopted. As noted above, discussions in Parliament have taken the RCP as well as the WHO, the U.S.Surgeon General and the Government's Chief Medical Officer as authorities on the scientific aspects of the smoking debate. Furthermore, MPs have used the RCP's recommendations for action to press the Government on smoking policy. Not only have the RCP's conclusions concerning the scientific evidence been accepted by the Government but also their recommendations for action have been taken seriously, put under review, and in some cases implemented by them. This can be illustrated by looking at the reaction to the three RCP reports and by looking at the recommendations made in them in comparison to the action taken by the Government through the voluntary agreements.

In response to the 1962 RCP report, Enoch Powell, then Minister of Health, stated in Parliament that: [27]

"...this report is undoubtedly an extremely valuable and powerful weapon in the hands of health education which the Government will now be actively supporting. The other suggestions made in the report of the Royal College are under consideration by the Government."

The first report recommended: more public education on the hazards of smoking; more effective restrictions on the sale of tobacco to children; restriction of tobacco advertising; wider restriction of smoking in public places; an increase of tax on cigarettes, perhaps with adjustment of the tax on pipe and cigar tobaccos; informing purchasers of the tar and nicotine content of the smoke of cigarettes; investigating the value of anti-smoking clinics to help those who had difficulty in giving up smoking.[28] Apart from his general statement on considering the RCP recommendations, the Minister of Health announced that he was providing support for the local health authorities in their health education campaign and he was considering running experiments with anti-smoking clinics.[29]

Following the report, the tobacco industry voluntarily agreed with the Independent Television Authority(ITA) to restrict cigarette advertisements on television until after 9p.m. and agreed to restrict the content of all cigarette advertisements to comply with a code suggested by the ITA to exclude references which may appeal to the young and which over-emphasised glamour.[30] However, failure to comply with this voluntary agreement ultimately led to the banning of cigarette advertising on television with the TV.Act 1964, implemented in 1965.[31]

The second RCP report recommended various actions to be taken by the Government: consultation with the broadcasting authorities and the newspaper industry on more thoroughly informing the public of the dangers of smoking; better means of educating children about smoking; regulations forbidding the sale of cigarettes to children should be strengthened and cigarette vending machines should be removed from public places; advertisements of cigarettes and gift coupon schemes should be prohibited; more restrictions on smoking in public transport and places of entertainment should be enforced; life insurance companies should consider reduced premiums for non-smokers; warning notices should be printed on packets of cigarettes and, if they are allowed to continue, on cigarette advertisements; more effective techniques for helping unwilling smokers to stop must be developed in special research clinics and smoking-control clinics should be established; amounts of tar and nicotine in all marketed brands should be published and a public statement made on the possible health effects of smoking them; the Government should consider imposing statutory upper limits to the nicotine and tar contents of cigarettes; the MRC should consult with the tobacco industry with regard to tests of cigarettes which are likely to be less hazardous and should conduct research to determine the effects on health of smoking such cigarettes; those who continue to smoke should be encouraged to adopt less dangerous smoking habits; differential taxation of different types of tobacco should be imposed to discourage more hazardous forms of smoking; an official inquiry should be made into the economic consequences of present smoking habits and of the results of a general reduction in cigarette smoking.[32]

Following this report, the Prime Minister announced that an inter-departmental committee had been set up to discuss all its implications.[33] The negotiations for the voluntary agreement reached in 1971 were begun before the publication of the RCP report, but were concluded after it. The agreement contained the following elements: a health warning was to be printed on all cigarette packets for sale in Britain; all advertisements in the press and other publications and on hoardings were to carry a reference to the health warning; a standing scientific liaison committee would be established comprising of members appointed by the industry and the DHSS to review research into less dangerous forms of smoking, and to devise a method for publishing the tar and nicotine content of cigarettes.[34]

The Secretary of State for Social Services, Sir Keith Joseph, also announced that he was suggesting to hospitals and local health authorities ways in which they could discourage smoking.[35] He was also considering, with the Secretary of State for Education and Science, the problem of discouraging the young, and was appealing to those responsible for public transport, cinemas, theatres and the like to set aside more accommodation for non-smokers. He was inviting life insurance companies, in the light of the RCP report, to consider the possibility of lower premiums for non-smokers. He was also hoping to enlist the help of those responsible for television programmes to help make the dangers of cigarette smoking widely understood, and he proposed to supplement the work of the Health Education Council by making extra money available for an intensive television campaign.[36]

In the following year the voluntary agreement was extended and the industry agreed to include health hints on cigarette coupons.[37] These came directly from the RCP's recommendations for less dangerous smoking habits to be encouraged among persistent smokers. They also agreed: to cover up specific brand advertisements at televised sporting events, to ensure that all brand advertisements at sporting events carried a health warning; to ensure that cigarette advertisements in cinemas and those sent through the post carry a reference to the Government health warning.[38] Of the 1971 agreement, Sir Keith Joseph said: "The Government think that the agreement is a very proper first reaction to the report of the RCP".[39]

In 1973 the DHSS produced a pamphlet giving a review of the economic consequences of a reduction in cigarette consumption.[40] In September 1971, after the publication of the Royal College report, the Consumers' Association published a table showing the tar and nicotine content of a number of leading cigarette brands[41], and in 1973 the first Government tar and nicotine league tables were published, giving the yield of all cigarettes on the British market on the recommendations of the Cohen Committee.[42] Also in 1973 the standing liaison committee was replaced by the Independent Scientific Committee on Smoking and Health (the Hunter Committee), under the chairmanship of Lord Hunter of Newington. The committee was to advise on scientific matters concerning smoking and health, but its first priority was to produce guidelines for, and to adjudicate, the testing of tobacco substitutes and additive materials.[43] The first significant increase in tobacco tax came in 1974, and a new voluntary agreement was reached in 1974-5.[44]

In 1977 a new Royal College report was published[45] and this made recommendations concerning public education, the health services, smoking in public places, tobacco sales promotion, price of tobacco goods, limitations of tar/nicotine levels and research. These included a recommendation that a special smoking research unit should be established to facilitate research into ways of encouraging people not to smoke, the pharmacology and psychology of dependence, less harmful forms of smoking and the detection of people who are especially liable to be harmed by the habit.

The voluntary agreement made in the same year, came out before the Royal College report, but a Government White Paper on Prevention and Health[46] published in 1977, although intended as a reply to the recommendations made by a sub-committee of the Expenditure Committee in their report on Preventive Medicine,[47] specifically discussed a number of the College's recommendations. These included the suggestion for a maximum permissible tar yield, tax differentiating against more harmful cigarettes with high-tar yields, vigorous joint effort by health and education authorities to educate young people about the dangers of smoking, smoking in health premises, the recommendation for courses of further research. Finally, the Paper announced that the Government proposed to discuss with the MRC the need for a special unit, such as the RCP had recommended.[48]

It is clear that the various recommendations for action which the RCP proposed in its three reports have been, at least, considered by the Government and in some cases implemented. As seen above, there have been numerous direct references to the College's recommendations and the Government has put each report under consideration. Furthermore, the type of action that has been taken through the voluntary agreements has, to some extent, reflected the RCP's recommendations.

It is clear that there is a link between Government action concerning smoking and the recommendations for action made by the RCP through its three reports. These recommendations have been discussed in Parliament and reviewed by the Government, and some have been implemented through the voluntary agreements. Of course, not all these recommendations may originate with the RCP, and it is clear that some courses of action have been under discussion between the Government and the industry before the Royal College have published them. The RCP provide a review of recommendations just as they do of the scientific evidence, and these recommendations are based on evidence from studies and surveys which have already been published. So, possible types of action can come from studies, from experience in other countries and from other authoritative bodies such as the WHO. But the RCP have provided a crystalisation of these possible courses of action and although this has not been the only input into Government decision-making it clearly has been an input. It should perhaps also be pointed out that, although various Governments may have put the RCP recommendations under consideration, they have clearly favoured only some courses of action and have rejected others.

If the Health Department has accepted the medical case and the argument for some action to be taken by Government, the question still remains of what lines other departments with an interest in smoking have taken, and what attempt has there been to co-ordinate the various actions of different departments with respect to smoking? Another question related to this is who exactly within the DHSS has had responsibility for the smoking issue?

These questions are closely related. In 1970 a Senior Medical Officer was appointed in the DHSS to co-ordinate the work of various departments on smoking and health.[49] By 1973 she was no longer working full-time on problems of smoking and health.[50] The problem of co-ordination is compounded by the lack of continuity within a department arising from the turnover of different Ministers and Secretaries of State. A further discontinuity problem specific to the Health Department arose when in 1968 the Ministry of Health was subsumed under the new Department of Health and Social Security. Some examples of the problems arising from these discontinuities are given in Popham.[51]

It has been noted that the WHO recommended that each government should establish a central committee or other appropriate body to co-ordinate specific programmes for the control and prevention of cigarette smoking.[52] Various countries have complied with this advice. For example in the USA there is the National Clearinghouse for Smoking and Health, and the National Interagency Council on Smoking and Health. The former is a body established by the US Public Health Service and set up within the Department of Health, Education and Welfare. It provides the focal point of governmental action against cigarette smoking in the US., its aims being; to reduce the number of people smoking cigarettes, to reduce the number of young people taking up cigarettes, and to help develop less hazardous smoking for those who do not wish to quit, or are unable to do so.[53] The National Interagency Council was set up in 1968 to facilitate the development of a smoking and health coalition of national health interest groups with the help of three government agencies.[54]

In Britain there is no such body within the government bureaucracy. One attempt at coordination seems to have been the appointment of a Senior Medical Officer in the DHSS, mentioned above. Although the impact of this on policy is unclear, since the atempt was shortlived, it is doubtful that much impact was felt. The real focal point for the smoking issue in Britain has been Action on Smoking and Health (ASH), a group established by the RCP with the publication of its second report.[55] This group have been largely funded by the DHSS and Popham has suggested some of the functions performed by ASH which may be helpful to that department, for example, monitoring the voluntary agreements and providing an educational role.[56] Although ASH clearly provide coordination for the anti-smoking fight, there seems to have been little or no formal coordination between government departments.

One example, interpreted by some as the only known formal example,[57] of co-ordination of policy on smoking was an inter-departmental committee set up in 1971 to consider all the implications of the second RCP report.[58] The committee consisted of officials from the DHSS, the Scottish Home and Health Department, the Treasury, the Department of Trade and Industry, Customs and Excise and the Central Statistical Office and was chaired by a Cabinet Office official. Their report, written in 1971, was never published.[59] It recommends a large-scale education campaign on the hazards of smoking but reveals the conflict between health considerations and economic ones. The report states that a fall of twenty or forty per cent in cigarette smoking would result in a substantial reduction in premature loss of life and smoking-related diseases and that the reduced suffering cannot be quantified. But the economics of such a reduction are also calculated, taking into account the cost of retirement pensions paid to the 100,000 people who would live longer, an increase in domestic demand encouraging imports, adversely affecting the balance of payments and causing a net loss of tax revenue since the tax component on general items of consumer expenditure is smaller than it is for cigarettes. The report also discussed the effect of increasing tobacco duty, and argued that a reduction in cigarette consumption would not result in substantial unemployment, though areas of traditionally high unemployment would be worst hit. The powerful economic arguments clearly won the day since the committee could not agree on a co-ordinated policy to discourage smoking.[60] They did recommend a large-scale health education programme on the dangers of smoking and mentioned that cigarette promotion might hinder the aims of this programme. However, even this recommendation went unheeded. Where the report recommended spending £3 million for a number of years - about £8.4 million at 1980 prices, the total spent on anti-smoking advertising in 1979 by the HEC and ASH was about £400,000. This compares with about £80 million spent per year by the industry on cigarette promotion.[61]

To judge from the report of this inter-departmental committee, the Government have accepted the medical arguments but economic considerations have outweighed the calls for action against cigarette smoking. As Popham has pointed out, this conflict between health considerations and economic interests is institutionalised in inter-departmental conflict between the health department and the economic departments.[62] The ambiguity of the Government's position on smoking and the inter-departmental conflict is highlighted by the fact that where, on the one hand the DHSS have provided funds for ASH and the HEC have devoted a large proportion of their resources to anti-smoking campaigns, on the other hand the Department of Industry has paid out large sums of money to tobacco firms in the form of re-equipment and regional development grants.[63]

In 1975 it was announced by the then Minister of Health, Dr.David Owen, that in the Government's view what was needed to deal with smoking and its health-related problems was machinery analogous to that provided for in the Medicines Act 1968 which would enable action to be based on advice from an expert and independant advisory committee after consultation with interests likely to be affected.[64] Owen further announced that he was starting consultations with the tobacco industry, the Medicines Commission and the Independant Scientific Committee on Smoking and Health about these plans. This move would bring many aspects of smoking policy under legal control, including the use of substitutes and additives, reduction in yields of tar, nicotine and carbon monoxide, health warnings and information on advertisements and packets, the restriction of promotion and codes of practice for advertising and sponsorship. A suggested benefit of this policy proposal was that it would 'depoliticise' the smoking and health issue and thus reduce the fluctuations in interest depending on who is Minister of Health.[65] The idea was probably to produce a more rational approach to smoking policy.

In 1976 the DHSS published a consultative document on Prevention and Health[66] which discussed, among other things, smoking policy. Re-iterating the department's position on the medical case the document said, "No one can seriously doubt any longer that the habit of cigarette smoking has been directly responsible for an enormous amount of preventable disease and untimely death in this country."[67] The document went on to discuss policy and identified a need for other 'lines of action' than just health education. It mentioned less dangerous cigarettes and the use of tobacco substitutes, and discussed the possibility of stricter controls over cigarettes referring to the comprehensive legislative approach adopted in Norway under the Tobacco Act.[68] Referring to the evidence from the study of British doctors the document argued that a reduction in cigarette consumption would be reflected within a relatively short time in a reduction in smoking-related diseases. The document repeated the Government's proposals to bring smoking under the ambit of machinery analogous to the Medicines Act.

In discussing costs and benefits of a reduction in smoking, the document provides a counter argument to the 'cynics' who argue that prevention leads to greater expense through prolonging the lives of people through the age of dependance when they must be supported from public funds.[69] The document argues that prevention is valuable even if it is expensive, and that prevention increases the active earning life of individuals and postpones the cost of medical care which can be of substantial economic value. Bearing in mind the deliberations of the 1971 inter-departmental committee, the 'cynics' referred to could well be officials from the Treasury and other economic departments.

In 1976, as mentioned above, Kilroy-Silk brought a

Private Member's Bill into the Commons which proposed that tobacco, tobacco substitutes and additives be brought under control of the Medicines Act (1968). In the debate on this Bill Dr.Owen announced that the Government intended to bring tobacco substitutes and additives under the Medicines Act, though not tobacco itself.[70]

In 1975 the Social Services and Employment Sub-Committee of the Expenditure Committee began an inquiry into preventive medicine, and tobacco was one of a number of case studies investigated.[71] They interviewed representatives from the DHSS, the BMA and the TRC and TAC. In the report of this inquiry the sub-committee stated that, "None of our witnesses doubted that cigarette smoking was the main cause of lung cancer,"[72] and further said that cigarette smoking is accepted as being a major factor leading to at least three diseases, heart disease, lung cancer and chronic bronchitis. In the sub-committee's words, "We, like successive governments, accept that smoking is dangerous."[73] They also stated that most witnesses believed that reducing smoking, particularly cigarette smoking, should be one of the most, if not the most important target of a campaign to improve people's health. They go on to mention the 'less than impressive' results of the policy of successive governments working in co-operation with the tobacco industry, and they discuss the stronger action taken by Norway and Finland.[74]

The report concluded with a series of statements on the arguments presented, for example they state that from a health point of view they would like to see the habit 'disappear overnight', but admit that this is not politically possible. They believe that cigarette coupons tend to increase consumption and are not impressed with the argument that advertising is directed only to encourage 'brand-switching', and believe that encouragement to the addicted smoker to turn to lower-tar brands can be delivered at the point of sale. Following these and other statements they make a ten point list of recommendations, including legislation to bar tobacco advertising except at the point of sale, annual increases in duty, abolition of cigarette coupons and stronger health warnings.[75]

In 1977 the Government published a White Paper whose function was to respond to the sub-committee's report and to explain Government policies on smoking and health.[76] In the White Paper it is stated that: "The Government's main objectives are to discourage people, particularly the young, from starting to smoke cigarettes; and to encourage existing smokers to stop."[77] But, on the means of bringing this about, it is stated that, "The Government's view is that present trends towards a non-smoking society can be sustained and reinforced by a consistent strategy of education and persuasion of the individual and co-operation with the tobacco industry."[78] The paper discusses both the recommendations of the sub-committee and some of those from the Royal College's third report. The most powerful of the sub-committee's recommendations, that legislation should be introduced to ban the advertising of tobacco and tobacco products except at the point of sale, that cigarette coupons should be abolished, that cigarette machines should be available only on premises to which children do not have ready access and that there should be a stronger health warning on packets and tins, were all rejected by the Government. The only recommendations which were accepted without reservation were that there should be stricter control of advertisement through sponsorship, which was already the subject of discussion between the Government and the industry and that the present trend to provide non-smoking areas in public places should be encouraged to continue.[79]

In discussing the proposal to bring tobacco substitutes and additives under the control of the Medicines Act (1968), the White Paper stated that the 'vires' of doing so have been questioned. In the March 1977 agreement with the industry it was recorded that the government powers to control substitutes and additives under this Act would be used only if the need arose.[80]

The move to bring substitutes and additives under the Medicines Act was never taken, though Popham points out that the threat of it may have significantly influenced the 1977 voluntary agreement persuading the industry to make concessions.[81]

To return to the issue of what type of arguments have been used in this period of policy development, the situation seems clear. The health arguments have not been challenged on the basis of medical or statistical evidence, but have been outweighed by economic considerations. What controversy there has been has centred around arguments over the effect of cigarette advertising on consumption, possible effects of an increase in tobacco duty, the function of cigarette coupons, and other issues connected with the economic or commercial side of cigarette consumption.

These arguments are illustrated in the various official documents discussed above. The Social Services and Employment sub-committee made clear statements on their position concerning the various arguments put forward by the industry, by the medical profession and by government officials concerning cigarette advertising, cigarette coupons and so on. For example, in a twelve point summary of their position on these arguments only one point refers to the medical argument that smoking is harmful, and with this they agree.[82] The other points include judgements that revenue and employment are not more important than health, that pricing up cigarettes will affect those least able to afford it but that this is a way of encouraging people to give up smoking. They don't accept the argument that advertising only affects brand loyalty, and they believe that cigarette coupons tend to increase consumption. The other points refer to sports and arts sponsorship, deterrants to giving up smoking, and health education. They also state that they welcomed the Government's intention to extend the provisions of the Medicines Act to cover tobacco additives and substitutes.

Similarly the 1977 White Paper accepts the medical arguments quoting as an authoritative source the RCP third report. Most of the space is devoted to discussion of arguments concerning education, reducing the danger of smoking through substitutes, reducing noxious yields of cigarettes, advertising, sponsorship, coupons, taxation, education, health warnings, smoking in public places, smoking in health premises, sales to children, vending machines and further research.[83]

It is interesting to note that many of these arguments are based on evidence from surveys and experience in other countries where certain types of measureshave been introduced.

These arguments will be further discussed in the section on the industry's response.

8.3 Policy Development: The Medical Profession

The Royal College of Physicians, through its three reports on smoking and health, has provided the medical rationale for government action on smoking. These reports not only reviewed all the available scientific evidence on smoking but, further, made recommendations for action to prevent or reduce its ill effects. Both the evidence and recommendations for action made by the RCP have been taken seriously by the Government as shown above. The RCP have been the accepted authority on the medical aspects of smoking and health. Following the second RCP report in 1971 the Royal College set up Action on Smoking and Health (ASH) to co-ordinate the fight against smoking. The aims of ASH were to provide information on smoking, to stimulate and support research into the problems of smoking and health, to co-ordinate the activities of other bodies concerned with the problem of smoking and health, to influence public-opinion forming bodies and to act as an authoritative voice speaking for all individuals and organisations seeking to influence public and private attitudes towards smoking.[84] ASH have provided a co-ordinating function which has been noticeably absent in the official Government approach to smoking. The only formal institutional attempts at co-ordination within government have been the 1971 inter-departmental committee which disbanded after presenting its report and the existence of a Senior Medical Officer in the DHSS with the task of co-ordinating various government departments on the smoking issue. It is interesting to note, in this context, that ASH have been largely sponsored by the DHSS. Being a respected body with links to the authority of the RCP working in association with the HEC and sponsored by the DHSS, ASH have provided a focus for the anti-smoking forces. They have lobbied Members of Parliament and Ministers providing briefs for MPs in debates on smoking and health. They have promoted an all-party committee on smoking in the Commons,[85] and have participated in and organised national and international conferences on smoking. They have also been able to raise awkward questions on health at the annual meetings of tobacco companies by becoming shareholders with one share in Rothman International, Imperial Tobacco and British American Tobacco.[86] ASH have played a central role in pushing for Government action and in monitoring the voluntary agreements,[87] and generally in keeping the smoking issue alive. Friedman[88] has emphasised the role of Sir George Godber, the Chief Medical Officer at the Ministry of Health, throughout the 1960's in keeping the smoking issue alive through his constant verbal attacks on cigarette smoking both in speeches and in his annual reports.

An international dimension to the role of the medical profession in pressing for action to combat illness caused by smoking is evident in a number of respects. Reports from medical authorities in a number of other countries such as the US Surgeon General's reports and from international organisations such as the World Health Organisation (WHO) have been publicised in Britain and referred to in Parliament as authoritative statements of the case against smoking. There is an international dimension in the production of medical research on smoking, and also in research on the effects of various government policy options. Countries which have applied various measures to control cigarette and tobacco consumption have been the subject of different types of social surveys which have attempted to assess the effects of different government actions. Evidence from these studies has, in turn, been used in Britain by the medical profession to press for similar government action.

In 1967 the first of a series of World Conferences on Smoking and Health was held in New York. These have enabled medical experts and anti-smoking campaigners from various countries to get together and discuss both the new developments in scientific evidence and different approaches to policy for controlling the hazards of smoking.[89] The second World Conference was held at Imperial College in London in September 1971 and was organised by the HEC.[90]

The medical profession has played an active role in pressing for government action on smoking. Medical experts from outside government and from within the government bureauracy have used medical evidence and evidence from social surveys and opinion polls in this process. An interesting example of a medical expert and anti-smoking campaigner is Dr.Charles Fletcher. He has done original research concerning tobacco smoking and health, some of which was sponsored by the TRC.[91] He was also honorary secretary of the RCP committee which produced the 1971 report, and was on the committees which produced the other two, so he has been involved in reviewing and interpreting all the scientific evidence on smoking and in making recommendations for action. Furthermore, he has been Vice-Chairman of the HEC and Chairman of ASH, so his involvement has run to pressing for government action, and, since the HEC is the main instrument of the Government's education programme, he has been involved in carrying out government policy on smoking.

An interesting insight into the attitude of medical researchers on the subject of inferring cause from association and in the justification for taking action is provided in the following statement made by Bradford-Hill in a Presidential Address to the Section of Occupational Medicine of the Royal Society of Medicine.

On the subject of action, he states that: [92]

> "Finally, in passing from association to causation, I believe in 'real life' we shall have to consider what flows from that decision. On scientific grounds we should do no such thing. The evidence is there to be judged on its merits and the judgement (in that sense) should be utterly independent of what hangs on it. But in another and more practical sense we may surely ask what is involved in our decision."

This reveals the concern of medical experts not just with scientific evidence but also with action to prevent or reduce disease. It also reveals how this concern with action can influence apparently scientific judgements. This concern with action manifests itself in the way in which medical experts slide easily from providing conclusions on the scientific evidence to proposing appropriate courses of action, to pushing for that action through political involvement to participating in the enactment of policy.

Parallel to this action in the policy domain has been the continuous undercurrent of the technical controversy. This has remained very much at the fringes of the policy debate during this policy development period, and the medical experts have had few challenges to their scientific conclusions or authority in the public arena. What debate there has been on the scientific conclusions has been, for the most part, confined to a number of journals and books, though this has received occasional publicity. The controversy is documented in Reeve[93] and discussed briefly in Chapter 6 above.

8.4 Policy Development: The Tobacco Industry

In the period following the first report of the RCP there has been something of a change of emphasis in the tactics used by the industry in reaction to the medical arguments and in their involvement in policy. This changing emphasis follows the Government's acceptance of the medical arguments and the movement into a new phase of policy development. With increasing pressure on the Government to consider policy options other than health education, such as the control of tobacco advertising, the commercial activities of the industry have been more directly threatened. In keeping with the desire of successive Governments to avoid legislation they have entered into negotiations with the industry through its trade organisation the TAC. This has given the industry a direct role in influencing policy development. It may be argued, as Popham has done, that this formal channel for industry involvement has proved to be their 'trump card'.[94]

While the major industry tactic has been to protect their commercial interests as far as possible through negotiations with the Government, they have adopted a number of other approaches to cover various eventualities. The industry has involved itself in a programme of diversification into non-tobacco fields, and at least some of this diversification may be a reaction to the possible long term effects of the health issue on their profits.[95] Another industry hedge has been to devote a large amount of research to the development of 'safer' smoking products, and to follow this research up with modifications to existing products and the development of tobacco substitutes. It would clearly be in the industry's interest to develop a 'safe' cigarette to protect them against any successes the medical profession may have in persuading the public and the Government that tobacco is harmful and should ultimately be eliminated. Furthermore, through this research and product development the industry can claim to be showing a responsible attitude to the problem and can use this as an argument against the necessity for legislation. This argument has also been used in connection with the funds which the industry provide through its research organisation, the TRC, for basic medical research.[96]

Throughout this later period, the industry have consistently claimed not to make medical judgements, leaving this to the medical experts,[97] though this claim is incompatible with some of the statements put out by the TRC.[98] It appears that, as a working hypothesis, the industry have accepted the medical profession's interpretation of the scientific evidence and based much of their research and argument on this hypothesis, while at the same time not committing themselves to accepting the causal interpretation and, through the TRC, sponsoring research into alternatives and emphasising the role of other factors in the causation of the relevant diseases. In other words, while attempting to keep the medical question open, they have covered their bets by acting as if the causal hypothesis was proven.

The negotiations with successive Governments havebeen the industry's major channel for protecting their commercial interests. Through these negotiations they have been able to voluntarily accept some fairly limited restrictions and thereby forestall any serious legislative action which might seriously jeopardise their interests.[99] For example, the 1971 voluntary agreement allowed for a health warning to be printed on cigarette packets and to get this agreement through, the Secretary of State for Social Services requested that a Private Members Bill proposing legislation to enforce a health warning be withdrawn. Popham suggests that the Bill was killed off by the Minister and pro-tobacco backbenchers.[100] The industry got away with a voluntary agreement to print a mild warning on cigarette packets and avoided legislation to print a much stronger warning on all tobacco products. The RCP, in its third report, argue that the warning was unlikely to have any effect on smoking habits since surveys had already shown in 1964 that the majority of smokers recognised that smoking could damage their health. They also argue that, "...it is inconceivable that the manufacturers would voluntarily agree to a warning that might affect their sales."[101] Finally they argue that surveys show that it did, in fact, have little effect.

A major topic of discussion in the negotiations between the DHSS and the industry has been cigarette advertising. One of the industry's key arguments has been to defend advertising and other means of promotion on the grounds that it does not increase the cigarette market but merely persuades people to smoke a particular brand. That is, advertising is concerned with market shares not with the size of the market. An early presentation of this argument was given in some notes which the TAC sent to the RCP committee preparing their first report. These were published in the report as an appendix.[102] In these notes the TAC argued that advertising of tobacco products takes the form of advertising individual brands and provides no direct appeal to non-smokers to smoke. They also argue that the effect of brand advertising on total consumption is likely to have been small and they provide some evidence for this. The notes contain other arguments concerned with advertising in relation to consumption and smoking by young people.

These arguments have been repeated in Parliament by pro-tobacco MPs briefed by the TAC, they have also appeared elsewhere and were presumably used in the negotiations with the DHSS. In a Commons debate on smoking in 1966, in which particular attention was paid to cigarette coupons, W.A.Wilkins with a constituency interest in tobacco, and briefed by the industry, argued that coupons do not necessarily cause an increase in consumption of cigarettes and he quoted a rapid rise in coupon trading for 1961-1966 corresponding with a fall in total consumption of tobacco.[103] In the same debate, John Dunwoody, who proposed the motion, said that he did not believe that all advertising was merely brand advertising and suggested that it has a particular effect on young people who may consider starting to smoke. In 1975

Dr.David Owen (Minister of Health) stated in the House of Commons that, "There is good evidence that cigarette advertising increases cigarette smoking."[104] In the evidence given to the Social Services and Employment Sub-Committee of the Expenditure Committee in 1976, Mr.Ord Johnstone, the Chairman of the TAC, said that there was no evidence that he was aware of that cigarette coupons ever made people smoke more and he did not think that they had a great effect on sales.[105] The effect they do have, he thought, was to retain smokers to a brand. The subcommittee did not concur with this view, however, feeling that cigarette coupons must increase consumption.[106] They also found the argument that advertising is directed only to encourage 'brand-switching' unimpressive.[107] The Government White Paper following the Expenditure Committee Report suggested that, on the issue of advertising and consumption, there was little direct evidence one way or the other.[108] The third Royal College report noted a change of attitude towards this issue from the Conservative Government in 1973 to the Labour Government in 1975.[109] They referred to the statement by Dr.David Owen, mentioned above, as evidence that the Government did not now accept the industry's argument concerning sales promotion. The RCP themselves argued that brand advertising must inevitably increase the total market.[110]

In a Parliamentary debate in 1980 a past Secretary of State for Social Services, Ennals, mentioned the industry's argument that the objective of advertising was to influence smokers in their choice of brand and not to increase the number of sales and claimed that he did not believe this argument.[111]

The industry have used many other arguments relating to advertising and promotion of cigarettes, sports and arts sponsorship, smoking by young people, tobacco taxation, tar and nicotine yields and tobacco substitutes. One argument used in conjunction with the one discussed above is that brand advertising is necessary to persuade people to switch to brands with lower yields of tar and nicotine. This argument has frequently been presented and has been met with a mixed response, as with the case illustrated above. For example, the Sub-Committee argued that encouragement to addicted smokers to turn to lower-tar brands could be delivered at point of sale and any other advertising was unnecessary.[112] The Government White Paper however argued that a ban on cigarette advertising would detract from the Government's strategy of encouraging addicted smokers to smoke lower-tar brands.[113]

The progress of the Government-industry negotiations has not always been smooth[114] and has been complicated by the fact that the industry have not always presented a unified front. For example, Popham mentions a split between smaller and larger firms in the 1980 negotiations on the subject of advertising expenditure.[115] Friedman, also, argues that the unequal division of market shares in the British tobacco economy has prevented a united industry response to anti-cigarette pressures.[116] This has led to inter-industry conflict and vicious competition increasing advertising expenditures and cigarette coupon schemes.

After an agreement reached in June 1966 lapsed, when the Government and the industry could not agree on terms for its continuation, the Government then sought a voluntary agreement to eliminate coupon schemes. These had proliferated after the ban on television advertising and were boosting sales.[117] They had become a topic of much discussion in Parliament and by June 1967 coupon brands held 55% of the market.[118] Imperial Tobacco, who benefitted most from coupons, rejected the Government's formula for reducing them, but other companies favoured the voluntary agreement to avoid possible restrictive legislation and to protect their threatened market shares from a continuing coupon war. Carreras launched a scathing attack on gift coupon schemes and increased their advertising expenditure. In October 1967, after failing to get a voluntary agreement with the industry, the Labour Minister of Health, Kenneth Robinson, announced in Parliament the Government's intention to introduce legislation to ban cigarette coupon schemes and to limit other forms of cigarette advertising.[119] This action never materialised and Popham reports on the conflict between Robinson and his senior cabinet colleague, Richard Crossman, at that time.[120] When the Ministry of Health merged with that for Social Security, Crossman as the Secretary of State for Social Services, blocked Robinson's plans and he was shortly replaced as Minister of Health.[121]

Another major tactic of the tobacco industry, and an important element in the co-operation between industry and government has been the research and development put into tobacco substitutes and additives, and the modifications to existing tobacco products. The major part of the TRC's own work has been concerned with studying the constituents of tobacco smoke, and with the biological effects of these constituents on animal tissue. This work has provided the rationale for the development of tobacco substitutes and the modifications to existing products.[122] Individual companies have done their own research on methods of removing or reducing particular constituents in the tobacco smoke. From the TRC work identifying constituents of tobacco smoke which produce biological effects such as tumours when painted on the skins of mice, and the individual company's work on reducing yields of particular constituents, has flowed a series of product modifications including the change to filter cigarettes and reductions in tar and nicotine yields.

In the mid-1960's, when the manufacturers were reducing tar levels, at least one company urged the Minister of Health to produce a league table of tar yields to publicise the availability of lower-tar brands.[123] This proposal was rejected and the first government tar league tables did not appear until 1973 following the work of the standing liaison committee of industry and government experts.[124] A league table of major brands had been produced in 1971 by the Consumers Association.[125] It should be noted that the 1962 report of the RCP characterised tar yields of cigarettes as a danger and this probably had some influence on the industry's research and product modification.[126] This was certainly suggested by Sir Clifford Jarrett, the Chairman of the TRC, in his evidence to the Social Services and Employment Sub-Committee in 1976.[127] Another influence on the reduction of tar yields came from 'a small group of wise men' which included Sir Alexander Haddow and Dr.Charles Fletcher. The TRC discussed with them the evidence from the Harrogate Laboratories to see if it provided a sensible basis on which to modify cigarettes.[128]

In 1966 Imperial Tobacco, together with ICI, started work on a tobacco substitute, 'New Smoking Material'(NSM), and Courtaulds have also done work on and produced a substitute, 'Cytrel'. The industry's public attitude to the question of smoking and health has been to claim that they were not qualified to make medical judgements and would leave this to medical experts and the Government health authorities.[129] In the case of substitutes, the industry were particularly keen for the Government to make a judgement on their suitability, since the health effects could not be fully foreseen.[130] In this situation, if the companies marketed a substitute cigarette as being safer than tobacco, and it eventually turned out that it was not any safer, they may be subject to legal liability. In 1973 the Government announced the establishment of an Independent Scientific Committee on Smoking and Health (actually established in 1974), which replaced the existing Standing Liaison Committee, under the chairmanship of Professor Robert Hunter, Vice-Chancellor of Birmingham University. The committee was constituted of experts proposed by government and industry and its first priority was to produce guidelines for, and to adjudicate the testing of, tobacco substitutes and additive materials.[131] In 1975 the Hunter Committee produced its first report, <u>Tobacco Substitutes and Additives in Tobacco Products</u>, which set out a series of guidelines for the testing and marketing of tobacco substitutes and additives.[132] In 1977 it was announced that the Hunter Committee had given a limited go-ahead for the marketing of cigarettes containing the tobacco substitutes 'NSM' and 'Cytrel'.[133]

By 1977 Imperial Tobacco alone had spent about £ 20 million on NSM which it had developed jointly with ICI.[134] In that year eleven new brands of cigarette containing 24% tobacco substitute (with one exception) were marketed.[135] These were a commercial failure. The RCP view of substitutes is that, although they might lower the risks, "The only safe cigarette is one that isn't smoked",[136] and the HEC produced an advertising campaign with the copy line "Switching to a cigarette with tobacco substitute is like jumping from the 36th floor instead of the 39th." [137]

The (Labour) Government welcomed the advent of substitutes[138] but the Hunter Committee had been very cautious in their approach and concluded that, "On the basis of existing scientific knowledge and on the evidence it has seen, the product may be no more damaging to health than a similar product containing tobacco only and could prove to be less injurious". [139] In line with this cautious approach, the Government Health Department sought to ensure that the public were not misled by extravagant headlines into concluding that a safe form of cigarette smoking had arrived.[140] In 1977 the use of additives was also permitted.

The industry tactic of diversification is a difficult one to assess with respect to the smoking and health issue. It is clear that the industry have followed a programme of diversification initially into tobacco related trades and then more widely into a variety of fields including food, beer, wines and spirits, packaging and plastics, engineering pumps and retailing. The difficult question is how much of this diversification is a reaction to the health issue? Friedman has argued that at least some of it is a direct result of the health controversy,[141] and diversification has been suggested by anti-smoking forces and welcomed when it has occurred.[142] However, although the distant prospect that tobacco may be a dying trade must have had some subtle influence, there are clearly other factors determining industry diversification.

The industry has then covered its bets with a strong input into policy making through negotiations with the Government, by developing tobacco substitutes, by reducing various yields of cigarettes and by diversification. It seems that the industry have acted as if the causal theory of the relation between smoking and various diseases, notably lung cancer, chronic bronchitis and coronary heart disease, is proven. But, another tactic which they have used throughout the policy development period is to attempt to keep this scientific question open through research and through critical assessments of the medical profession's arguments. It is clear that this has not been their major tactic in this period and the reasons for this will be discussed later.

As was shown in Reeve[143], the industry's research body, the TRC, have continued in this period to emphasise a need for more research and have commissioned studies which emphasise the role of factors other than tobacco smoking in the causation of disease, such as air pollution. They have also argued that smoking has psychological and physiological benefits. However, where in the earlier period, before smoking was on the policy agenda, the TMSC/TRC and its advisers such as Fisher vigorously criticised the medical profession's interpretation of the scientific evidence, in the later policy development period this direct criticism from the industry has been less evident. There has been criticism from a number of academics but, after making critical assessment of the first RCP report and the first report of the US Surgeon General, the TRC itself has been less vocal on the causation issue.[144] The major part of the TRC's own work has been concerned with studying the chemistry and biological effects of tobacco smoke with a view to identifying and eliminating, or minimising, substances in tobacco which may be injurious to health.[145]

It is interesting to note that towards the end of 1970 a major change of direction occurred in the work of the TRC.[146] Until then the major part of the work done at their own Harrogate laboratories involved the painting of tobacco smoke condensates onto mice skins. This work aimed to refine the condensate down to such a point that one or two components were identified which contained the whole of the biological activity as far as generating tumours on the backs of mice was concerned. The ultimate aim would have been to identify these components and then remove them from cigarettes under the suspicion that they caused the biological activity in humans associated with smoking. This work did lead to the various product modifications in the 1960's and provided the rationale for the development of tobacco substitutes. By 1970, however, this research programme was questioned as refining and analysing the tar components became more difficult and as it became apparent that some of the constituents involved were the result of burning organic matter which cannot therefore be eliminated if you smoke tobacco cigarettes. This programme was then dropped and various other programmes were introduced, for example one to assess, using animals, the role of nicotine or carbon monoxide in the process ending in heart disease.

So, while the TRC have continued to emphasis the role of factors other than smoking in the causation of disease, their criticisms of the medical profession's position have become less vigorous and their own work has proceeded with the working hypothesis that cigarette smoking is a cause of lung cancer and other diseases. This does not mean that they have accepted the causal interpretation once and for all.

Even by 1976 the TRC stated that it neither admitted nor denied that the accepted statistical association represents a causal relationship.[147] Although the TMSC/TRC would probably have made the same claim in the earlier agenda period, they were far more open then in their criticism of the causal interpretation and publicised evidence which seemed to conflict with it and Fisher, a scientific consultant for the TMSC, was a very vigorous opponent of the causal hypothesis. This claim, in 1976, seems to represent something of a softening of position for the TRC. While still not admitting the validity of the causal interpretation, they were not such verbal critics of it by that time. The Council's view, as expressed by its chairman, Sir Clifford Jarrett, was that: "...it is appropriate for the health authorities, whether it is the medical profession organised, for example, in one of the Royal Colleges, or the Department of Health, to look at the evidence and do the interpretation, and it (the TRC) forms such judgements as it thinks fit." [148]

This statement contains something of an ambiguity, particularly when viewed in the light of the industry's frequent claims not to make medical judgements. The TRC seem to be saying that it is up to the health authorities to make medical judgements but the TRC can decide whether it wants to accept those judgements or not. The industry have consistently claimed that it does not make medical judgements, but the early criticisms of the conclusions of the medical profession and support for the genetic hypothesis clearly conflict with this claim. It may be, however, that in the later policy development period the industry have in fact avoided making overt medical judgements as far as possible. However, the work of the TRC must involve some medical judgements. Where Jarrett's statement accepts the authority of the 'health authorities' but reserves the right to agree or disagree with their conclusions, in the earlier period the industry would have been much more vocal in questioning the conclusions of those authorities and the evidence on which they were based.

In the later period, the TRC acknowledged that in their research they adopted the assumption, for example, that tobacco smoke causes lung cancer by direct action on the lining of the lung as a working hypothesis.[149] They emphasised, however, that this was only a working hypothesis and that they did not admit that it is the case. The chairman of the TRC suggested that the adoption of the causal interpretation as a working hypothesis was useful in that it enables the TRC to have scientific liaison with the medical profession including the RCP, the Department of Health and notably the Chief Scientific Adviser to the Hunter Committee and his staff. "All this would be much more difficult if the industry were publicly attempting to take up a position on causation, but the organisation refrains from doing that."[150] Clearly this statement contains a hidden premise in that the contact with the medical profession would only be made more difficult if the public stand which the industry took on causation was that the causal hypothesis was wrong. If the industry publicly accepted the causal hypothesis, liaison with the medical profession would surely be improved.

The chairman of the TRC also pointed out that there are dissentient opinions which do not conform to those of the medical establishment, or of the Chief Medical Officer of the Ministry of Health. He specifically mentions 'a new voice' added to this dissent, namely Professor Burch.[151] He also emphasised the psychological benefits of smoking and suggested that it would be difficult to find something which would replace the 'psychological balance' produced by smoking.

In the later period the TRC have, of course, continued to provide funds for notable supporters of the alternative genetic hypothesis, such as Eysenck, though they have also funded some prominent causal theorists as well. While the industry have not vigorously pursued the technical controversy over the interpretation of medical evidence with the same vigour in the policy development phase as they did in the agenda formation phase of the controversy, it is clear, through comparison with the corresponding controversy in the USA, that this was a possibility open to them.[152] Although in many respects the debate in the USA is very similar to that in Britain, there are a number of differences. Where the debate in the USA has been more public and more vociferous, for a variety of reasons, the publicity given to scientific controversy has been more prominent, and this controversy seems to have lasted longer in the public domain, at least until the early 1970s.[153] In fact it appears to have re-emerged in the public debate again only recently.[154] In Britain, where something of a scientific controversy still exists, as far as the policy debate is concerned this scientific controversy moved to the fringes of the debate at a relatively early stage. It appears that a consensus developed within the medical profession by the early 1960s and members of this profession have been taken as the legitimate experts on this subject. In this environment policy-makers in Britain accepted the medical experts' scientific case at an early stage.

The opposing scientific views were publicised by the industry in the early phases of the debate, in an attempt to forestall serious consideration of government action against smoking. Once the medical profession's views had been accepted by government, however, the debate concentrated more on the nature of government action, using political and economic arguments, with the scientific controversy achieving only occasional reference in policy circles.

The situation in the United States seems to have been slightly different in that the existence of scientific controversy remained a major string in the industry's political bow for a longer time. The reasons for this difference relate to the different structure and style of government policy-making in the two countries, the relative power of the tobacco lobbies and the importance of tobacco in the economies, not on the nature of the scientific issue as such. Clearly, the more public and vociferous nature of the debate in the US may have influenced the use of scientific arguments, where in Britain the development of policy through voluntary agreements and consultation with the industry in addition to the economic importance of tobacco to Governments must have influenced the predominance of economic arguments.

While the scientific debate has been characterised by experts adopting similar value judgements explicitly, but disagreeing over the interpretation of evidence, the later policy debate has been explicitly about the role of government on questions of public health and the question of individual liberty as well as the best type of action to take, while actual policy development has been mediated by the Government's conflicting concerns of public health and revenue and the political power of the tobacco lobby.

Petersen and Markle[155] have illustrated a case where a controversy which began in a scientific forum was broadened as one side found that they were not having any success in the scientific debate. In the smoking case it may well be that the industry, while initially publicising and supporting one of the scientific arguments, on finding that the medical profession and Government accepted the alternative, moved on to a stronger political tactic using the economic argument and their political muscle to influence government policy.

8.5 Summary.

In the phase of policy development or decision-making the technical debate between the members of the medical profession and the dissenters who are mostly not memebers of the medical profession, has continued. However, as the policy issue shifted from whether or not to act with respect to smoking to how to act, this technical debate has receded from the centre of the policy debate. The policy debate has concerned the issue of the best course of action to take and the issue of causal versus genetic interpretations of evidence has been treated as resolved. The policy debate has still centred on a type of technical argument since most discussion has concerned the effects of particular policy options. For example, would restriction on cigarette advertising reduce the number of people taking up smoking, or would it merely restrict tobacco companies' ability to persuade existing smokers to switch brands.

While successive governments have accepted the causal argument of the medical profession and the industry have employed it as a working hypothesis, in the policy sphere health considerations have largely been outweighed by economic considerations. The industry have not vigorously criticised the causal interpretation in the policy arena but have chosen to pursue other courses of action to protect their interests. There appear to be two reasons for this. First, with the government having accepted the causal theory, to continue to pursue an alternative would be a weak argument especially as the government began to consider specific policy options. The attention of the industry was thus naturally turned to defending itself against these specific policy proposals. Second, with a very powerful role in influencing policy through the system of negotiations and voluntary agreements and confronted with government attitudes to smoking policy which are at least ambiguous, the industry have not been seriously threatened by the policy implications of the health argument.

The industry have, of course, attempted to keep the causal versus genetic question open and continue to refuse to accept the medical profession's arguments.

This clearly illustrates the use of technical arguments as political weapons. While the tactic of pursuing the genetic interpretation and criticising the causal one was potentially useful to the industry in attempting to keep the smoking question off the agenda, this was vigorously pursued. Once this option became less effective than other tactics, it was pursued with less vigour. Also illustrated by this case is the fact that the nature of the technical issue has not influenced policy development as much as political and economic considerations have. While the nature of the technical issue meant that it was quite possible for the rival interpretations of the evidence to be supported and publicised in the policy arena, this has not happened to the extent that was possible. The development of policy has been influenced less by this technical debate than by the power of the industry and the economic interest of the government in smoking.

The role which medical experts have played in this phase has been to keep the issue on the agenda. They have been concerned, not just with providing information, but with action to press for more vigorous policy measures to combat smoking. Medics have been taken as the legitimate experts in this context and their proposals for action have at least been taken seriously by policy-makers. They have also provided support for particular options in the form of surveys of their effects. This evidence may well have influenced policy-makers within the DHSS to favour particular options, but it is likely that the influence of the industry, through its negotiations with that department,and the government's reluctance to take action which will serously affect revenue from smoking have had a far more profound effect on the development of policy with respect to smoking.

CHAPTER 9

EDUCATIONAL PSYCHOLOGY:

THE ABSENCE OF CRITICISM.

9. EDUCATIONAL PSYCHOLOGY: THE ABSENCE OF CRITICISM.

9.1 Introduction

In 1974 an American professor of psychology, Leon Kamin, published a critique of some work by Sir Cyril Burt, the eminent British educational psychologist, which suggested that Burt's work was of a very poor quality and perhaps of dubious origin.[1] Later a British journalist alleged that some of Burt's work was fraudulent.[2] Finally, when L.S.Hearnshaw published his official biography of Burt in 1979 he revealed that Burt had, in fact, been guilty of fraud.[3] This revelation was highly significant since the work in question had become classic contributions to the debate over the contributions of heredity and environment to IQ. It is also significant because the work of Burt and others from the same school of thought within educational psychology has been cited as a major influence on British education policy in the post war period.

The first of the papers which Kamin called into question was published in 1943 but most of Burt's work and the major influence of his school of thought occurred in the first half of the century. It was not until the 1940s that this school was seriously questioned. The questions which this investigation will attempt to answer are: Why was the work of Cyril Burt and the other psychometricians not seriously criticised until the 1940s, when it was always clear that it had profound implications for education policy? What influence did education policy have on this work and what influence did the work actually have on education policy?

The present chapter will assess the position of psychometrics in educational psychology and in British psychology generally in an attempt to explain the apparent lack of criticism, from within the science, of this approach and its main theoretical claims. The question of the influence of policy on the development of the science itself will also be discussed in this chapter.

The following chapter will address the question of the influence of educational psychology on the development of education policy in Britain.

9.2 The Development of British Psychology 1900 - 1940

The first half of the twentieth century saw the institutionalisation of psychology as a distinct scientific discipline and the professionalisation of educational psychology. The question of Burt's place in British psychology must be seen in this context.

The most important secondary source on the history of British psychology is L.S.Hearnshaw's 'Short History' and this will be drawn on extensively in what follows.[4]

9.2.1 The Institutional Context

Around the turn of the century the first attempts to establish psychology as an experimental science distinct from philosophy were being made. A handful of small psychology laboratories were established in various universities around that time. In England the most notable developments were at Cambridge and University

College, London. In 1897 a small experimental psychology laboratory was set up at Cambridge under W.H.R Rivers. In 1912 Rivers was succeeded by C.S.Myers whose pupils, according to Hearnshaw, "...formed the backbone of British psychology between the wars."[5] Under Myers' directorship the laboratory followed an empirical 'middle of the road' course.

In the same year that the Cambridge laboratory was established a small laboratory was started at University College, London under Sully who was then Grote Professor of Mind and Logic. While he was professor, first W.H.R. Rivers then William McDougall took charge of experimental work. In 1903 Carveth Read took over the chair from Sully and in 1907 Charles Spearman was appointed Reader in Psychology and given charge of the laboratory. In 1911 Spearman replaced Carveth Read as Grote Professor of Mind and Logic which he remained until 1931 when he was succeeded by Cyril Burt. With Spearman in the chair a characteristic 'London school' of psychology emerged.[6]

Developments in the establishment of small laboratories, chairs or lectureships took place in a number of other institutions around this time, for example at Bedford College, London; Kings College, London; Liverpool; and Reading. The first full time chair which was exclusively psychological was established at Manchester University in 1919. The first holder of the chair was T.H.Pear. In Scotland progress in the establishment of psychology in the universities was quicker and more consistent than in England. At Aberdeen and Glasgow lectureships were established in 1896 and 1907 respectively, and these later became chairs. At Edinburgh a laboratory was established in 1906 and its directorship later became a chair.

These various institutional developments in psychology occurred against a background of hostility from traditional philosophers, particularly in England. The slow development of psychology at Oxford is given as an example, by Hearnshaw, of the effects of resistance from philosophers.[7]

As well as developments in the academic study and teaching of psychology in universities around the turn of the century, there were also developments in learned societies. In 1901 the British Psychological Society (BPS) was founded with the first meeting taking place at University College, London. Up until the end of the first world war the society remained small and select, but in 1919 a major change took place. The membership rules were changed to allow a broader range of people into the society and three sections were formed devoted to medical, industrial and educational fields of psychology. This change produced a rapid increase in the society's membership.

The British Association for the Advancement of Science (BA) also provided a forum in which psychologists could present and discuss their work. In the early part of the century psychologists presented papers in the Anthropology, Physiology and Education sections of the British Association until, in 1913, a special sub-section of the Physiology section was set up for psychology and regular meetings took place. In 1921 a separate section (Section J) for psychology was established. As well as providing a forum for psychological work, the British Association initially played a considerable role in the promotion and co-ordination of research and it helped to establish psychology as a respectable science, bringing it to the notice of both the public and other scientists.

Journals also perform an important role in the establishment of a discipline and, before the first world war, psychologists were publishing in a variety of journals from education, philosophy, physiology and medicine. In 1904 the British Journal of Psychology was established by James Ward and W.H.R.Rivers at Cambridge, and in 1914 it became the official organ of the BPS. After the expansion of the BPS in 1919 it began to produce specialist journals devoted to particular branches of psychology. Thus the British Journal of Medical Psychology and the British Journal of Educational Psychology were established in 1921 and 1930 respectively. The latter was produced in co-operation with the Training College Association and replaced its organ the Forum of Education . Also in 1922 the Journal of the National Institute of Industrial Psychology was started by the Institute.

Thus, psychology began to develop an institutional framework in the early part of the century and between the wars various special branches of the discipline emerged and began to be institutionalised. But, despite these institutional beginnings for psychology, its further development in Britain was very slow, particularly when compared with developments in other countries such as the USA and Germany. This was due, Hearnshaw suggests, to the general conservatism of British universities and scepticism and hostility from philosophers.[8] As well as influencing the institutional development of academic psychology, the scepticism of philosophers also affected the areas which psychologists could study.

9.2.2 The Cognitive Dimension

Philosophers who distinguished between acts and contents of consciousness argued that either there was nothing for psychology to study, or, that it should be concerned only with acts and not with contents of consciousness. That is, psychology was to study conation not cognition. This, combined with early emphasis on conation by psychologists such as Ward and Stout, led to an emphasis in academic psychology on conation.[9] This emphasis fitted in with two strong trends of thought in British psychology between the wars; McDougall's hormic theory and psychoanalysis and its derivatives. But, Hearnshaw argues, psychology was 'saved by its applications' and it was through these that the obstructiveness of philosophical sceptics was surmounted.[10] It was also through these that the study of cognition became important.

The main theoretical trends dominating British psychology between the wars were McDougall's hormic system, Spearman's factor approach to human intellect and cognition and the psychoanalytic approach to clinical problems.

According to Hearnshaw the hormic system dealing with conative aspects of human psychology: [11]

> "Paired with a factor analytic theory of intelligence and cognition this psychology provided the framework of a comprehensive theory of personality in its dual aspects of conduct and ability, and a working basis for applications, particularly in the field of education."

The hormic psychology of McDougall was based on the notion of purposive striving as the characteristic mark of behaviour, with instincts as the sole ultimate source of hormic energy. While the psychology of conation was based on instincts, the psychology of cognition was based on the notion of innate intelligence. The London school of psychologists, under Spearman, applied statistical techniques to the results of intelligence tests to establish the 'factors' of ability, the most important of these being general intelligence which was deemed to be innate.

While psychology in other countries was going through a period of conflict between a range of 'rival' schools, this did not take place in Britain. British psychologists, according to Hearnshaw, were slow to abandon older ways of thought.[12] Behaviourism, particularly, was rejected by British psychologists, for whom 'conscious experience' was the subject matter of psychology. A comparison, which Hearnshaw makes, between psychology in Britain and that in the USA in 1940 is very instructive.[13] He points out that there was a wide divergence between British and American psychology, with little reference in either country to the work in the other. The only field with some close links was psychometrics. It is interesting to note that while in America traditional concepts were subjected to fierce criticism, in Britain psychologists remained faithful to older ways of thought. In America the topic of learning became a major area of research, while in Britain it was largely neglected, similarly with the conditioned reflex. Again in America there were attempts to introduce experimental methods into the study of personality and social psychology, while in Britain methods in these remained observational and verbal.

Social psychology was largely neglected in Britain between the wars while ideas in this field were dominated by McDougall's theory. When McDougall moved to the USA his ideas met with fierce criticism there from behaviourists and from sociologists.

9.2.3 Psychology at London and Cambridge

The two most important centres for British psychology during its period of growth and establishment were the Cambridge laboratory and University College, London (UCL). Both of these institutions developed particular styles of approach to the study of psychology and were important as teaching centres. Through the central position occupied by these institutions in British psychology their main figureheads exerted a considerable influence on its development. The most notable of these were Charles Spearman and Cyril Burt at UCL and C.S.Myers and F.C.Barlett at Cambridge.

The distinctive characteristics of psychology at UCL were both methodological and theoretical.

These were: a focus of interest on problems of human ability and personality; a reliance on statistical and psychometric rather than experimental methods; a relative indifference both to data of, and theories derived from, laboratory research.[14] These factors owed much to the influence of Charles Spearman who was Grote Professor of Mind and Logic at UCL from 1911 to 1931, and they provided the basis for a distinctive London school of psychology. The inspiration for this type of work had come from Francis Galton.

The work of Spearman focussed on intelligence and its measurement and he first introduced and continued to develop the idea of factor analysis. This, Hearnshaw describes as a 'momentus development in psychology.'[15] The technique of factor analysis involved the application of correlation methods to data from mental tests to assess the relations between abilities. Spearman argued that each psychological capacity contained a general factor 'g' and a distinctive special factor 's'. The general factor was usually identified as equivalent to the popular notion of general intelligence. Factor analysis became a thriving research field, particularly at UCL, though its development was not uncontroversial.

Much of the controversy surrounding factor analysis was of an 'internal' nature. That is, it represented differences of opinion amongst factor analysts themselves. For example, whereas Spearman proposed the existence of the general factor and the specific factors, Burt proposed the existence of 'group' factors common to a number of different abilities. Again, while some investigations found hierarchies of factors in their correlations, others did not. Some criticism was more fundamental. For example, Godfrey Thomson, a key figure in the mental testing movement, denied the necessity for a two factor theory. He argued that it is not in virtue of any psychological laws, but simply as a result of the laws of chance that it is possible to analyse ability into a few common factors.[16] According to him the mind works as if it were so composed and the postulation of factors is purely speculative and hypothetical. Thomson was, himself, a factor analyst though. Similarly, Burt came to argue that factors need not exist in the mind, but were useful guides like the lines of longitude and latitude on a map.[17]

When Cyril Burt took over the chair at UCL from Spearman he carried on the same broad traditions of interest there. According to Hearnshaw, Burt's aim was to make London the focus for a brand of psychology founded and developed there by Galton, that is, 'individual' or 'differential' psychology.[18] Throughout his professional life general intelligence was Burt's main concern. One of his main activities was in the development of techniques of intelligence testing and the group test of intelligence was primarily due to Burt, according to Hearnshaw. Some of Burt's major publications illustrate this concern with testing and general intelligence as well as with factor analysis. For example, his first report to the London County Council in 1917 <u>The Distribution and Relation of Educational Abilities</u>[19] was concerned with the results of testing with respect to demarcating between ordinary and defective children and verifying the hypothesis of a general educational ability underlying all school work. His well known Mental and Scholastic Tests (1921)[20] was concerned with Burt's 'London' revision of the Binet-Simon intelligence test and with tests of educational attainment. This became a handbook for users of mental tests. Work concerned with the use of tests in demarcating between normal and defective children was presented in The Backward Child (1937),[21] and The Factors of the Mind (1940)[22] was concerned with general intelligence and factor analysis. So Spearman's interests in factor analysis and intelligence were carried on by Burt at UCL, where he developed a theory of group factors. Intelligence was defined by Burt as, 'Innate, general cognitive efficiency'.[23] Towards the end of the inter-war period research at UCL was being done in applying psychological tests to the orectic field by developing tests of personality. This work was more controversial than intelligence testing.

It is argued, by Hearnshaw, that Spearman established the first important school of research in Britain with a co-ordinated scheme of research. He points out that seven of the first ten, and twelve of the first twenty of the Monograph Supplement of the British Journal of Psychology were written by those who had worked with Spearman. This school was continued by Burt with statistical and factorial methods providing the backbone of the school's methodology.

The main characteristic of the Cambridge 'school' of psychology was a commitment to experimental research. It was a common methodological approach that defined this school, rather than a commitment to any particular set of theories. Cambridge psychology was pragmatic, eclectic and, to some extent, atheoretical but committed to experiments. A key secondary source on the history of psychology at Cambridge is a thesis on this subject by Colin Crompton and the following discussion draws heavily from this as well as from Hearnshaw.[25] It is argued by Crompton that Cambridge represented a school in that there was a common intellectual thread running through the work of its central members and that it became an important training school for eminent psychologists.

The Cambridge laboratory came under the influence of a series of eminent psychologists. In 1877 James Ward had attempted to set up a laboratory at Cambridge but it was in 1897 that this was achieved with W.H.R.Rivers as Director of the laboratory. In 1912 C.S.Myers took over the Directorship and it was under Rivers and Myers that the tradition for eclectic, broadly based experimental work developed at Cambridge. Under Rivers and Myers Cambridge psychology embraced a whole range of fields and theoretical ideas, but it did not adhere firmly to any one of them. Crompton argues that the two main reasons for this were the influence of the philosophy and psychology of James Ward, and the personalities of Rivers and Myers.[26] During this period the laboratory had been building up a reputation, particularly for applied research and this helped the growth of the laboratory.

In 1922 F.C.Bartlett took over from Myers as director when the latter left to continue his work at the National Institute for Industrial Psychology (NIIP) which he had founded in 1921. In 1931 Bartlett's directorship of the Cambridge laboratory was finally made into a professorship. Bartlett's main work was on memory and on anthropology and cross cultural psychology, as well as applied psychology work in the industrial and military spheres done particularly during the wars. He continued the experimental tradition at Cambridge and was influential both as a teacher and while serving on several government and NIIP committees. According to Crompton, Bartlett disliked psychological tests and statistics, and it was partly because of this that Cambridge psychology contained very little mathematics.[27] He never encouraged testing work at Cambridge except in the context of wartime selection purposes. This is an important point to note. Crompton summarises the research work done at Cambridge before the second world war and under Bartlett's leadership as consisting of applied studies, perceptual and cognitive studies and animal psychology.[28] Although these were the main themes, Crompton describes as a breakaway group P.E.Vernon, and O.A.Oeser who were working on personality tests. He argues that these, along with R.H.Thouless who lectured in educational psychology, were never really accepted by Bartlett.[29]

Finally Crompton emphasises the influential role which the Cambridge school, and particularly Bartlett, had as a teaching centre for psychologists. He also provides some useful figures on the role of Cambridge and UCL as training centres for psychologists. Crompton concludes that Myers, Bartlett, Spearman and Burt provided, through their teaching institutions, a flock of pupils who, until the late 1960s, monopolised British professorships.[30] In the 1930s Cambridge and UCL produced the majority of young psychologists and attracted post graduates from elsewhere. This had the effect, Crompton argues, of producing a 'Bartlett cluster' and a 'Burt cluster' of pupils.[31] When in the 1940s and 1950s psychology expanded and new chairs were created, these pupils were the best equipped to fill them. Furthermore, through a secondary level effect of future professors being taught by pupils of Bartlett and Burt, even today two thirds of British psychology professors have received training either directly from Bartlett or Burt or from one of their pupils.

Crompton presents the following figures:[32]

British chairs in psychology		Post Graduate Training of the 16 professors in 1957	
1937	5 chairs	10	Trained by Bartlett & Myers
1947	10 chairs	3	" " Burt & Spearman
1957	16 chairs	1	From America
		1	From Australia to Birkbeck.
		1	Trained at Kings College, London by Aveling.

Crompton attributes the discrepancy between the number of professors trained at Cambridge and those trained at UCL to characteristics of Bartlett.[37] He was concerned to place his students in influential positions and personal attributes made him a considerable success as a teacher. Crompton adds the caveat that Burt's pupils went into more diverse subjects than Bartlett's and many of them went abroad. Members of these two schools also dominated the positions of responsibility in societies, committees and on journals within psychology during its formative years.

9.2.4 Applied Psychology

It is argued by Hearnshaw that, in the face of hostility from philosophers, psychology was 'saved by its applications'.[34] As psychology was becoming quite self-consciously more scientific, breaking away from its philosophical background, applications in education, medicine and industry were explored. During the first world war psychology was extensively applied to military and clinical problems and this added to the respectability of applied psychology. Following the war the applied fields were crucially important for psychology both as important cognitive fields for exploration and as resources in the establishment of psychology as a distinct scientific discipline. The whole process by which psychology was applied to these fields and its relation to the establishment of psychology and the institutionalisation and professionalisation of educationists, personnel officers in industry and medical doctors working in asylums has been studied by D.C.Doyle.[35] Here it is argued that for the 'clients' in education, industry and medicine psychology represented a solution to the problem of professionalisation. As educationists, doctors in mental asylums and managers in firms' personnel departments sought to increase their professional status and fend off lay control a scientific psychology provided them with the basis for achieving this. This, in turn, brought benefits to psychology as: 'clients' produced psychological research for themselves; psychological work could be published in 'client's ' journals and at their meetings; 'client's ' demand for training courses led to the setting up of some new psychology departments and a career structure opened up for psychologists in 'client' professions presumably making the discipline more attractive to students. It is further argued by Doyle that psychology spread into these areas through 'colonisation' and that the reorganisation of the BPS, the foundation of the three new sections, and the takeover of the relevant journals illustrate this process.[36] These arguments will be discussed in more detail with respect specifically to educational psychology in the next section.

9.3 Educational Psychology 1900-1940

9.3.1 Education and Psychology

The relationship between educational research and psychology began to develop in the early part of this century. By the turn of the century educationists were already doing their own research and had even used mental tests in schools. This research was at the time referred to as 'Experimental Pedagogy' and some institutional developments occurred in this field during the early years of the century. Departments of Education were established in most of the provincial universities and in 1911 The

Journal of Experimental Pedagogy first appeared. Even at this stage, there was an intimate relationship between educational research and psychology. Some educationists had been trained in psychology and the techniques of educational research were the same as some of those used in psychology, notably the application of statistical methods and the use of mental tests. This relationship developed and became even more intimate.

It has been shown, by Doyle,[37] how this developing relationship can be seen in terms of a client/practitioner model with educationists, the clients for psychological knowledge, using psychology as a resource for professionalisation which, in turn, influenced the institutionalisation of psychology. Psychologists published articles in the *Journal of Experimental Pedagogy* and in other education journals and educationists themselves were contributing to psychological research. Educationists were pressing for teachers to be university trained and this provided an impetus to psychology as this formed a large component in education courses. Scottish universities were particularly advanced in initiating training for teachers with courses as early as 1905 and with the establishment of Bachelor of Education degrees at Edinburgh and Glasgow in 1917 and at Aberdeen in 1918. These courses had a very strong psychology component and Doyle argues that these developments were a major force in the establishment of psychology departments at Edinburgh and Glasgow in 1906 and 1907.[38] Psychology was also taught at some teacher training colleges.

This relationship between psychology and education further provided a career structure for psychologists in teacher training colleges or university education departments. Doyle interprets the re-organisation of the BPS in 1919 as a cuccessful attempt by psychologists to capture the 'markets' of education, medicine and industry.[39] At this time membership rules were relaxed to allow those <u>interested</u> in psychology into the BPS as well as those <u>practicing</u> psychology. Also, three new sections were established, medical, educational and industrial. Following this re-organisation the membership of the BPS increased dramatically. Psychology was consciously becoming less philosophical and more scientific, and this made it of more use to clients in education and the other applied fields. Doyle describes this process as one of colonisation.[40] Psychologists were persuading educationists to become applied psychologists and the re-organisation of the BPS was one part of this. Other aspects included the migration of pupil-teachers into psychology and their transformation into academic psychologists and the considerable contribution made to psychology by educationists such as P.B.Ballard, Godfrey Thomson, C.W. Valentine and W.H.Winch. (Valentine was trained in psychology but was employed as an educationist). Finally, the relationship was sealed when the journal <u>The Forum of Education</u> which had replaced <u>The Journal of Experimental Pedagogy</u> became in 1931 <u>The British Journal of Educational Psychology</u> by agreement between the education section of the BPS and the Training College Association.

For educationists psychology represented a tool in the transformation of their occupation into a profession, according to Doyle.[41] It promised the objectivity of science with which to fend off control by laymen, it promised to help educationists to establish peer consensus and professional solidarity and it provided a basis for academic training which was a prerequisite for improved professional status.[42] In a similar vein, Sutherland and Sharp have argued that it was the image of the academic that appealed to educationists which goes some way to explaining the attraction of psychology and the influence of people like Burt.[43]

To illustrate the links between psychology and education Hearnshaw provides some examples of educationists at various centres closely involved with psychology.[44] For example, inspectors attached to the London County Council (LCC) such as C.W.Kimmins, P.B.Ballard, W.H.Winch and A.C.Hughes made important contributions to psychology and were instrumental in establishing the first professional post for a psychologist, which Burt filled. The staff at the London Day Training College contributed to psychology and passed on an interest in the subject to their pupil-teachers. Among these staff were Sir John Adams, Sir Percy Nunn, Cyril Burt, H.R.Hanley and Susan Isaacs. In Scotland strong centres of educational psychology developed at Edinburgh and Glasgow and some departments of education in English provincial universities showed a strong psychological bias, particularly Birmingham where C.W.Valentine a Cambridge graduate in psychology was Professor of

Education from 1919 to 1944. He was also editor of The Forum of Education and subsequently of The British Journal of Educational Psychology.

A major problem facing educationists in the early part of the century was the allocation of defective children to special schools or classes. According to Sutherland and Sharp, interest in this problem ante-dated the development of sophisticated diagnostic tools to deal with it.[45] This was one of the main areas in which psychology had something practical to offer education. Interest in this problem became intensified as legislation allowed local education authorities to create special schools and classes in 1899. The techniques of mental testing provided a means for selecting children for special schools. Interest was shown in mental testing by the LCC and by Dr.George Newman, Chief Medical Officer of Health to the Board of Education.[46] In 1913 Burt was appointed to a half-time post as psychologist to the LCC for the purpose of applying tests to children nominated for admission to special schools for the mentally defective, though Burt enlarged his role somewhat.

Developments in mental testing and scope for its application to the problem of identifying mental deficiency expanded as legislation was passed in 1913 and 1914 concerning the identification of adult mental deficiency and making it the duty of local education authorities to ascertain the mental competence of all children between the ages of seven and sixteen. Local authorities were then required to provide appropriate training for educable defective children. But the first world war prevented such action, and with the financial problems caused by the depression after the war there was little expansion in the identification and separate education of mentally defective children.[47] This was despite calls for action by George Newman and by a specialist committee set up by him to investigate this problem. Sutherland concludes that mental testing reinforced and extended an already developing policy for the identification and separate treatment of defectives. But, although it had widespread acceptance and increasingly elaborate and sophisticated use, financial restrictions brought about stagnation in its application.[48]

Another problem in education for which mental testing provided an answer was that of classification of normal children. Sutherland and Sharp argue that, as differentiation between normal and sub-normal children became a stagnant area for the application of mental testing, so a growth area appeared in the form of differentiation between the normal through selection for secondary school education.[49] Although Sutherland [50] has argued that these two problems are quite distinct elsewhere it has been argued that they were always intimately related.[51] Again this is a case where the problem existed before the techniques such that mental testing reinforced and extended a trend towards the classification which was already apparent.[52] In 1820 legislation created scholarships for pupils to go on to post-primary education. Other legislation provided an impetus to the selection problem and, in 1907, the Free

Place Regulations increased the proportion of secondary school places which were free and open to children from public elementary schools. Between 1912 and 1920 the absolute numbers of free-places nearly doubled. Also in 1907 the Board of Education introduced the idea of free-place examinations. As demand for the free places expanded rapidly, the free place examinations became intensely competitive.[53] Initially individual intelligence tests were used alongside normal selection examinations and they were mostly used for borderline cases. But, later on, a new development in mental testing appeared, the group test.

Group tests were first used, in addition to the usual scholastic tests in free place examinations, by Bradford County Borough Education Committee in 1919. These tests had been devised by Cyril Burt for research purposes. It is worth pointing out here that Sutherland and Sharp reminded us that this piece of information was supplied by Burt himself[54] and they could find no reference to this in the Bradford County Borough archives.[55] In 1920 Northumberland Education Committee were advised by Godfrey Thomson, then Professor of Education at Newcastle, to use group tests to single out a small number of high scorers and then to apply individual tests to these to select primary schoolchildren for free secondary school education.[56] During the period 1917-1931 the LCC were also experimenting with group tests. Following the experiments with the use of group tests at Bradford and Northumberland, two publications appeared in 1921 which Sutherland argues,

"...inaugurated group testing as an industry",[57] these were Thomson's account of the Northumberland experiment in the British Journal of Psychology and Burt's handbook Mental and Scholastic Tests.[58] Sutherland documents the 'rapid and sympathetic' response of the Board of Education to group tests and, in contrast, the slow adoption of these tests by local authorities because of a range of factors, including the effects of the depression, lack of power on behalf of the Board of Education, reactionary authorities, uninterested constituencies, the vested interests of teachers, misunderstanding of the experts and technical problems with the tests.[59] When a sub-committee of the Board of Education Consultative Committee considered group tests over the period 1920 - 1924, Godfrey Thomson was one of the witnesses and advocated their use in selecting children for different forms of education and Burt, a co-opted member of the sub-committee, wrote a large part of the report.[60] The report recommended the use of tests in selection exams on an experimental basis, and the correlation of their results, with those of attainment tests.

Psychology thus provided a solution to two different types of problem facing educationists, one social/institutional pertaining to the occupation of education, the other practical pertaining to the methods of education. It represented a tool in the professionalisation of education and it provided a technique to solve some major problems of selection.

9.3.2 Mental Testing and Psychometricians

Out of the relationship between psychology and education grew educational psychology. In the previous section we saw that mental testing was extremely important for the development of educational psychology. In fact the first professional educational psychologist was employed specifically to apply mental tests to school-children. But mental testing was not the only area of study within educational psychology. To further assess the importance of mental testing it will be useful to survey the fields of study with which educational psychology was concerned up until the early 1940s. Two useful sources on this are Schonell's review of the development and state of the art of educational research in 1947[61], and Blackwell's list of researches in educational psychology.[62]

In surveying the early history of educational research Schonell confirms that the psychological work which was initially most influential concerned testing and the intellectual powers of children and adults. The pioneer work in psychological research and its application to education was done in the second decade of the century, according to Schonell, by P.B.Ballard, Cyril Burt, Godfrey Thomson and W.H.Winch. This work was all related to the development of tests, their standardisation and application in education and research. In addition to this pioneer work Schonell cites three other sources of influence on the development of research in education. The first is the 1924 Board of Education report on Psychological Tests and their use in education, referred to above,[63] and prepared largely by Burt. Secondly, some influential institutions are mentioned: the establishment of the NIIP in 1920 and its work on vocational guidance; the foundation of the education section of the BPS in 1917; the foundation of the Committee for Research in Education in 1923 and the Scottish Council for Research in Education in 1928. Finally he mentions some key individuals and publications which have influenced the development of educational research, including Spearman and Ballard, and the journals The Paidologist (1899) which became Child Study (1908), and particularly the Journal of Experimental Pedagogy (1911) which became the Forum of Education (1923) which then became the British Journal of Educational Psychology (1931).

A look at the headings which Schonell gives to the fields of study of educational research is instructive in assessing the place of mental testing in educational psychology up until 1947. His first heading is 'Individual differences in intelligence and scholastic achievement'. Here intelligence and attainment tests have provided the main tool in research, with Burt providing the pioneer work and Thomson and others extending this work. It has far reaching implications for education and has helped to change teachers' practice, according to Schonell. The second heading 'Mental Development in Childhood and Adolescence' represents the area of child study and although at first glance it seems to be an area which potentially could be antagonistic to the idea of fixed innate intelligence, Burt has made contributions to it and the main contributors in Britain were Susan Isaacs and

C.W.Valentine. (This area will be discussed more fully in a later section dealing with potential sources of criticism for the psychometrists). 'The Measurement of Intelligence' field is dominated by intelligence testing and, again, the names of Burt and Thomson are most frequently cited. Schonell points out that by 1947 this field was enormous. Various problems with testing had opened up whole new areas of research, though the value of testing was not questioned. 'Attitudes and Interests' - the measurement of these was a newly developing field in the 1940s with important implications for education. It had been realised that intelligence was not the only important aspect of an indiviual's psychology, but that emotional life was also important, particularly regarding motivation. 'Curriculum Content and Teaching Method' - this area received a lot of attention from researchers but little of this had influenced actual practice by teachers, according to Schonell. The main finding of the research was the importance of emotional conditions and the pupil's background and experience in the learning situation. A major method for the 'Assessment of Scholastic Progress and Practice' was through scholastic tests and, again, the pioneer work here was done by Burt and others such as Ballard. These tests had been extensively applied in clinics and schools for a variety of purposes. Also in this area there had been discussion of the use of School Record Cards and the role of psychological tests with respect to these and in selection for secondary education. Schonell claims that advances in the allocation of pupils for secondary education were due directly, or indirectly, to educational research. The final categories which the review deals with concern handicapped children.

It is clear, from looking at these headings, that mental testing was the main component of education psychology at this time and that the work of Burt, Thomson and other psychometricians were the major contributors to the field.

Another illustration of the central place which mental testing played in educational psychology is provided in Blackwell's list of research in this subject.[64] This list also provides some indication of the influence of the London school within educational psychology. The list deals with research in both educational psychology and teaching method, and the data concerns research theses presented for higher degrees in British universities during the period 1918-1943. See Table 1.[65]

Out of a total of 526 theses in educational psychology, 353 are concerned with 'Mental Development and Capacity' and 260 of these concern 'Intelligence and Mental Tests'. So nearly one half of all theses in educational psychology between 1918 and 1943 were concerned with intelligence and mental tests. It is interesting to note that out of the total 526 theses in educational psychology only 23 were concerned with social psychology. It is clear from the figures that, as far as University research was concerned, intelligence and mental testing was the most important aspect of educational psychology. This confirms the views given in a review of the progress

University	Mental Development + Capacity - Comparative Psychology.	Sense+ Sense Perception.	Executive Functions Emotion Sensibility Feelings etc.	Higher mental Processes.	Special Mental Conditions.	Abnormal Psychology.	Relations to other subjects Sociology - Social Psychology.	Teaching Method	Total Educational Psychology.	Total Educational Psychology + Teaching Method.
Wales	10	1	2	2	1			7	16	23
Leeds	22	2	4	8			1	28	37	65
Glasgow	54	4	6	7		1	3	18	75	93
London	106	11	24	33	1		11	38	186	224
Durham	3			2			1	10	6	16
Edinburgh	100	4	5	5	1	1	2	7	118	125
Birmingham	26		2	4			2	4	34	38
Sheffield	2		1	1			1	3	5	8
Manchester	11		2				1	28	14	42
Liverpool	6		1			1	1	24	9	33
Bristol	5		2					6	7	13
Dublin	1							13	1	14
Oxon	5	2		4					11	11
Reading	2		2	2				3	6	9
Aberdeen			1						1	1
Belfast								2		2
TOTAL	353	24	52	68	3	3	23	191	526	717

<u>TABLE 1.</u> <u>Research Theses in Educational Psychology and Teaching Method Presented to British Universities, 1918 - 1943</u>. (Derived from Blackwell's 'List'). 65

of research in education by the BPS Committee for Research in Education in 1925 in which it was claimed that "...much attention is being given to the problem of mental testing".[66] The report clearly saw this as the main field of research.

It is also possible, using Blackwell's list, to make some assessment of the relative contributions of different centres of research. Out of the total of 526 theses on educational psychology 186 were submitted to London university, 118 to Edinburgh, 75 to Glasgow, 37 to Leeds and 34 to Birmingham. At London University there were a number of colleges with psychology or education departments, for example, Kings College and Birbeck College. However, the main centres for educational psychology were University College, where first Spearman and then Burt held the chair in psychology, and the London Day Training College (later the University of London Institute of Education) where Sir Percy Nunn was director and Burt was professor of educational psychology from 1924 to 1932. Other major centres for educational psychology research were also dominated by psychometricians, for example Edinburgh where Godfrey Thomson worked at the Moray House Training College, and Birmingham where C.W.Valentine was Professor of Education.

Another measure of the role of psychology and particularly of mental testing in educational research is the character and membership of bodies concerned with co-ordinating research in education.. Until the National Foundation for Educational Research in England and Wales, of which Burt was Vice-President, was formed in 1945, educational research in England and Wales had not had a central co-ordinating body, but this function had been fulfilled to some extent by various committees.

In 1908 the Teachers Guild of Great Britain and Ireland created a sub-committee on Psychological Research in Schools; this was at the suggestion of W.H.Winch who became the sub-committee's chairman. According to Winch this committee was really concerned with experimental pedagogy. The committee acted as a co-ordinating and directing body for research and it helped in influencing the attitudes of educators on such matters as memory work, mental fatigue, mental tests and the teaching of reading and other skills.[67] Winch was an LCC inspector who made some pioneer contributions to educational psychology and particularly mental testing. He was also the original candidate for the LCC post which Burt filled. At this time, Winch was on unpaid leave from the LCC to study the practical applications of contemporary psychology to the school problems of the day.[68]

In 1909 the BA set up a committee on 'Mental and Physical Factors Involved in Education', which reported on matters such as the testing of mental deficiency and psychological factors in reading, writing and spelling. In 1916 Burt was a member of this committee. He was also a a member of the Research Committee of the Child Study Association founded in 1915.

In 1921 and 1922 a research committee of the London Head Teachers Association published reports in <u>Educational</u>

Research, a supplement to the Head Teachers Review, which served to stimulate research amongst teachers and spread some of the results of scientific research among members of the association. In 1923 the education section of the BPS set up a standing committee to co-ordinate research. The Committee for Research in Education served as a bureau of inquiry for educationists and every year published a list of psychological research work then in progress on educational problems. The committee had been set up by Susan Isaacs who was also its secretary, and Burt was also on the committee. This committee was discontinued in 1935. In 1934 another standing committee was set up by the BPS - the Committee on Human Mental Measurements. According to Edgell this committee was instituted to study individual differences of a measurable kind, and it worked in close cooperation with the International Federation of Eugenic Organisations. It issued bulletins from time to time giving full information on the various studies in progress in British Psychological laboratories.

In 1928 the Scottish Council for Educational Research was established and this represented a landmark in the co-ordination of educational research. It was the first permanent co-ordinating body for this work. Hearnshaw points out that, under Rusk's direction, it published thirty reports in the space of twenty years.

Psychometrics was then an important component of educational psychology and its leading exponents were highly influential psychologists and educationists. The spread of this influence is further illustrated by the place which psychometrics and its practitioners had in the training of teachers. There is a large secondary literature on the history of teacher education in Britain[69], but a useful source for our purposes is Frisby, who reviews the introduction and development of educational psychology in teacher training colleges.[70]

As the relationship between psychology and education developed, so psychology began to be taught in university education departments and teacher training colleges. Four phases in this process are identified by Frisby.[71] In the period 1890 - 1910 psychology was first being introduced as a subject in training colleges, with some relevant questions appearing in syllabuses and examinations and some lecturers with psychology qualifications being appointed. According to Frisby the inspectorate were important in helping to get psychology introduced and established in training colleges. The university education departments were in the forefront of the adoption of psychology and tended to be more in touch with the most recent psychological work, and it was educationists in these departments who argued for a scientific educational psychology. As we have seen, Doyle interprets this as part of a strategy for professionalisation of education.[72] At this stage the time allotted to psychology in the training college course was small. During this period psychology was undergoing the change from the old associationist ideas to a purposive biological view of human development with emphasis on the individual. This view was becoming prominent in psychology generally and was propagated in textbooks for teachers. It was at this time that McDougall published his Introduction to Social Psychology which we have already seen dominated British psychology for nearly thirty years. According to Frisby this also became the most widely studied textbook in English training colleges for a period of a quarter of a century.[73] The new psychology was also propogated in teacher training courses by leading educationists such as Sir John Adams and J.J.Findley, and these developments were further encouraged by the Child-Study movement.

In the period 1910-1925 the psychology taught in training colleges was becoming more scientific and educational psychology was becoming established and accepted as an important factor in teacher training. In fact, by the end of the 1920s, it was regarded as indispensable and is the real basis of the education course by teachers and students in the colleges. Staff in the colleges were becoming better qualified to teach psychology and the 'hormic' psychology of McDougall, with its emphasis on instinct and the individual, became firmly established. It is argued by Frisby that the publication of Sir Percy Nunn's Education: Its Data and First Principles in 1920[74] saw the culmination of this trend towards the hormic and individual centred view in education and psychology[75]. This book was an important influence on the training of teachers and was influential in a broader context as well, including the field of education policy. As well as the establishment of hormic psychology in training college courses, mental testing also began to be taught in this period. Textbooks began to give full coverage to mental testing and questions concerning it appeared on Diploma examinations. The main emphasis of examinations was placed on the development and individual capacities of children. In 1924 Burt himself became professor of educational psychology at the London Day Training College where all students took a course in general psychology and a course in individual psychology, including mental testing. There was also an educational psychology option with emphasis on experimental work and testing.

In the period 1925-1945 mental testing became more prominent in training college courses, with questions on this topic appearing regularly on examination papers. It also received more coverage in textbooks. Instinct theory was still the dominant perspective in educational psychology but, towards the end of the 1940s, it began to receive criticism and was beginning to be replaced by the concept of 'tendencies'. For most of the period, however, the main psychology taught was instinct theory and mental testing, with play and child development also covered. Psychoanalytic theory was important in British psychology at this time but its inclusion in training college courses was treated with some caution. Educational psychology courses concentrated on the intellectual development of the individual child and included the study of intellectual and emotional backwardness. Towards the end of the period, with the advent of war, some attention was beginning to be paid to the important part which society had to play in the mental and emotional development of children.[76]

After the second world war, mental testing became even more important in educational psychology taught in training colleges, because of the implications of the 1944 Education Act, and the enormous amount of research concerning it. A large number of questions on examination papers were devoted to it. Instinct theory was, by then, disregarded by educationists. Furthermore, the isolation of British psychology and educational psychology from foreign developmentsbegan to break down. English textbooks which had been dominated by McDougall's ideas now tended to cover four main topics: the nature of the learning process; the psychology of individual differences; personal development; the relation of the individual to society.[77] In the 1950s American textbooks began to be used in training colleges. About a third of the time spent on educational psychology in the training college courses was devoted to the study of attainments, ability and psychometrics.[78] More emphasis was now being given to the social development of the individual. By 1962 62% of training college staff were qualified in psychology.[79]

Thus the type of psychology taught in teacher training colleges was the biological, genetic conation of McDougall's hormic theory and the study of individual differences and mental testing. Thus educational psychology was propagated in teacher education by leading educationists in university departments of education.

The educationists were influential through their own departments, through teaching at local training colleges, through the Day Training Colleges affiliated with universities and through textbooks. Furthermore, it was at these departments that many future training college lecturers received their own training. Thus it was that prominent psychologists and educationists, such as Sir Percy Nunn, Cyril Burt and H.R.Hanley of the London Day Training College, C.W.Valentine at Birmingham University, Sir Godfrey Thomson at the University of Edinburgh and Moray House, spread the psychology of instincts, individual differences and mental testing to the world of teacher training.

9.4 The Rise of Criticism

9.4.1 Possible sources of criticism

In Britain Sociology and Social Psychology were very slow to develop. While the first part of the century saw some major developments in sociology in Europe and America in Britain there was only one chair in the subject, founded in 1907 at the London School of Economics (LSE). By 1962 there were twentythree chairs. It has been pointed out that British psychologists were generally ignorant of the sociological work being done elsewhere.[80] A symposium on the Study of Society was held at Cambridge in 1939 organised by F.C.Bartlett and M.Ginsberg, Professor of Sociology at the LSE, but, as Hearnshaw points out, this merely revealed the ignorance of psychologists to sociological work.[81]

Social psychology was also slow to develop. In Britain social psychology had been dominated by McDougall's work, with the notion of action being controlled primarily by instincts. This notion was fiercely criticised abroad, particularly in the USA where a whole series of social psychologists attacked McDougall's ideas, as did the behaviourists who, of course, emphasised the role of learning. But, in Britain, behaviourism was scorned and social psychology was slow to develop. The social psychology section of the BPS was not established until 1940, when Professor M.Ginsberg was elected chairman.

Anthropology was more developed in Britain and at Cambridge the links between anthropology and psychology were very strong. For example, nearly half of Bartlett's publications concerned anthropology and cross-cultural psychology.[82]

In the 1920s and 1930s, at the same time as McDougall's notion of instincts was coming under attack in the USA, there was a major Nature/Nurture controversy in that country.[83] But this had little impact in Britain, though scattered references to it can be found. During this period and as part of this controversy numerous twin studies were published and most of these were taken to support the view that the influence of nature over intelligence was greater than that of nurture. The first studies of monozygotic twins reared apart were published in the late 1930s and early 1940s and these showed a significant effect of environment on intelligence.[84] Studies were also being done in America at this time of children in foster homes. By the 1940s this work was beginning to attract more attention amongst British psychologists and geneticists.[85]

An area which had developed under the umbrella of educational psychology was that of child study, or child development, now called developmental psychology. In Britain this area was already established at the beginning of the century, but it developed close links with psychology along with other educational research and ultimately became a part of educational psychology. Two major contributors to this field in Britain between the wars were Susan Isaacs and C.W.Valentine; Burt also made numerous contributions. Being concerned with the mental and intellectual development of children, this area would seem to hold the potential of conflict with the individual differences, psychometrics and factor analysis schools, yet between the wars the two fields coexisted in harmony with the same personnel contributing to both. The solution to this apparent paradox is that, while the psychometricians' viewpoint was dominant, child study was concerned with the way in which the hereditarily determined intelligence of the child was applied and how this application developed with experience and education. Furthermore, it has been argued that before about 1950 the main developments in child study were in terms of techniques for study of children and it was not until after 1950 that attention was turned more to theory.[86]

9.4.2 Criticism

The first major criticism of the psychometrics school in Britain came from within psychology and started in the early 1940s. The main critics, initially, were Cambridge psychologists. In 1943 E.G.Chambers argued that, "Too frequently, it seems, mathematical psychologists build elegant and dizzy numerical edifices, forgetting in their architectural zeal the flimsy foundations upon which their fabrics stand."[87] He went on to question the extent to which the concept of quantity can be applied to psychological data and how far it was amenable to treatment by arithmetic methods. He is particularly critical of intelligence testing and factor analysis, arguing that we don't know what tests measure, that the concept of quantity of intelligence seems to have no meaning and that it is quality of intelligence, not quantity, that differs from person to person. Chambers views intelligence as a complex of qualities which is living, so that it may change and which does not function in isolation. This view is rather like that of the Gestalt psychologists and is in keeping with Bartlett's views and those of other Cambridge psychologists. Chambers warns of the grave dangers of intelligence testing, seeing a tendency to regard psychological qualities in isolation as a reversion to the days of the old 'faculty psychology'.[88] He espouses the Gestalt notion of the whole being more than the sum of the parts. He argues that, since there are no direct measures of psychological qualities, test scores are subjected to mathematical treatment without knowledge of their real meaning.

Factorial analysis, for Chambers, rests on a very uncertain basis.[89] He emphasises the need for experimental research and argues that statistics are only of use when applied to data obtained from well designed experiments. Mental tests and factorial analysis add nothing to theoretical knowledge of human abilities, according to Chambers.[90] As well as being critical of the foundation on which both are based, he also has serious doubts about their applications and he suggests that the whole approach to vocational selection may be unsound. He advocates selection by elimination of the incapable rather than the differentiation of everyone, with emphasis placed on training.

It is also interesting to note, in the light of later revelations about the quality of Burt's research, that Chambers argues that: "Too often the essential collection of facts is performed in a perfunctory manner."[91] Chambers, in this paper, acknowledges help and stimulus from F.C.Bartlett. During the 1940s Bartlett, himself, made a number of criticisms of the psychometrics school. In 1947 he warned that intelligence testing tended to emphasise the use and understanding of symbols as intelligent behaviour and that this was a prejudiced and incomplete view.[92] He also argued that the extension of the use of tests to cover whole ranges of different skills was fraught with hazards. The main value of intelligence tests, he argued, was a pragmatic one in that their use for selection, particularly academic selection, did not depend on views about the precise psychological nature of intelligence, which was still hotly debated. Again in 1948

Bartlett warned of an overemphasis on tests and claimed that no problem of human behaviour can ever be solved in terms of testsalone.[93] Interestingly, again in the light of Burt's later downfall, he argued that most important for psychology was a "... most complete loyalty to experimental requirements, whether in or out of the laboratory."[94]

In 1950 another Cambridge psychologist presented criticisms of mental testing and of factor analysis in particular. In his An Introduction to Modern Psychology O.L.Zangwill[95] repeats Chambers' claims that psychologists are not agreed as to the nature of intelligence, and that nothing psychological is truly measurable. He quotes from Chambers the passage referring to 'dizzy numerical edifices' and his main argument is that intelligence testing is a technology whose theoretical foundations are insecure.[96] Of factor analysis he argues that its practice does not correspond to scientific method. Science, he characterises as involving three stages, the collection of facts, the devising of hypotheses to explain these facts, and the submission of these hypotheses to the test of experiment. But factor analysis does not follow this pattern, according to Zangwill, it procedes by first designing arbitrary tests of abilities, by assigning arbitrary scores to the results so obtained and, finally, by submitting the resulting correlations to a mathematical analysis which results in the isolation of factors. The factors are then identified with the original abilities, though their psychological significance is unknown, according to Zangwill. He claims that, if the objections are valid, factorial analysis will in future be viewed as a "...misguided departure from the central path of empirical psychology".[97] Of intelligence tests generally, Zangwill argues that they have some use in a pragmatic sense but that they sample a limited and arbitrarily selected range of capacities under highly artificial conditions. Ratings based on them must be considered of an empirical and rough order only. "Human intellectual capacity is too complex and many-sided for one to be able to place any great reliance on psychometric ratings."[98] He refers to Bartlett's (1947) paper on this.[99] On the question of intelligence he argues that intellectual capacity is, in large part if not wholly, inherited.

Comparative studies and studies of child development are discussed by Zangwill and he emphasises the evolution of intelligence. Also, while admitting that psychology has a role to play in the theory and practice of education, he warns that the psychologist must restrict himself to the study of the nature of mental development in general and refrain from judgements about the modification of educational practice.[100]

Another Cambridge psychologist who was critical particularly of the factor analytic school of psychometrists was A.W.Heim. In 1954 she published The Appraisal of Intelligence[101] which heavily criticised factor analysis. Much of the book concerned problems and reservations about various aspects of mental testing, including problems with the concepts of mental age and intelligence quotient, with the definition of intelligence, with the concept of test 'reliability' and causes of unreliability of tests, and with validating tests. But, while critical of various aspects of mental testing, the author is not against psychometrics as such. However she is highly critical of the factor analytic school, which she claims virtually dominated the field of mental testing. Some of her criticisms sound familiar. For example, "The tendency of the last few decades has been towards elaborate statistical techniques combined, at some later stage, with over-simplified and sometimes irrelevant psychological interpretations....".[102] According to Heim the factor analysts have used their technique as a means to an 'inappropriate' end and even as an end in itself. Her main criticism is that factor analysis, while involving complex statistical techniques and much computation, produces hypothetical factors of mind which are then given names based on intuition. She argues that even when factorists claim that factors are interpreted only as statistical phenomena relating to tests and not as psychological entities, they do not usually comply with this claim and proceed to talk of factors in terms such as 'general intelligence'. She is very critical of the tendency within factor analysis for controversies over numbers of factors revealed by a set of data, names of factors, results of specific tests and so on to be pursued without reference to fundamental premises. These controversies are fought, according to Heim, by the analysis and re-analysis of a given collection of data with different analysts producing different conclusions. Heim argues that, "It is clear that conclusion x or non-x can be extracted given the material, the technique, the inclination and the time."[103] She claims that a skilled practitioner can get out of factor analysis whatever he chooses. For Heim the way to resolve such controversies or to pursue psychological research in general is first to define the question and second to conduct a series of experiments to answer the question. Finally, a factor analysis of the results of these experiments may, or may not, help to verify specific and previously stated hypotheses. She concludes by arguing that the domination of mental testing by factor analysis, and the treatment of factor analysis as an end in itself, is sterile and may have actively interfered with progress in psychology.[104]

These, and other criticisms of mental tests and factor analysis in the 1940s and early 50s produced responsesfrom the psychometricians. In 1953 Charlotte Banks and Cyril Burt[105] replied directly to the criticisms of Chambers and Zangwill, claiming that it was both theoretically possible and practically useful to apply statistics to psychological phenomena. They compared these criticisms with those put forward by German intuitionist psychologists in the 1920s and 30s. It is interesting to note that in this early debate Spearman had not only criticised the intuitionists for ignoring statistical techniques, but had warned psychometricians against too much concentration on statistical technique without enough attention to theory.[106] Here Spearman argued, after Udny Yule, that the main function of statistics was to verify conclusions reached through other means. It is also interesting, in the light of later revelations about Burt's work, that Spearman warned of the 'mischievous tendency' of some psychometricians to suppose that exactitude of statistical technique can compensate for inexactitude or ineptitude of observation or experiment.

The first thing to notice when considering the criticisms of mental testing by Cambridge experimental psychologists, is the period during which they emerged. The most notable criticisms, which are those specifically discussed in this study, were published in the following years: 1943, 1947, 1948, 1950, 1954. The obvious question to ask is why did criticism of mental testing emerge from within psychology during this period? A preliminary answer to this question is provided by the critics themselves. In his critique Chambers reveals that he has serious doubts about the applications to which mental tests are being put. Similarly, Zangwill is worried about the role of psychologists in the field of education policy and he warns against the tendency of psychologists to make judgements on policy. For Bartlett it is the extension of the use of tests to cover a whole range of skills which prompts his criticism. These reasons given for the publication of criticism of mental testing from the mid-1940s onwards are highly significant and will receive further attention following the discussion of education policy in the next chapter.

In addition to this criticism from Cambridge experimental psychologists, in the later 1940s some criticism of mental testing emerged from social psychologists as their discipline began to break away from the stranglehold of McDougall's instinct based theory. The war had provided a spur to the development of social psychology, with attention being turned to the influence of society on the individual. Furthermore, a group of social psychologists, influenced by anthropological work and adhering to a 'culture-pattern' theory of human development, began to concentrate their work on the social influences on human behaviour, arguing that these were more influential than hereditary influences.[107] For example, Julian Blackburn, lecturer in Social Psychology at the LSE, published a number of works which included some criticism of mental testing.[108] It is interesting to note that Blackburn not only draws on anthropological work, but explicitly acknowledges the influence of F.C.Bartlett on his work.[109]

In the 1940s, as well as these general criticisms of psychometrics and factor analysis, the nature-nurture debate was spreading into British psychology from America, and from debates within the biological sciences in Britain. During the 1920s and 1930s a series of studies had been produced in America on children in foster homes and twins, to assess the relative contributions of nature and nurture to intelligence.[110] These were beginning to receive some attention in Britain.[111] One popular argument in this period, particularly amongst eugenicists, was that since intelligence was innate, and since there was a negative correlation between family size and intelligence test scores, then the intelligence of the population generally must be declining.[112] In 1949 there was a BA symposium on this subject with P.E.Vernon supporting this argument.[113] But L.S.Penrose, a geneticist, was critical of the argument and, in a book, he argued that there could be genetic reasons why the first two propositions of the argument could be true, while the conclusions could be false.[114] Furthermore evidence from surveys of intelligence published around the same time, conflicted with this argument. Another critic of this view was Heim, who was also critical of the nature-nurture dichotomy.[115] Drawing on Penrose's work, she argued that questions such as 'How far is x due to heredity, and how far to environment?' were absurd. In reviewing the evidence on nature and nurture, both P.E.Vernon and L.S.Penrose drew mostly on American studies.[116]

9.5 Summary

The first half of the century saw the establishment of psychology as a distinct scientific discipline, with its own institutions and cognitive content. In institutional terms the centres at University College, London, and at Cambridge were particularly influential in British Psychology. On the cognitive side the main theoretical trends were McDougall's hormic system, Spearman's factor approach to human intellect and cognition, and the psycho-analytic approach to clinical problems. Thus conation was understood in terms of innate instincts and cognition in terms of innate intelligence. Theories of innate intelligence and the practice of mental testing were central features of British psychology and, in the absence of rival schools of thought in Britain, they went largely unchallenged until the 1940s. The influential school at UCL was the home of this approach.

An important factor in the development of psychology as a discipline was the ability of psychologists to 'capture' various applied fields. Among these fields the establishment of educational psychology was particularly significant. Psychology provided a solution to two different types of problem facing educationists. It represented a tool in the professionalisation of education and it provided a technique to solve some major problems of selection. The most important area within educational psychology was that of mental testing, and its leading exponents were highly influential psychologists and educationists.

Psychometrics was then a central feature of educational psychology and of general psychology. Its proponents were influential within the science and the school at UCL was one of the most important centres for British psychology. This approach went largely unchallenged until the 1940s when criticisms began to appear from psychologists based at Cambridge. While Cambridge did not represent a rival theoretical school, it was characterised by a difference in methodology, emphasising experimental method in contrast to the statistics and factor analysis of the psychometricians. This criticism seems to have been motivated by fears of the over zealous use of psychological tests in various applications, including education. Criticism also emerged at this time from social psychologists who held a culture-pattern theory of human development which emphasised the influence of social factors on individual's behaviour. Also, at this time, criticisms of some eugenic arguments which were based on psychometrics began to appear, and they started to draw on evidence from the American nature-nurture controversy.

CHAPTER 10

EDUCATIONAL PSYCHOLOGY:

THE INFLUENCE ON POLICY.

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10.1 Introduction

Education policy as it developed in the period following the first world war, leading up to the Education Act of 1944, is usually thought to have been influenced strongly by developments in psychology.[1] However the nature of the relationship between education policy in this period and psychology bears some investigation since this relationship is not a simple one. One suggestion for the possible nature of the influence of psychology is made by Walker[2] who proposes that psychology, rather than initiating particular policy developments, provided a justification for options already favoured on other grounds. This suggestion receives some support from the evidence and from other secondary sources. Before assessing the nature of this relationship it is necessary to review some of the major developments in education policy during this period. This ground has been covered in some detail by a number of secondary sources, notably Simon[3], and these will be drawn on for the following review.

10.2 Education Policy

The structure of the education system as it was up until the end of world war two was largely formed in the first twenty years of the century. In 1899 the Board of Education was established and the 1902 Education Act established local education authorities. This Act also enabled the establishment of fee-paying secondary schools, aided by rates and exchequer grants, which were of an academic character being modelled on the public schools.[4] At that time these schools were separate from the system of elementary education which consisted of all-age schools taking children from ages 3 or 4 years to 13 or 14 years.

In 1907 the Liberal Government issued new regulations for secondary schools which required that 25% of places in these schools be offered freely to candidates from elementary schools. To qualify for one of these free places candidates were required to pass an examination which came to be known as the free place 'scholarship', taken at age 11 years. This examination became highly competitive and there was growing pressure on the limited number of free places. With a big demand for more facilities for a higher level of education than that provided by the traditional elementary schools, and with a limited number of free places in secondary schools, various types of post-primary schools developed within the elementary system. This provision varied from region to region around the country.

In 1900 higher elementary schools, taking children from age 12 to 15 years, had been established and further encouraged by the Board in 1905, but these met with very little success. From 1905 onwards a small number of junior technical schools, recruiting at age 13 years, were established and 1910 saw the introduction of the more successful 'central' schools in larger urban areas like London and Manchester. These recruited at age 11 years. Although these began by providing a different type of education to secondary schools, with a 'bias' of industrial or commercial character, they soon began to resemble secondary schools. However, being administered under the elementary code of regulations, they had poorer facilities than the secondary schools, administered under a different code, and soon became a kind of second best to secondary schools.

The end of the first world war saw great pressure for educational reform and in 1917 the call for free secondary education for all children became the official policy of the Labour Party and the TUC. In 1918 the Education Act, although not meeting the Labour call, did go some way to encourage educational reform, raising the school leaving age to 14 years with no exemptions, planning an expansion of nursery schools, proposing continuation schools and envisaging compulsory part-time education for all from the age of 14 to the age of 16 years. It also provided for an exchequer grant of 50% towards building costs, to encourage the expansion of the education service. It further laid the 'duty' on local education authorities of developing advanced post-primary courses for older or more intelligent children in elementary schools by providing central schools or other arrangements. Finally it established that no child should be debarred, "...from receiving the benefits of any form of education by which they were capable of profiting through inability to pay fees." [5]

With the shortage of secondary places and the intense competition for the limited number of free places, and with the Labour demand for free secondary education for all, the topic of secondary education was well on the political agenda following the first world war. The Labour call met with opposition from the Coalition Government which was dominated by Conservatives, but it received some support in local areas, for example in Bradford where Labour had a majority on the city council.[6]

In 1919 H.A.L.Fisher, President of the Board of Education and architect of the 1918 Act, appointed a Departmental Committee to investigate the problem of scholarships and free places for secondary education. The committee was chaired by Edward Hilton Young, a Liberal MP, and published its report in 1920.[7] It established that 21,000 potential entrants were being excluded from secondary schools through lack of space and argued that sufficient places should be provided to accommodate the number of children 'capable of profiting' from secondary education. This phrase meant 'intellectually capable' in relation to 'attainment, promise and capacity'.[8] The report concluded that practically all children, except the subnormal, are capable of profiting by full-time education up to the age of 16 years or beyond, and 75% of each age group was given as a target figure to work towards. The committee argued that secondary education need not all be of the same kind as that provided at the time. They argued that secondary education should be free but, as a first step to achieving this, the number of free places should be increased and maintenance allowances should be provided. The age of entry to secondary school should be 11 years according to the report, as this marks a definite stage in the development of the majority of children.[9] Selection for secondary school should be on the basis of a formal test of capacity and promise rather than attainment, according to the majority of the committee, although a minority registered a 'profound objection' to this proposal, arguing for tests based on normal school work. It is worth noting that Burt was among those giving evidence to this committee.

This report was warmly welcomed by educationists including R.H.Tawney. But, by 1920, economic pressures were beginning to be felt and education was singled out for criticism as Fisher and the Board were accused of squandering funds.[10] Through pressure from the Cabinet and a Select Committee of the House of Commons on national expenditure, the Board was forced to reverse its policy of encouraging reform. This set the pattern for education policy throughout the inter-war period. With pressure for reform coming from the Labour party, teachers and educationists, and from official committees on education, the Board facing cuts in its expenditure estimates followed a policy of restricting reform through administrative means.[11]

In 1922 the Geddes Axe fell, proposing an £18 million cut in education spending estimates. This was reduced to £6½ million by the Cabinet for political and contractual reasons.[12] At this time R.H.Tawney, as chairman of the Education Advisory Committee to the Labour Party and the Trades Union Congress (TUC), published Secondary Education for All: A Policy for Labour.[13] This pamphlet, laying out Labour policy, criticised the limitations of the secondary schools and argued for a variety of forms of secondary school, claiming that," Equality of educational provision is not identity of educational provision, and it is important that there should be the greatest diversity of type among secondary schools."[14]

In 1924 the Board of Education Consultative Committee, an influential body more authoritative than a Departmental Committee,[15] under the chairmanship of Sir W.H.Hadow, published a report on Psychological Tests of Educational Capacity.[16] The committee had been asked to assess what use could be made in the public system of education of psychological tests of educable capacity.. They started considering the matter in 1920. The committee consisted mainly of educationists including Tawney. The topic had been suggested in 1919 by E.K.Chambers and, by 1920, experiments with the use of group intelligence tests in free-place examinations were already under way in Bradford and Northumberland. The following year saw the publication of Thomson's account of the Northumberland experiment in the British Journal of Psychology[17] and of Burt's handbook Mental and Scholastic Tests.[18] Thus, psychological tests were a topic of some interest at this time, bearing in mind also the competitive pressure which the scholarship examination was under.

The committee decided that, because of the 'technical' nature of the topic, they would need, "...much expert assistance and advice,"[19] so they co-opted three psychologists to serve on a sub-committee, P.B.Ballard, C.S.Myers, and C.E.Spearman. They also sought the help of Burt who supplied evidence to the committee, wrote the first chapter of the report on the history of psychological tests and supplied memoranda for three of the report's appendices. In the first chapter Burt defines 'intelligence' as 'inborn, general, intellectual efficiency,'[20] and he mentions some of the experiments with the use of group tests in scholarship examinations undertaken by various local authorities. Details of these were provided in an appendix to the report. These, he claims, have been successful and statistical analysis of the results revealed that the test results corresponded closely with results of independent scholastic examinations or independent personal judgements. He further point out that group tests of intelligence were introduced into the competitive examination for clerical posts in the Civil Service in 1920, such that nearly 40,000 candidates had so far been tested. Here analysis showed that the psychological test correlates more closely with the general results than any other single paper. He concludes by describing as a 'remarkable achievement' the success and widespread use of intelligence tests.[21]

The need for intelligence tests is described by the report in the following terms: "It is obvious, as was pointed out to us by several of our witnesses, that some simple, uniform and trustworthy device for gauging the educable capacity of children is urgently needed."[22]

The report points out that intelligence tests are claimed to be more objective, more systematic and more trustworthy than ordinary written or oral examinations.

Most of the psychological witnesses had claimed that tests enable the prediction, from an early age, of the intellectual level of a child when he is grown up and that IQ remained 'tolerably' constant while the child is growing. Some specific claims for mental tests by witnesses were given as follows: they provide a method of comparing inborn capacity of children; although test results depended to some extent on environmental influences only a minimum of common environmental conditions was required and tests gave a more even chance to all candidates, than did any other form of examination; intelligence tests were prognostic whereas ordinary examinations were diagnostic; tests provide an objective standard of comparison, unlike other examinations; tests involve an accurate age allowance; group tests take less time to work and to mark than ordinary examination papers.[23] Three types of evidence are said to suggest the value of tests as indices of educable capacity: comparison of test results with observations of teachers and others, follow-up after testing of children's school careers, the correlation shown by experiment to exist between the results of such tests and those of ordinary standardised scholastic tests in the case of children who had been afforded normal facilities for education.[24]

The report reviews the different views of psychologists concerning the nature of factors involved in general intelligence, including the disagreement between Thomson and Spearman, Burt and others about the existence of a general factor. But the report sees these various hypotheses as reflecting merely a difference of emphasis. In answer to the question of what intelligence tests measure the report replies with what Hearnshaw would call a piece of 'pure Burt':[25] "What tests of 'intelligence' measure, therefore, is inborn, all-round, intellectual ability,"[26].

The report also outlines the main presuppositions which it was argued underlay the use of intelligence tests. These were: that there were certain mental factors which remained more or less constant during the lifetime of the individual human being; that methods of examinations had been discovered or could be discovered by which those factors in any individual could, to a great extent, be ascertained and differentiated from the results of training and education. It was also pointed out that the educable capacity of a child depended both on inborn psychological abilities and acquired capacities, but that inborn abilities constituted a limit which could not be exceeded.[27]

The committee reviewed different types of psychological tests and various possible applications of them in the field of education. The report concluded that tests would be most useful when used in connection with the free place examinations, particularly in areas where there were a large number of candidates. Also considered was the experimental use of tests within schools for various functions such as classification of pupils at entry, transference of pupils to classes or schools of different types and the determination of the causes retarding or accelerating the development of children. The conclusion was that tests of intelligence were of value as supplements to, but not as substitutes for, the existing methods of estimating individual capacity.[28] However, it is interesting to note that the report argued that any system of selection whatever which determines at the age of 11 years the educational future of children is gravely unreliable. The committee agreed with transference to secondary schools at age 11 years, but criticised the limited number of free places on the grounds of wasted capacity. This was a clear call for more free secondary education. They described the accommodation of secondary schools and the provision of free places as 'inadequate'.[29] As well as the provision of more free secondary education the committee obviously envisaged a common core of education past the age of 11, or at least some flexibility in educational opportunity after that age. Another conclusion was that tests should be devised, studied and interpreted by recognised experts.

The report recommended that the value of intelligence tests in free place examinations for entry to secondary schools and in selection for admission to central schools, should be investigated by tentatively adding group tests to the customary written examinations. The two sections of the examination would then be evaluated by calculating the correlation between the separate results and the subsequent development of the pupils. It also recommended the provision of courses in experimental psychology and modern statistical methods for teachers and medical officers to qualify to use group and individual intelligence tests. Finally it recommended the establishment by the Board of Education of an advisory committee to work in contact with university departments of psychology and other organisations engaged in research, to provide co-ordination and availability of the results of scientific research on psychological tests of educable capacity.[30]

For a few months in 1924 the first Labour Government was in power and Trevelyan, the President of the Board of Education, attempted to reverse some of the economic restrictions which had been placed on education. But, by the end of 1924 the Conservatives were back in control and, although the new President, Lord Eustace Percy, supported a steady expansion of secondary education, by 1925 another round of 'economy' was introduced.[31]

In 1926 the next report of the Consultative Committee, again chaired by Hadow, was published - <u>The Education of the Adolescent.</u>[32] The Committee's terms of reference were to report on, "...courses of study suitable for children who will remain in full-time attendance at schools, other than Secondary Schools, up to the age of 15..."[33]. The reference stated that attention should be paid to both the requirements of a general education and the desirability of providing a variety of curriculums, "...for children of varying tastes and abilities,"[34] and that the possible occupation of pupils should be considered. Contrary to the usual interpretations of the origin of this report which suggest that it was a Labour initiative, Simon has convincingly shown that the terms of reference were prepared by Selby-Bigge, a permanent secretary to the Board, with the help of some of the committee's members, and this was approbated by Edward Wood the Conservative President of the Board in 1924.[35] It is further argued, by Simon, that the reference was carefully worded so as to actually forestall any attempts to achieve secondary education for all.[36] In this context it is clear that the phrase, 'other than Secondary Schools,' was intended to preclude the committee from considering any change that would alter the position of the secondary schools and to channel their thoughts to an alternative system of education to secondary. It is further suggested, by Simon, that it was during the reign of the Labour Government of 1924, and Trevelyan's presidency of the Board, that the idea of a general organisation of education took firm root, influencing the thinking of members of the Hadow committee.[37]

The committee again paid attention to the views of psychologists and educationists. Although Burt was not called as a witness he did supply memoranda and information to the committee. An important and influential witness was T.Percy Nunn, whose <u>Education its Data and First Principles</u>, published in 1921, was highly influential among educationists.[38] He was also a member of the drafting sub-committee and Burt has claimed that he contributed some important passages to the report.

Significantly, the report, indicating the lines of advance, stated that: "The question is not one of erecting a structure on a novel and untried pattern, but of following to their logical conclusions precedents already set, and of building on foundations which have long been laid."[39]

The committee revealed that there was, more or less, a consensus amongst the witnesses, as well as general agreement among teachers and administrators, that the age of 11 to 12 years should be treated as the beginning of a new phase in education. This consensus existed for two reasons according to the report, the arguments of psychologists and developments in the organisation of the education system.[40] Psychologists argued that the age of 11 or 12 years marks a point of transition from childhood to adolescence and by this time children have given some indication of differences in interests and abilities, "...sufficient to make it possible and desirable to cater for them by means of schools of varying types, but which have, nevertheless, a broad common foundation."[41] Educational organisation had already marked the years 11 to 12 as the natural turning point up to which primary education leads and from which post-primary education starts. The free place examination took place at that age and transfer to other forms of post-primary education, such as central schools, also took place then. Furthermore, the raising of the leaving age to 14 years also made 11 to 12 years the appropriate age for a turning point.

The report concludes from this that arguments derived from educational theory are reinforced by practical considerations. However, the nature of the recommendations made in the report, the emphasised general agreement amongst teachers, administrators and legislators, and the long history of discussion and organisational development based on the age 11 to 12 years, suggests that this statement may be more appropriate when turned around the other way. That is, the practical considerations are reinforced by educational theory. This point will be expanded later when considering the influence of psychologists.

The report recommended the raising of the school leaving age up to 15 years and the provision of a variety of types of post-primary school. The report argued that this variety was necessary because of the variety amongst individual children, " There are a diversity of gifts, and for that reason there must be diversity of educational provision, "[42] and further, using Tawney's phraseology, "Equality, in short, is not identity...(it is) essential that education should not attempt to press different types of character and intelligence into a single mould..."[43]. It is further pointed out that the 1920 report of the Departmental Committee on Scholarships and Free Places strongly emphasised the need for variety in the post-primary system.

The Hadow report recommends that all post-primary education should be known as 'secondary' education, thus producing a type of secondary education for all. With a variety of types of secondary education the committee hoped to replace, 'selection through elimination', with 'selection by differentiation'.[44] The types of school recommended were secondary schools of the existing type now to be called grammar schools, central schools, non-selective central or 'modern' schools, senior classes, junior technical and trade schools. These six types of school were already in existence in various parts of the country, though the last five only accounted for five per cent of the elementary school population between the years of 11 and 14.[45] It is interesting to note that only one witness to the Hadow Committee had recommended the adoption of a single secondary school. The report recommended an examination at 11+ to discover the type of school most suited to the abilities and interests of the child. This examination might be supplemented with a psychological test, particularly in dealing with borderline cases or cases where the results of the written examination and the teacher's estimate of proficiency do not tally.

Another piece of evidence given to the committee which is worth noting is that from the Education Advisory Committee of the Labour Party and the TUC, whose chairman was Tawney. This committee argued that all children should be transferred at age 11+ to some form of secondary school or central school administered under the secondary regulations, and stay in school until as near as possible to the age of 16 years.[46]

The report was generally well received in the educational press and journals and by teachers' organisations and educationists, but there was some criticism of the report's advocacy of psychological tests as part of the examination.[47] The Board of Education refused to sanction the recommendation of raising the leaving age to 15 and revealed, in 1928, that its interpretation of 'Hadow reorganisation' was of a, "Readjustment of the existing elementary system". The Board's policy was clearly to retain the existing secondary schools in their present form.[48]

From 1929-31 a Labour Government was in office and Trevelyan was again President of the Board. However, attempts at educational reform were blocked by a Chancellor of the Exchequer still concerned with 'economy' measures as well as other factors and Trevelyan eventually resigned. In 1931 a Coalition Government took over and a new financial crisis appeared with another Geddes type committee, the May committee, insisting on economy measures and reversing educational advance.[49]

In 1931 the Consultative Committee, still chaired by Hadow, produced another report, this time on The Primary School[50]. In an appendix to the report, on the mental characteristics of children between the ages of 7 and 11, Burt argued that older children differ more widely in intellectual capacity than younger children.[51] He went on to argue that, as infants they can be grouped together regardless of mental endowment but, by the age of 8 or 9, that mental endowment is extremely heterogeneous and, by the age of 10, children need to be spread over at least three different standards. Furthermore: [52]

> "...and by the age of 12 the range has become so wide, that a still more radical classification is imperative. Before this age is reached children need to be grouped according to their capacity, not merely in separate classes or standards, but in separate types of schools."

The committee accepted this argument and recommended the introduction of streaming with a 'triple track system or organisation'.[53] The advice was frequently followed in large schools, and streaming was employed, not just before the age of 11, but also in some of the separate post-primary schools.[54]

The policy of 'economy' continued until 1935 when a National Government, dominated by Conservatives, was elected. In 1936 a Board circular lifted some restrictions which had been placed on educational development, for example, restoring the exchequer grant of 50% towards building costs which had previously been withdrawn.[55] This enabled the building of new senior elementary schools which a Conservative Board was in favour of, since this pre-empted calls for more secondary provision. In the same year an Education Act was passed which set the date of 1939 for raising the leaving age to 15. This Act was highly criticised in delaying the measure for so long and for allowing exemptions and not providing maintenance allowances.

By 1936 the call for 'secondary education for all' had been muted and, according to Simon, after a series of policies of retrenchment and 'economy', educational reformers were trying to get educational development back to the stage that it was before these policies. In 1935, for example, the Labour Party's election manifesto had pressed to get back to even the limited measures of 'Hadow reorganisation' that the Board had laid down in 1928.[56]

Although differentiation appeared to be taking root in education and policy circles, an alternative system of organisation had been proposed and was supported, particularly by teachers' organisations. This was the concept of the multilateral school, a common secondary school providing courses with different 'biases', which was a means of bringing different forms of secondary education under one roof. This idea began to emerge in about 1925 when the Association of Assistant Masters passed a resolution at its annual conference in favour of secondary education for all in one type of school. Throughout the 1920s and 1930s this idea was reiterated by other bodies representing teachers, some of which drew on the American system of education as an example. Those supporting the multilateral idea accepted that children differed in their intelligence, interests and rates of learning but argued that these could be catered for in one type of school.[57] It is pointed out, by Simon, that the first time that the issue of the multilateral school was raised at an official level was in 1928 when a deputation from the NUT put the idea to the Board.[58] According to Simon, two-thirds of teachers associations, in their reactions to the Hadow report, favoured the multilateral school.

In the 1930s this idea was gaining support and it is interesting to note that Godfrey Thomson in A Modern Philosophy of Education (1929) came out in favour of the single secondary school, following his experience in America where he had been a visiting Professor at Columbia University.[59] According to Rubenstein and Simon, Thomson's book was the first important educational work to use the American term 'comprehensive' for the single secondary school.[60] As well as teachers' organisations the TUC came out in favour of the multilateral school in the 1930s as did the London Labour Party. In 1935 a sub-committee of the London County Council (LCC) advocated the establishment of multilateral schools, although at the time this was not legally possible because of the different statutory regulations then governing elementary and secondary education.[61]

By the mid-1930s a new call for educational reform was developing. With a new type of sociological study into education revealing a massive wastage of human potential through the exclusion of working class children of high ability from secondary schools, combined with a stress on the economic benefits of education, the pressure for educational reform grew.[62] The Board of Education were only interested in making minor adjustments to the education system though, and were therefore concerned with fending off the wastage criticisms. One aspect of Board policy at this time was to increase the numbers in secondary schools, not by allowing more pupils to enter, but by extending the leaving age in these schools thus consolidating secondary schools in a different class of their own. The Board was also concerned with finding a way of combating the over-specialisation of the secondary curriculum.[63] This helps to explain the interest shown by the Board in the junior technical schools.

At this time the Consultative Committee, under the chairmanship of Will Spens and with a number of new members, was given a new reference to report on. This was to consider the "...organisation and inter-relation" of schools, "...other than those administered under the elementary code, which provide education for pupils beyond the age of 11+", with particular attention, "...tothe framework and content of education of pupils who do not remain at school beyond the age of about 16".[64]

From this remit it is clear that the report was to be concerned with secondary education and, as Simon argues, that, since the Board intended to raise the leaving age for the existing secondary schools to 17, the intention was to develop a different type of secondary school to cater for those leaving at 16.[65] As Simon further points out, what the Board probably had in mind was to bring the existing junior technical schools under the secondary code of regulations, with an entry age of 11 instead of 13 and adding to their number. The Board, in framing the Committee's remit, had also restricted discussion to secondary education, just as the Hadow report had been confined to elementary, and this clearly indicated the intention to keep the two systems separated. This move met with some criticism from teachers' organisations.[66]

In giving evidence to the committee, a number of organisations, including the TUC, the County Councils Association, the Association of Directors and Secretaries of Education, called for the establishment of a common code of regulations for all post-primary education, which would imply that all secondary education must be free. A number of organisations argued for the introduction of a single secondary school, though interpretations of what these would be like varied. There was also pressure to raise the leaving age for all children to 15.[67]

The report was published in 1938 and, again, there were numerous contributions from Burt, including a chapter on the 'Physical and Mental Development of Children between the Ages of 11+ and 16+' of which the section on mental development was based on a memorandum from Burt, and an appendix by Burt on 'Faculty Psychology'.[68] The report accepts Burt's arguments concerning 'general intelligence', notably that it, "...seems on the whole to be the most important factor in determining his (the child's) work in the classroom."[69] In the light of the later arguments concerning different types of secondary education, it is worth quoting the following passage as well:[70]

> "We are informed that, with few exceptions, it is possible at a very early age to predict, with some degree of accuracy, the ultimate level of a child's intellectual power, but this is true only of general intelligence and does not hold good in respect of specific aptitudes or interests."

The report also reiterates Burt's arguments concerning the increase in individual differences in intelligence as the child gets older to the point where, "Different children from the age of 11, if justice is to be done to their varying capacities, require types of education varying in certain important aspects."[71]

It was recommended, by the report, that all types of post-primary school be of equal status, administered under a single secondary code of regulations, thus taking the Hadow recommendations to their full conclusion. The idea of the multilateral school was rejected largely for administrative reasons.[72] The report did, however, allow for some introduction of multilateral schools as an experimental measure in sparsely populated areas and on new housing estates. But the pattern of secondary education envisaged by the committee was a tripartite one consisting of three different types of schools of equivalent status. These were the existing secondary schools renamed 'grammar' schools, new technical secondary schools an idea derived from the already existing junior technical schools, and modern schools of secondary status as advocated in the Hadow Report. It was envisaged, by the committee, that grammar schools would cater for only about 15% of an age group which Simon argues is in keeping with the Board's policy of maintaining these schools in their current form and restricting their intake.[73]

A large part of the report was concerned with aspects of curriculum and methods of teaching. On the subject of curriculum the report recommended that all types of secondary school should have a common curriculum up until the age of 13, to facilitate transfer from one type of school to another. The report seems to accept the 11+ examination as successful and the views of the psychologists, particularly Burt, on the value of mental testing. Selection for technical schools was to be through the same examination as for grammar schools, supplemented by an interview with both the child and parents.

This report proposed the measures necessary for establishing secondary education for all, however it did meet with some criticism in proposing a tripartite structure of secondary education on the grounds that this would perpetuate inequality of opportunity. Generally, however, the report had a warm reception.[74]

The Board rejected the proposals of the Spens report of establishing a common code of regulations and of abolishing fees or extending special places on the grounds that these would involve unacceptable increases in expenditure. The parts of the report concerning technical high schools and grammar schools curricula were praised by the permanent secretary of the Board.[75] With the outbreak of war even the raising of the leaving age to 15 which, under the Education Act of 1936, was to be done in September 1939, was shelved. It is claimed by Simon that, by this time, the Board wanted to dispose of the Consultative Committee.[76] However, calls for educational reform were growing.

In 1941 the Board issued a memorandum on, 'Education After the War',[77] which accepted the Hadow policy of secondary education for all over the age of 11 and advocated a tripartite system of secondary schools following the Spens recommendations. In 1942 the Labour Party Annual Conference gave official support to the idea of multilateral schools and this was supported by the TUC and other bodies, including the LCC. Although the issue of the single secondary school was discussed at this time more than it had been before, according to Rubinstein and Simon it was overshadowed by other major educational issues. Pressure for reform concentrated more on the need for raising the standard of all post-primary schools to the level of existing secondary schools and for abolishing fees.[78]

In 1943 the Board published a White Paper on

Educational Reconstruction [79] confirming its commitment to the tripartite system as proposed by the Spens report, with some allowance for a type of multilateral school in some areas. Later in the same year a report was published entitled Curriculum and Examinations in Secondary Schools.[80] This was prepared, not by the Consultative Committee, but by a special committee of the Secondary Schools Examination Council. The committee was chaired by Sir Cyril Norwood, a former headmaster of Harrow, and it lacked the relative independence of the Consultative Committee.[81] The Norwood committee did not seek the advice of psychologists, although they discussed the nature of the child and the child's mind. In fact the committee made their views with respect to psychology very clear at the beginning of the report, claiming that they had, "No sympathy with a theory of education which proposes that the aim can be dictated by the provisional findings of special sciences."[82] Furthermore, while the remit of the committee was to consider curricula and examinations in secondary schools, it went beyond this to consider the organisation of secondary schools. The report recommended a tripartite system of education but introduced a new justification for this. It was argued that educational practice had revealed three rough groupings of children with different types of interests and abilities. For these three groups of children three types of secondary school were needed, grammar, technical and modern. It is important to note that these types of pupil were supposed to have emerged from educational experience and were not based on theory.

So far as the committee were concerned, "Whether such groupings are distinct on strictly psychological grounds, whether they represent types of mind, whether the differences are differences in kind or in degree, these are questions which it is not necessary to pursue."[83] The main consideration of the report, with respect to secondary organisation, was that children with particular types of mind should have a training suited to them which could lead to an occupation suited to them. In attempts to justify the concept of a group of children with a technical type of ability, the report claims that, "The various kinds of technical school were not instituted to satisfy the intellectual need of an arbitrarily assumed group of children..."[84] but they were set up to prepare children for certain crafts and it is usual to think of these children as having a particular set of interests or aptitudes. This argument derives from the structure of the educational system and then imposes on children the characteristics of that system, basically to justify the continuation of that system. They seem to be arguing that, because there is a technical type of school, there must be a technical type of child otherwise that type of school would not have been introduced, while at the same time admitting that the school was introduced to train pupils for a particular type of occupation. It seems to be the Norwood committee itself who have introduced the 'arbitrarily assumed group of children'. On the subject of allocation at 11+ to different types of schools, the report recommends that the teachers' judgement should be the major deciding factor, with intelligence tests used only as a supplement to this.[85]

The report met with strong criticism from many quarters including educationists, teachers, psychologists and local authorities. The chairman of the Consultative Committee, Will Spens, had registered concern with the President of the Board about the new committee going over ground already covered in his committee's report. The LCC criticised the report as a piece of rationalisation[86], and Burt was critical of the idea of different types of mind which, he claimed, was in contradiction to the facts as revealed by psychology.[87] He argued that the proposal of different types of mind was similar to the discredited views of the old faculty psychology and phrenology. The criticisms which Burt makes of the Norwood report are not altogether consistent with his views as expressed elsewhere. He criticises the 'clear-out' at eleven, arguing that this is based on administrative rather than psychological reasons, yet his own evidence to the Hadow committee seemed to at least confirm the age of 11+ as appropriate for the break between primary and secondary education if not actually to propose it. It has been suggested, by Hearnshaw, that at least some of Burt's criticism may have been motivated more by pique at the committee's neglect of psychological evidence rather than by fundamental disagreement with the committee's conclusions.[88] It is actually admitted by Burt that he agrees generally with the policy recommended by the report, but he disagrees with the reasons given in support of that policy.[89] Although Burt criticises a number of aspects of the report the most justifiable criticism, given his views as expressed elsewhere, is that of the 'types of mind' idea. His justification for different types of secondary school was based on the concept of different levels of general intelligence not on specific aptitudes.

The Education Act 1944, sponsored by R.A.Butler as President of the Board, was the culmination of this period of policy development. The Act finally established secondary education for all, visualising the education system as a continuous process with three phases; primary, secondary and further education. It raised the leaving age to 15 years with an allowance for a further raise in the future. With respect to the organisation of secondary education the Act was very general, making no mention of types of secondary school. Local authorities were required to give:[90]

> "...all pupils opportunities for education offering such variety of instruction and training as may be desirable in view of their different ages, abilities and aptitudes, and of the different periods for which they may be expected to remain at school, including practical instruction and training appropriate to their respective needs."

As Rubenstein and Simon point out, the wording suggests that a tripartite system was in mind and Butler made his support for this clear in Parliament, but other forms of secondary provision such as the multilateral system would now be legally possible.[91] The Act also established a Ministry of Education and reorganised the structure of local authorities. The local authorities were required to submit development plans to the new Ministry of Education which now had powers to control and direct the implementation of educational policy.

It is interesting to note that the LCC proposed to reorganise most of its secondary schools on multilateral lines. While London and some other authorities proposed multilateral systems of secondary education, the majority of authorities stuck to the tripartite model. With some exceptions then, the period 1945-51 saw the tripartite system firmly established. Three reasons are given for this by Rubinstein and Simon, these are that the different types of school already existed, many authorities were more concerned with the reorganisation of their all age elementary schools which had not yet been accomplished, and with the raising of the school leaving age in 1947 there was an urgent need for the provision of new facilities involving planning, building and equipment.[92] It is argued therefore, by Rubinstein and Simon, that many local authorities had to concentrate on immediate necessities to meet their obligations under the new Act and they found it easier to accept the structure that had already developed rather than opting for radical change.

Furthermore, following the Education Act, the Ministry of Education now had increased powers over local authorities and these were brought to bear in influencing the character of secondary provision. In a series of publications following the Act, including a circular in 1945, two pamphlets in 1945 and another pamphlet in 1947 which was reprinted until 1958, the Ministry confirmed that it favoured the tripartite system.[93] Even though the Labour Party came to power in 1945 having earlier pledged support for multilateral secondary education, two successive Labout Ministers of Education continued to favour the tripartite system and the Ministry continued this policy under their leadership. To some extent what support there had been for multilateral schools waned during this period, largely through fear of damaging the grammar schools.[94] The Ministry did not shirk from exercising its power and in 1949 a number of proposals for comprehensive schools were turned down.

Although the tripartite system was the favoured policy of the Ministry, provision of technical schools was small and the system developed more on bipartite lines. Officially it was argued that equality of opportunity had been achieved with selection for grammar school places now on objective lines being based on examinations consisting of English and Arithmetic attainment examinations and intelligence tests, not being based on parental income since all places were free.

It was not until after 1951, when the Conservatives were elected into Government that the issue of differentiation versus multilateral or comprehensive education began to develop into a party political issue.[95] The issue did not even figure in the 1950 election manifestos.[96] Even at that stage it was only an issue amongst educationists and local authorities, it began to emerge as a political issue later in the decade.

10.3 The Influence of Psychology

10.3.1 Introduction

Discussing the 'Intelligence' test, in 1953, Simon has argued that:[97]

> "In the last resort, it is the very keystone of the present educational system, for the theory that children can be divided into different groups, that they have fundamentally differing mental capacities which determine their whole future development, is derived from the theory and practice of intelligence testing."

Clearly there was an intimate relationship between psychology and both education policy and educational practice. But the question is, what was the nature of this relationship? More specifically, what was the influence of psychology and psychologists on education policy? From the previous account of the development of education policy in the first half of the century, it is clear that different chronological stages in the process, representing agenda formation, policy development and implementation are difficult to discern. However, these concepts are not totally inapplicable but must be applied with caution. In this case the distinction between policy and implementation is a particularly useful one. In fact this example of the policy process serves to elucidate the model demonstrating particularly the iterative nature of the various phases of the process.

Following the first world war problems in education were firmly on the political agenda. With intense competitive pressure for the small proportion of secondary school places which were allocated as 'free places' the issue of secondary and post-primary education was particularly to the fore. The 1918 Education Act seemed to suggest that a programme of educational development was on the way and pressure was being exerted from various quarters to bring about reform, the Labour campaign for 'free secondary education for all' being the most notable example.

During the inter-war period education policy developed under the guidance of the Board of Education in a context of strict Government economy measures. While the Board pursued its particular doctrine different local authorities also had different policies and there was thus some variety in the implementation of policy measures around the country. However, the major feature of this period was economic cut-backs retarding all forms of educational reform. The real significance of the inter-war period is the development of ideas concerning policy through the series of official committee reports and public campaigns for reform combined with ideas forming within the Board in the light of existing patterns of organisation. These ideas were crystallised in 1944 with a new Education Act. While the inter-war period might, then, be characterised as a period of policy development, the caveat must be added that this development was informed by the various measures implemented around the country by local authorities.

The 1944 Act, as a statement of policy, was rather unspectific concerning the organisation of secondary education, which means that the implementation of this Act is where the true nature of the Board's and the local authorities' policies can be found.

This is an outline of the various phases of the education policy process as it developed in the first half of the century. It is now time to discuss the influence of psychologists and their ideas on this process.

The influence of psychologists on education policy is interpreted in different ways by different secondary sources. In his biography of Burt, L.S.Hearnshaw acknowledges the influence of Burt on the Consultative Committee under the chairmanship of both Hadow and Spens. He writes, "Of this influence there can be no doubt."[98] But Hearnshaw goes on to argue that the reports of this committee were not particularly originative in that their recommendations followed developments that were already being made in the organisation of education. Thus, he concludes that the committee, and therefore Burt, cannot be held as wholly responsible for the particular education policy that emerged. One problem with this argument is that it misses the point that Burt was involved in some of those very developments in the education service which influenced the Consultative Committee, through his work with the L.C.C. However, the need to question the originative role of that committee is a real one. It is clear from the recommendations in some of the reports, as well as from other commentaries in secondary sources, that many of these recommendations did reflect the existing pattern of educational organisation. But, again, it can be asked why this was the case. Was the evidence of psychologists critical in leading the committee not to break away too far from the existing pattern of educational provision?

In his numerous discussions of this issue Simon makes various points about the role of psychology and its practitioners in influencing policy. He is probably one of the 'left-wing critics' whom Hearnshaw accuses of unfairly regarding Burt as the architect of the selective school system.[99] But Simon's interpretation of the role of the psychologists is not quite that simplistic. In 1978 he pointed out the important role which Burt had in providing evidence for the Consultative Committee and, in reference to this evidence, he claims that, "It provided, in fact, the theoretical rationale for the selective and hierarchical system of public education as it was developed at that time." [100] This argument does not seem to claim that the committee's ideas originated with Burt, but simply that Burt provided a theoretical justification for those ideas. He goes on to claim that it was Burt's evidence that provided the rationale for the recommendation by the Spens report that children over 11 should be separated in different types of secondary school. On the same page, Simon further claims that the Consultative Committee not only accepted Burt's theory, but also its educational implications in terms of streaming and selection. This does seem to imply an originative role for Burt. The basis of Simon's argument in his 1953 book is that intelligence testing is the keystone of the selective system and arguments for or against selection, and its rival the common secondary school, stand or fall with the theory of intelligence testing.[101] In a later book covering developments in the inter-war period, his arguments are more subtle and more detailed.[102]

Another author, Kazamias, argues that many of the main reommendations made in the Consultative Committee's reports were ideas which were in currency at the time of the Bryce Commission in 1895.[103] The difference which Kazamias identifies between earlier proposals concerning such issues as the age of transfer to secondary schools and the later proposals of the Consultative Committee is that the latter introduced a psychological justification for their recommendations. This sort of argument is reiterated by Rubenstein and Simon, who point out that the principle of differentiation was already well established before World War One.[104]

A number of authors seem to question the originative role of the Consultative Committee reports, but emphasise the importance of psychological theory in providing a new justification for policy measures. So far we have been looking at the influence of psychologists on education policy at a general level following the analyses of various secondary sources. To assess the character of this influence it will be necessary to look more closely at particular policy issues and areas of influence.

10.3.2 Different Types of Secondary School

The argument that there should be a variety of types of secondary school was never really challenged forcefully at a high level until after the 1944 Act, in fact it did not become a party political issue until the 1950s.[105] Throughout the inter-war period there was something of a consensus on this issue. Although there was support among teachers' organisations for multilateral schools, the main point of emphasis was on the issues of free secondary education for all and raising the school leaving age. Furthermore, those arguing for multilateral schools did not envisage the same type of education for everyone, but a variety of courses within one school.[106] In 1920 the Labour policy document Secondary Education for All, prepared by Tawney, criticised the limitations of the literary academic curriculum of the existing secondary schools and called for a variety of forms of secondary school.[107] It was this document which provided the source for the phrase, later repeated in the Hadow report, 'Equality of educational provision is not identity of educational provision'.[108] While calling for universal secondary education it also called for a diversity of types of secondary school. The reason for favouring a variety of types of school seems to derive from an appreciation of the value of curricula developed in higher grade schools, science schools and technical day schools, and criticism of the narrow curricula of existing secondary schools.[109] However, Hearnshaw argues that Tawney was influenced in this partly by Burt's work on the distribution of educational ability.[110] This interpretation seems to be at variance with that of Simon who identifies a subtle change in the arguments concerning a diversity of secondary provision between Tawney's 1920 document and the Hadow report of 1926.[111] He argues that, while the first document envisages a real variety of education, the Hadow report places emphasis on variation in the abilities of children, producing the argument that diversity of gifts requires a diversity of educational provision. This view expressed in the Hadow report is then embellished with some of Tawney's phraseology taken from the Labour policy document.

It is certainly reasonable to suppose that when preparing the 1926 report the Consultative Committee, including Tawney, should have been subject to a considerable influence from the ideas of psychologists, especially Burt, after having recently prepared a report on psychological tests.[112] Certainly, in the Hadow report, arguments for a diversity of educational provision at the secondary stage are being supported by psychologists' ideas concerning variety of ability amongst children. The question remains whether this was a means of providing justification for a policy option already formed on other grounds. Before further discussion of this issue in the context of the Hadow report it will be useful to look at the views of other bodies involved.

The Board of Education, and Conservative President of the Board, appear to have favoured a diversity of post-primary education for other reasons. The Board was concerned to keep the existing secondary schools intact and, to this end, were keen to fend off calls for secondary education for all and to encourage the development of alternative post-primary provision. As Simon convincingly argues, the careful framing of the reference for the 1926 Hadow report by Selby-Bigge represented part of this strategy, as did the Conservative manifesto pledge in 1924, subsequently supported by Percy as President of the Board, to encourage the development of central schools.[113] In this context it is interesting to note that in 1920 the main policy of a Conservative controlled LCC was to double the number of selective central schools, while Bradford, for example, was providing a much higher number of secondary places, all of which were free.

The 1920 Departmental Committee on Scholarship and Free Places, while concluding that practically all children were capable of profiting from full-time education up to the age of 16, also made the point that secondary education need not all be of the same type as that already existing. It is interesting to note that this committee appears to have used concepts borrowed from the psychologists to argue in favour of secondary education for all.[114] As mentioned above the committee was concerned with those 'capable of profiting' from education in terms of 'attainment, promise and capacity'. Needless to say Burt was among the witnesses giving evidence to this committee.

It is clear from the quotation cited earlier, in which the Hadow committee revealed their intention of following the precedents already set in the education system to their logical conclusions, that this committee were greatly influenced by the existing educational structure.[115] It has been argued, by Kazamias, that the rationale for differentiation cannot be explained solely in terms of the intellectual climate of the post-war period. So that, while the committee cited the arguments of psychologists and educationists such as Percy Nunn in support of differentiation, the notion of diversity had been the concern of official bodies as far back as the Schools Inquiry Commission and the Bryce Commission.[116] It is further argued by Kazamias that the roots of the Hadow stance on differentiation, and it may be added the general consensus on the need for diversity in post-primary provision, lay in the British social ideology concerning the concept of 'equality of opportunity' and 'leadership'.[117] The force underlying the whole character of the Hadow report was the need to establish 'equality of opportunity' which did not imply to them identity of provision.

By the time of the publication of the Hadow report there was plenty of support for diversity of educational provision for children at the post-primary level. The fact that only one witness to the Committee supported the multilateral idea illustrates this point. The Board encouraged post-primary education other than secondary to protect the existing secondary schools, Labour educationists called for a diversity of types of secondary education to avoid the narrow specialisation of the existing secondary schools, and the Hadow report justified its support of a diversity of secondary provision, based on the existing situation, on psychological grounds referring to children's differing abilities. What is more, even when some teachers' organisations came out in favour of the multilateral idea after the publication of the Hadow report, they still supported the idea of diversity in education.

In answer to the key question of the influence of psychologists' ideas on these arguments, it seems clear that all these various groups, with their own particular reasons for favouring a diversity of provision at the post-primary level, based their ideas on the existing situation in the education system and used psychological ideas to provide the justification for their arguments.

When the Spens report came to consider the idea of multilateral schools, this was rejected on administrative grounds, contrary to Rubenstein and Simon's claim that it was on psychological grounds.[118] The Committee gave six reasons for this rejection: problems of size; the role of the sixth form; difficulty of finding heads competent to deal with 'grammar' and 'modern' sides; and special needs of technical education.[119]

10.3.3 Selection at Age 11+

On this issue Kazamias again argues that the principle was not a phenomenon of the post-war period.[120] He points out that opinions on this issue were given to the Bryce Commission and the idea of a transition from elementary to secondary education at age 11+ is found in the Commission's report. Since local authorities adopted the policy of selection at age 11+ for the scholarship places at secondary schools, the idea had been common since 1902. This age was accepted as the appropriate one for the beginning of secondary education by the 1920 Departmental Committee, by Tawney in Secondary Education for All, 1922, and by the Board in a circular in 1925.[121] In 1920, as Simon points out, the LCC development plan had claimed that it was generally recognised that there were three distinct phases in elementary education, 5-7 years, 7-11 years and 11-14+ or 15+ years.[122] The Departmental Committee had also argued that the age of 11 marked a definite stage in the development of children.[123]

In 1923, when the new reference for the Consultative Committee was being discussed, Selby-Bigge, the Permanent Secretary to the Board, made it clear that the Board wished to fend off calls for 'Secondary Education for All'.[124] He further explained that the favoured alternative was to develop advanced elementary education rather than secondary education. By this time at least one local authority had reorganised its elementary system into separate primary and post-primary schools, with the age of transfer at 11+, and other authorities were following suit. The Board wished to encourage this development and Selby-Bigge informed members of the Consultative Committee of this fact. In 1925 a Board Circular presented this pattern as official policy to which all new building had to conform, and the age of 11 was taken as the most suitable dividing line between junior and senior education.[125] Later, Selby-Bigge claimed that the Hadow Report endorsed the Board's policy of transferring all children in elementary schools at the age of eleven plus to separately organised senior schools. It is argued by Simon that this recommendation, "Was virtually inescapable on administrative grounds."[126]

As already indicated, the Hadow report recommended the age of 11+ as the appropriate one for transfer to secondary education and it referred to the existence of a consensus among their witnesses, as well as among teachers and administrators, on this issue. The committee argued that the consensus derived from psychologists' arguments and developments in the organisation of the education system.[127] The report presents the issue as one derived from educational theory and reinforced by practical considerations. However, given the longstanding tradition of selection for secondary education at 11+ and the longstanding general acceptance of this practice, it might be more accurate to say, as suggested earlier, that this was a case of practical considerations being reinforced by educational, or psychological, theory. There is no doubt that the psychological theory was supplied by Burt with his submission on the significance of differences in ability, particularly after the 11+ stage, and by Percy Nunn with his characterisation of the post 11+ stage as 'adolescence'. In his book, Education its Data and First Principles, Nunn had supported the idea that the age of 11+ was the appropriate one for transfer to secondary education.[128] Clearly much of the flavour of the Hadow report and, perhaps, even the title derives from Nunn's ideas with respect to the phase of 'adolescence'. He was an important co-opted member of the drafting sub-committee. In his book Nunn identifies three mental stages in a continuous education process, infancy (1 - 6 or 8 years), childhood (6 or 8 - 11 or 12), and adolescence (11 or 12 - 18). The influence of Nunn is emphasised by Kazamias and by Simon.[129] However, with respect to the issue of the age at which transference to secondary school should occur, it appears that Nunn's ideas merely provided a rationale for an already existing consensus. An interesting topic of investigation would be the origin of Nunn's ideas; did they derive from psychological theory or educational practice?

10.3.4 Streaming

The question of the influence of psychologists on the introduction of streaming in schools is relevant because streaming was an important component of the whole system and because it is relevant to the consideration of the development of tripartism.

With the introduction of the free place system for secondary schools at the beginning of the century and the increasing competitive pressure on the scholarship examination, local authorities began to adopt a policy of promoting likely candidates for scholarships through the elementary school at a faster rate than other children. This policy was endorsed by the Board in 1918, but it led to the problem of having classes containing children of widely different ages, a problem referred to by Burt in 1917.[130]

According to Simon, the idea of selection by differentiation within the elementary school, that is streaming, derived directly from these practical problems and the Board's approach to them, and this approach was formed by the development of mental testing.[131] Already, in the early 1920s, precedents existed of schools divided into junior and senior departments with a system of three parallel forms for each year-group with differentiated curricula.[132] Although he did not, at that stage, advocate streaming as such, Burt did discuss the possibility of having classes containing children with similar educational abilities in his 1917 report to the LCC.[133]

In a report to the LCC in 1925 Burt argued that, "The ideal plan," for organising schools, "Would perhaps comprise a 'treble track' system,"[134] comprising backward classes for slow children, advanced classes for quick children and ordinary standards for children of average ability. While the Board was concerned with the practical problems in schools promoting children for the scholarship examination and had already shown enthusiasm for having streams in schools at both the primary and post-primary stages, Simon argues that it was this report from Burt that really promoted the treble-track idea. "Here was an origin of the Board's enthusiasm for three streams in schools...." claims Simon.[135] For, as he points out, this report was reprinted in the 1927 edition of the Board's Handbook for the guidance of teachers.[136]

The Board's interest in streaming is illustrated by its interpretation of the 1926 Hadow report in terms of the ease of classification in schools organised along Hadow lines.[137] After this report, attention was concentrated on providing senior and junior schools large enough to accommodate three or four parallel classes in each year group. So, by the late 1920s, classification of children within schools was official policy.

When in 1931 the Hadow Committee reported on The Primary School they presented Burt's evidence on individual differences and the need for differentiation. According to Simon, Burt's advice corresponded precisely to the administrative pattern already emerging.[138] Whereas the 1926 Hadow report had said very little about streaming and stressed the need for a common core of education up to the age of 13 to enable transfer between different types of school, the 1931 report had a different approach. It reflected Burt's advice on streaming and,as well as presenting his advice in an appendix, it also reproduced his 1925 report to the LCC already reprinted in the Board's 1927 Handbook.[139] According to Simon the report, "...passively accepts psychological advice".[140] The report stated that: "In general we agree with our psychological witnesses in thinking that in very large primary schools there might be, wherever possible, a treble-track system of organisation." [141]

This pattern was subsequently adopted in junior elementary schools.[142]

According to Simon, as streaming became the accepted policy, the old idea of achieving standards went out and the idea of selection by ability took over.[143] Both differentiation through streaming and through selection for secondary school was now conceived of in terms of ability. By 1937 the Board were describing the benefits of Hadow reorganisation in terms of enabling classification within schools and argued that, to get the full benefit from this reorganisation, schools neededto contain relatively large numbers and follow a policy of annual promotion for children.[144] The Board's conception was now explained in terms of different types of children with different abilities following a particular differentiated course of work all the way through their school career.

It seems, then, that while real practical problems existed in elementary schools attempting to get children through the scholarship examination, while the Board were enthusiastic about classification within schools for administrative reasons and while some schools had adopted a form of streaming by the early 1920s, Burt's influence appears decisive in sponsoring the idea of a treble-track system of streams within schools based on 'ability'.

10.3.5 Tripartite Secondary Education

While the Hadow report recommended that six types of school should be known as secondary, the later Spens report recommended that there should be three types of secondary school. In the latter report these were to be administered under a single code of regulations, not simply re-named 'secondary'. The three types of school which Spens recommended were the existing secondary schools re-named 'grammar' schools, new technical secondary schools and modern schools. In the period preceding the publication of the Spens report the Board had taken an interest in the junior technical schools and had encouraged their development. It is suggested, by Simon, that the wording of the remit to the Consultative Committee at this time revealed that the Board intended to develop a different type of secondary school with a leaving age of 16 years.[145] This follows from the fact that the Board were considering raising the leaving age for the existing secondary schools to 17 years, yet the committee's remit specified provision for children not remaining in education after 16 years of age. Furthermore, following Simon's argument, given the Board's enthusiasm for the junior technical schools, it probably had in mind bringing these under the secondary code. In the event, this is part of what the committee recommended.

The other most significant development made by local authorities, and encouraged by the Board, had been the growth of selective and non-selective central or 'modern' schools. The Spens recommendation of three schools therefore follows to some extent the precedent already set in educational organisation. But this does not fully explain why they should recommend three types of school instead of any other number.

In the previous Consultative Committee report on The Primary School Burt had argued, as we have seen, that increasing individual differences in intelligence meant that children of 10 years needed to be spread over at least three different standards, and by the age of 12 they needed to be in different types of school. Following this advice the committee had recommended a triple-track system of streaming in Primary Schools and this had been put into effect in many areas.

This argument from Burt concerning increasing individual differences and the need for differentiation among children of different ability, was reproduced in the Spens report. According to Simon, the Spens argument for separate secondary schools depended directly on psychological advice.[146] However, as we have seen, the idea of different types of post-primary school was well established by then, so, although the report is often couched in phrases borrowed from Burt, this psychological advice can still be seen largely as providing justification, or a rationale, for a policy already favoured. However, while psychological evidence may have provided a justification for an already established policy of having different types of school, the recommendation of three types of school may owe even more to this source of advice.

It was clear that the secondary or grammar schools were to be kept intact, the Board had long pursued this aim. The conversion of the junior technical schools into secondary schools was also apparently favoured by the Board already and the existence of a large number of central or modern schools was already a reality. This does not, however, fully explain the recommendation to provide just three types of school. What the tripartite system of secondary education appears to be is an extension to the system of secondary education of the 'treble-track' system of streaming already existing in junior schools. Furthermore psychologists, particularly Burt, had been influential in establishing a system of streaming based on three tracks. It may be concluded therefore that the recommendation of three types of secondary school, as opposed to any other number, by the Spens Committee, derived from an existing administrative pattern which had been at least partially influenced by psychologists.

10.3.6 The Use of Psychological Tests

It is obvious that the advice of psychologists was crucial in influencing the introduction of mental tests into the education system. However, it is worth emphasising that the case was not simply one of psychologists developing a technique which they then tried to sell to educationists and educational policy makers. In the field of education policy a great need was felt for some sort of objective test, particularly for use at the 11+ stage in the scholarship examination.

Following World War One, the scholarship examination' was under heavy competitive pressure. There was parental and political criticism of the selective procedures for secondary education and secondary provision. Demand for places at secondary schools was much higher than the supply. Most authorities ran a qualifying examination for secondary education so that all pupils who reached a certain standard passed. The problem was that far more children qualified for secondary education than places available for them, and the majority of places were taken up by fee payers who normally had no effective entrance test.[147]

In 1919 the Board set up a departmental committee to review the problem and, as we have seen, this revealed that many children 'capable of profiting' by secondary education were prevented from doing so by the lack of available places.[148] A majority of the committee recommended that all children should be subject to a formal competitive test of capacity and promise rather than attainments. The Board began to concentrate its attention on the development of tests to determine children's capacity at 11+. It was concerned to develop a system of examination through objective and competitive tests based on merit to dampen criticism of the scholarship examination and the lack of adequate secondary provision.[149] The Board ordered the Consultative Committee, in 1920, to report on psychological tests and in 1924 asked the schools inspectorate to consider the scholarship examination in detail. In his 1921 handbook on tests Burt referred to the pressure to produce 'a mental footrule'.[150]

In 1919 and 1920 group tests were first being experimented with in the 11+ examination by Burt and Thomson and 1921 saw the publication of Burt's handbook and Thomson's account of his experiment with group tests. So, the first development and application of group tests in the education field coincided with the need of educational policy-makers for an objective test of capacity. It seems then that the introduction of mental tests into the education system was a two way process of supply push and demand pull. Psychologists were developing a test which they claimed could objectively assess children's mental ability and advised that these would be useful in education just at the time when educational policy-makers were looking for an objective test of ability to fend off criticisms of the secondary education system and pressure on the scholarship examination. Once psychological tests were adopted the excess of children qualifying to enter secondary school disappeared since the test results could be fitted to the number of places available.[151]

This case illustrates the intimate relationship between this particular science and policy. The development of psychological tests fed off the need felt in the educational policy area for an objective test of capacity. Just as in the policy area the notion that such a test would solve the problem of criticism and pressure on the scholarship examination was spawned by the development of psychological tests.

10.3.7 The Notion of 'Types of Mind'

The Hadow and Spens reports had discussed differentiation at the secondary level in terms of providing for the different 'needs and interests' of children. In the Norwood report this discussion was in terms of catering for children with 'different types of mind'.

In the Hadow Report of 1926 reference was given to the 'diversity of gifts' amonst children and the 'different types of character and intelligence'.[152] In this report it was argued that the range of educational opportunity should be sufficiently wide as to cater for children whose "...powers...differ widely, both in kind and in degree".[153] At this stage, then, there was discussion of both the different levels of ability and different types of ability. According to this report psychologists had argued that, by the age of 11 or 12, children have given some indication of differences in interests and abilities sufficient to make it possible and desirable to cater for them by means of schools of varying types. In discussing selection at 11+ the report argued that the need is to discover the types of school most suitable to a child's 'abilities and interests'. The psychological advice referred to in these arguments concerning selection for different types of school is that provided by Burt. In his evidence Burt is very specific in arguing that the most important aspect of individual differences in ability is the level of general intelligence. In the 1931 Hadow Report Burt argued that separate types of school were needed for children by the age of twelve because of increasing individual differences in the level of general intelligence, and provided some of the rationale for streaming in primary schools.[154] The Spens report accepted Burt's argument that general intelligence was the most important factor determining a child's work in the classroom. The Spens committee also accepted Burt's view that, whereas the level of a child's general intelligence could be accurately predicted, specific aptitudes or interests could not.[155] A clear picture of views on the relationship between types of schools and the different levels and types of children's abilities at this time is provided by the proposals which the Spens report makes for methods of selection. The report argued that there should be three types of school of equal status, administered under the same code. It is argued that selection for technical schools should be through the same examination as for grammar schools, but supplemented by an interview with both parents and children. This selection procedure would amount to selecting children for grammar and technical schools as against modern schools on the basis of performance combined with level of ability or general intelligence, and then selecting between technical and grammar schools on the basis of types of interest and ability as determined at an interview with both parents and children.

In all these reports there was discussion of both the level and the type of children's differing abilities. However, the evidence from psychologists strongly emphasised the importance of level of ability and it was accepted that, whereas this could be easily measured, different types of ability could not.

The Norwood report introduced the notion of different types of mind.[156] As we have seen, this committee shunned psychological advice and argued, on the basis of educational experience, that children fell into three groups. It is clear from the Norwood committee's rejection of psychological evidence, from their emphasis on the importance of teachers' judgements, their suggestion that intelligence tests were of only marginal use, and from the powerful criticism which the types of mind idea received from Burt, that this idea did not come from psychologists. The committee itself claimed that it was educational experience which had shown that these three types of mind existed. As we have seen, Burt claimed that this was contrary to the facts as revealed by psychology.[157] The committee considered the school record, compiled by teachers, "...the best single method available of discovering special interests and aptitudes and general level of intelligence".[158] The committee pointed out that psychological tests which differentiated between different types of ability had not been successfully developed yet and this was part of the reason for giving psychological tests a minor role in selection. The committee argue that if this type of test can be successfully developed, then they could be used in combination with the school record card which would be based on the judgement of the teacher.

So, while earlier discussions in the policy arena had referred to both level of ability and different types of ability, the emphasis had been placed firmly on the former. Furthermore psychologists' advice to the various Consultative Committees had focussed on differences in the level of general intelligence. Although by the mid-1940s psychologists were attempting to develop tests of specific aptitudes and there had been considerable debate amongst factorists concerning the relative merits of a view centred on one single general factor, as against a view based on several groups of specific factors, the emphasis in psychological advice given to educationists was still on the importance of levels of general intelligence.

The types of mind notion did not, therefore, come from psychological work or psychologists' advice. The notion, introduced by the Norwood committee, followed, it was claimed, from educational experience. Further investigation would be needed to discover whether the notion of three different types of minds was merely a way of imposing the characteristics of the already developed school structure onto the children themselves, or whether ideas about types of mind and corresponding types of education were in fact more related to ideas concerning the types of employment which a child might expect upon leaving school. For the purposes of this study, it is sufficient to conclude that the notion did not follow from psychological theory or psychologists' advice.

10.3.8 Conclusion

The primary function of psychology and psychologists' advice was to provide a justificatory rationale for policy options already favoured on other grounds. On issues such as the provision of different types of secondary school and transfer to secondary or post-primary education at age 11+ there was a broad consensus already existing which psychological advice merely served to reinforce. On the issue of streaming there was powerful pressure from the Board of Education to adopt this policy and precedents existed in some areas when, again, psychological evidence reinforced these trends. The idea for three streams and three types of secondary schools may have originated with Burt following his ideas on levels of general intelligence. The adoption of psychological tests followed from psychologists' advice and the Board's need for an objective tool to assess ability, while the development of group tests was at least partly in response to this need. The concept of different types of mind, in terms of specific aptitudes, needing different types of education did not follow from psychological advice and was, in fact, contrary to psychologists' views. Finally, one important influence of psychologists in the sphere of education policy was to provide a vocabulary for educational debates. The concepts of 'general intelligence', 'mental capacity' or 'ability' and 'individual differences' became common currency in discussions on educational policy.

10.4 The Eugenics Movement

Before leaving the question of the relationship between educational psychology and education policy, reference must be made to the influence of the Eugenics movement. Two points are worth commenting on here: the influence of the eugenics movement and eugenic thought on the development of psychometrics and the influence of eugenic thought on the acceptance in psychology and in the sphere of education policy of psychometrics.

It is clear that many of the founding fathers of psychometrics were also committed eugenicists. It was Francis Galton, who was committed to a eugenic programme and who established a Chair of Eugenics at UCL, whose ideas on intelligence and heredity inspired mental testers such as Burt. A number of leading psychometricians were members of the Eugenics Education Society, for example Burt, Spearman and Thomson. According to Lowe the eugenics movement was crucial in focussing attention on the need for diagnostic techniques.[159] At the turn of the century there was much concern with the problem of mental deficiency. Official reports were published on the matter and legislation passed concerning educational provision for defectives, and in 1908 a Royal Commission report was published concerning The Care and Control of the Feebleminded.[160] Alongside this concern with the feebleminded arose a concern with the need for diagnosis and classification. It is argued, by Lowe, that two prerequisites for the development of a system of classification through mental testing were provided by eugenicists. These were a general acceptance of the significance of heredity and the refinement of the techniques of correlation. Eugenicists argued the case for the importance of heredity and, according to Lowe, the work of the BAAS, particularly the 1910 committee, on "Mental deficiency from a psychological standpoint" was important in this respect. This committee provided a vehicle through which the hereditarian view of Burt and others was transmitted to other members of the BA. Around this time testing was beginning to get support in the Board of Education through Sir George Newman and Lowe argues that the eugenicists and psychometricians provided an important input into the debate which lead up to the Mental Deficiency Act of 1913. As we have seen it was in 1912 that Burt got his job with the LCC to work on the problem of distinguishing mentally deficient pupils. As Lowe points out Burt's interest in intelligence testing, according to his paper in Eugenics Review in 1912, derived largely from the problem of mental deficiency.[161]

The statistical techniques required for mental testing were also provided by eugenicists such as Karl Pearson, as Lowe points out.[162] It has further been argued by Norton that at least some eugenicist scientists such as Pearson and Fisher pursued particular problems because of their eugenic importance.[163] Others, such as Charles Spearman, Norton argues, were at least keen to draw eugenic implications from their work, even if the work itself was motivated by other interests, in this case a concern with the principles of cognition. It is pointed out, by Norton, that Burt's first paper was strongly influenced by the work of some of Pearson's fellow workers on the relationship between social class and intelligence. Much of Burt's later work had eugenic implications and he continued to publish papers on eugenic issues. It is argued by Norton that the Burtian view of the relationship between IQ and social class originated in the eugenical thought of Galton, that in Galton's formulation it was an ill-guided fantasy and that the development from this earlier view to the later Burtian version was the work of men who shared Galton's political perspective and who worked as they did because of these political beliefs.[164] It is further pointed out by Norton that in the work of Burt, Pearson, McDougall, Fisher and others their strong commitment to eugenics or at least hereditarinism meant that they were, "...not over-scrupulous in their reasonings", pointing out that even the conclusions of Burt's very first paper overstretched the evidence on which they were based.[165] (A judgement shared by Burt's biographer, Hearnshaw).[166]

It is interesting to note in the context of the earlier discussion of the role of psychology in providing a scientific basis for the professionalisation of education that McKenzie has argued that the eugenics movement can be seen as an attempt by the professional middle classes to improve their status (through emphasising the importance of intelligence and other factors such as moral worth and health) and many scientists were involved in this process.[167]

According to Lowe it was eugenicists, notably Burt and George Adami, who convinced the Board of Education of the value of intelligence testing. A eugenicist and member of the Board of Education Consultative Committee, Adami was the chairman of the sub-committee involved in the preparation of the report on Psychological Tests of Educable Capacity.

It is clear that much of the work of psychometricians such as Burt was inspired by eugenic ideas and that a general climate of opinion favourable to eugenic thought would certainly not have acted against the development and application of mental tests and the theories of educational psychologists. However, it has been seen in the present study that educational psychology was influenced as much by policy developments as were policy developments by educational psychology. Furthermore, the adoption of tests was by no means a straightforward and efficient process.

10.5 Summary

In the phase of agenda formation psychologists had virtually no role at all. The problem of the need for educational reform was forced onto the agenda by the competitive pressure on the free place examinations, both before and after the First World War, and by campaigns for more equality in the education system, such as the Labour Party's call for 'Secondary Education for All'. While it may be tempting to argue that psychologists influenced the definition of the problems facing educational policy-makers through providing a tool for selection and through focussing debate on the 'ability' of the child, on closer inspection even this claim is false. The debate was not forced into a concentration on means of selection by the development of tools for this purpose by psychologists. The problem arose in this form before tests were considered for this purpose. It was the inadequacies of the existing free place examinations which gave the debate its focus on means of selection. It is therefore possible to argue that, at this stage, rather than psychology influencing the policy debate by focussing it on to a particular issue, the needs of policy actually influenced the direction of psychological research by demanding a practical tool for selection leading to the development of group tests. Furthermore, while it is true that psychologists were focussing on the 'ability' of the child and their notions of ability did appear in official reports, the explicit espousal of the notion of different types of mind by officials in the face of Burt's insistence that only different levels of ability could be measured, shows that the notion of ability employed by officials was not that of the psychologists. What psychologists did appear to supply was a tool with which to implement the policy of selection by differentiation, though even this influence was not universally felt as the implementation of psychological tests was slow and patchy.

In the previous chapter it was seen that criticism of psychometrics developed within psychology in response to the prospect of its widespread use in practical applications.

It can now be seen that criticism of this scientific development crystallised in both the scientific and political arenas at around the same time in response to developments in policy. Once the policy of selection by differentiation was implemented through the 1944 Education Act and the Board's interpretation of it, the newly developed fields of social psychology and educational sociology were able to study the effects of this policy. Educational sociologists, notably those working in the Department of Sociological and Demographic Research at the London School of Economics and Political Science, in studying the effects of the policy of selection came to the conclusion that measured intelligence was largely an acquired characteristic.[168] They were thus critical of the whole genetic basis for the notion of general intelligence and mental testing. These studies initiated a controversy between educational psychologists involved with mental testing and favouring a genetic view of intelligence and sociologists favouring an environmental view of intelligence.[169]

Finally, with the implementation of the Education Act came political criticism of the policy of selection and a growing movement in favour of comprehensive education.[170] This political criticism of the policy of selection further contributed to the critical attention being paid to psychometrics. Thus it was developments in the policy arena which led to the crystallisation of criticism of both the policy of selection and the science of psychometrics from both political and scientific sources. From a situation before the 1940s of consensus on policy and consensus in science the situation shifted during the late 1940s and 1950s to one of dissensus on policy and dissensus with respect to science.

C H A P T E R 11

ENVIRONMENTAL LEAD:

THE USE OF EVIDENCE

CHAPTER 11. ENVIRONMENTAL LEAD: THE USE OF EVIDENCE

11.1 Introduction

In Part 1 it was argued that one source of difficulty with respect to policy relevant science is that practical problems are posed from outside of the sphere of science. This means that such practical problems do not always fit neatly into the existing boundaries of scientific disciplines and specialties. This observation is implied in the work of Robbins and Johnston[1] and Gillespie _et_._al_.[2] and is explicitly put forward in some sociological analyses of the political direction of scientific research.[3]

Scientific problems are normally posed from within a particular specialty in a form in which the techniques and concepts of that specialty can be brought to bear on the problem. In Popperian terms this means that hypotheses can be constructed which the techniques of the specialty are capable of testing, or in Kuhnian terms the solution to the problem is known or at least anticipated in advance, and again can be investigated using the tools of that specialty. Where such scientific problems do involve a range of specialties a whole host of difficulties arise in solving the problem, as was illustrated by the sociological studies discussed in Chapter 3.

Where problems are posed 'externally' to science they may span a number of specialties and this can mean that different techniques and concepts can be applied to the problem. These different approaches may not be easily compared and evaluated since no criteria exist for comparing one specialty's techniques with those of another. Specialties have their own technical norms and criteria for judging the efficacy of different approaches to a problem within the specialty. So, when a problem spans a range of specialties, experts with different cognitive backgrounds will be operating with different technical norms with no criteria for judging each other's approaches to the problem.

Not only will practical problems frequently transcend the boundaries between scientific specialties and even disciplines, but they will also receive more attention from public bodies such as Government Departments, Research Councils, Charitable Organisations, Trade Organisations, Corporations and Pressure Groups than would an area of science unrelated to policy. This increased attention may result in more funding for research relevant to the problem, so that a vast amount of evidence becomes available.

The present chapter will investigate two hypotheses relating to these characteristics of practical problems. These are: H1 - that scientists involved in policy debates will draw on evidence from some specialties to criticise arguments based on evidence from other, different, specialties; H2 - that scientists involved in policy debates have a vast range of evidence on which to draw and will pick whichever evidence supports their case. It is hoped that the investigation of these hypotheses will throw some further light on the issue of the content and context of debates as put forward in Chapter 2, and on the mechanisms of over-criticism as proposed in Chapter 4.

11.2 Method

To investigate these hypotheses a particular, well defined, conflict from within the debate over the health effects of environmental lead was identified. This was the publication of two reports each reputedly reviewing the evidence concerning lead and health, but each coming to different conclusions and making different recommendations for government action. The first report Lead and Health was produced by the DHSS Working Party on Lead in the Environment under the chairmanship of Professor P.J. Lawther.[4] In direct reply to this the Conservation Society Pollution Working Party produced their own report entitled Lead or Health which criticised the conclusions and recommendations of the Lawther report.[5] Both reports were published in 1980.

These two reports, produced to review the evidence on the same problem, reached completely different conclusions particularly concerning the relative importance to man of different environmental sources of lead pollution and about the health effect of different levels of exposure to lead. These different conclusions result in very different sets of recommendations for government action made in the reports.

To investigate the two hypotheses described above the main arguments of each report were identified and the bodies of evidence on which these were based were compared.

Attention was also paid to the specialties which the reports' authors were trained in.

To test the first hypothesis it was necessary to see if there existed a case where directly conflicting arguments in the two reports relied on evidence from different specialties. That is, a case where report 1 contained argument A1 based on evidence E1 from Specialty S1, and report 2 contained argument A2 based on evidence E2 from specialty S2, where A1 and A2 were directly conflicting arguments. Clearly, the real situation is likely to be more complex than this where E1 and E2 would probably derive from a range of specialties, but the test only requires that some of the specialties which E1 derives from be different to those from which E2 derives. If no such case exists then H1 is refuted. If such a case does exist then H1 is corroborated.

To test the second hypothesis is slightly more difficult than this, since H1 is entailed by H2, thus a case which corroborates H1 would also corroborate H2. What is needed is a particular test that can distinguish between the existence of the effect described in H1 and the existence of the more general effect described in H2. For this what is required is a case where directly conflicting arguments in the two reports relied on different evidence from within the same specialty. That is, where report 1 contained argument A3 based on evidence E3 from specialty S3 and report 2 contained argument A4 based on evidence E4 also from Specialty S3, where A3 and A4 are directly conflicting arguments. Again, if no such case exists then

H2 is refuted.

There were two main parts to the analysis of the evidence from the two reports. First, the key arguments within each report were identified. Then a sub-set of these were chosen for analysis and comparison. All the key arguments chosen for analysis were in the form of argument and counter-argument, that is they were directly conflicting, to enable direct comparison of the evidence relied upon. The bodies of evidence on which these chosen key arguments depended were identified and then tabulated for each report to enable comparison.

The second approach was to count the number of citations within each report to every paper cited in the bibliographies. With these figures tabulated a cut-off point was decided upon whereby all papers with more than a certain number of citations would be designated as 'high-citation' papers. All the high-citation papers were tabulated for each report so that these, too, could be compared. To supplement this, particular authors whose work was cited many times, though whose individual papers were only in the 'low-citation' category, were included in the tables.

These two approaches thus gave a qualitative assessment of the key arguments in the two reports and a comparison of the evidence on which they are based, and a quantitative measure of the importance to each report of particular bodies of evidence. Some reservations must be expressed about the approach of counting the number of citations to each paper since this may represent negative citation or the papers could be review articles or other reports covering a wide area of research and so being cited many times but referring to different aspects of their contents. However, this does not detract from the conclusion that 'high-citation' papers are important to the arguments of a report, even if those citations are critical ones.

11.3 The Reports

The DHSS working party consisted of a group of, "Independent experts actively engaged in the fields of clinical paediatrics, pathology, child psychiatry, psychology, epidemiology and the environmental sciences." [6] Their remit was to review the overall effects on health of environmental lead from all sources, with particular reference to effects on children and the contribution of petrol lead.

Broadly, the issues with which the Lawther report deals fall into two main categories. The first concerns various environmental sources of lead and their relative importance to the uptake of lead by humans. The second deals with the neuropsychological effects of lead on children. After reviewing evidence concerning these two areas the report draws conclusions and makes recommendations for action.

In their conclusions the Lawther working party emphasise the complexity of the issues, the many uncertainties involved and the inconclusive nature of the evidence, particularly that concerning adverse effects of low-level lead exposure. They conclude that the major part of lead body burden for people in the UK is derived from food and that the usual maximum of lead in food in the UK is well below the WHO guidelines for permissible levels. The next largest source of lead is tap water according to the report, and in the majority of the population this source makes no appreciable contribution to the total intake of lead. Concerning the uptake of lead from air, the report argues that for most people in the UK it is much lower than that for food and water. They maintain that for most people about ten per cent of the lead body burden comes from air, and about ninety per cent of airborne lead is derived from petrol. For both waterand air the report emphasises the importance of 'hot spots' where lead levels are high enough to make a more significant contribution to blood lead.

The report pays quite a lot of attention to what it calls 'adventitious sources' of lead, to which only particular sections of the community are exposed. These are important as possibly the only cause of overt clinical lead poisoning in the non-industrially exposed population in the UK, according to the report, and the importance of personal and social factors in the risk of exposure to these sources of lead is emphasised. According to the report the most important risk from adventitious sources is that associated with the presence of old lead-based paint and primers, where children have access, particularly when associated with the problem of Pica.

The report's emphasis on uncertainties is directed particularly toward the assessment of the effects of lead on children. The conclusions concerning this issue emphasise the need for careful studies while criticising much of the work done by that stage. The report identifies some major difficulties in this work such as; distinguishing between the effects of lead and of other factors, measuring the total lead intake by a child over a long period, difficulties in the measurement of intelligence and behaviour, and the inadequacy of certain types of study. However, they do make some firm conclusions, for example, that there is no convincing evidence of deleterious effects at blood level concentrations below about 35 μg/dl and that symptoms of lead poisoning and encephalopathy occur with levels in excess of 80 μg/dl.[7] The range between these two figures is identified as one of uncertainty. To re-emphasise the uncertainty on this issue the report notes that different studies give conflicting results and that there appears to be no dose-response relationship between the concentration of blood or tooth lead and the degree of reported intellectual impairment. Finally they recommend that research is needed into pre-natal exposure to lead.

The report's final conclusions are that food is the major source contributing to the lead body burden of the population, that there is no evidence that this is substantially enhanced by contamination by airborne lead, and that no conclusions can be reached concerning the effects of small amounts of lead on intelligence, behaviour and performance of children. The report calls for more work on this latter issue.

Following these conclusions the report makes a number of recommendations for action to be taken by the government. These form two sets, one aimed at dealing with the 'hot spots' problem where individual exposure to lead is high, the other aimed at reducing the cumulative effect from a multiplicity of sources. The first set contains recommendations for action to reduce risk from lead in paint generally and especially in high risk areas, to reduce risk from lead in tapwater in affected areas and to discourage the import and use of lead-containing cosmetics. The second set contains recommendations to keep permitted levels in foodstuffs as low as possible, to progressively reduce the contribution of lead to air from traffic and other sources, and to keep annual mean concentration of lead in air to less than 2 $\mu g/m^3$ in places where people are likely to be continually exposed for long periods.

Various other recommendations are made concerning other specific sources of lead but these two sets are clearly the most important as the report itself implies.[8] Finally the report makes some recommendations concerning further research and monitoring of the problem, including the suggestion of a central unit to co-ordinate environmental monitoring and clinical investigation concerned with lead.

It is worth commenting on these recommendations that they are couched in terms of 'progressive reduction', 'steps to discourage', 'encouragement' and 'measures' and this may be contrasted with the recommendations contained in the Conservation Society report. It is also worth noting that much emphasis is placed on the specific problem of 'hot spots' and the question of airborne lead is seen only as a component of a general problem of the cumulative effect of a range of sources. Thus the report mentions that the value of further reduction of lead in food and air lies in the need to reduce this cumulative effect.

The Conservation Society Report was produced by two scientists qualified and practising in Chemistry. The aim of the report was to comment on the Lawther Report and to update an earlier 'Memorandum' produced by the Society on 'The Health Effects of Lead on Children'.[9] This report therefore presents direct criticisms of the Lawther report as well as reviewing evidence, presenting conclusions and making recommendations for action. Criticisms are aimed at the Lawther conclusions concerning both the relative importance of different sources of lead and the effects of lead on the health of children. The Lawther conclusion that food is the most important source of lead with water and air following, in that order, is claimed by the Conservation Society report to be, "The most prominent and disastrous error."[10] This conclusion is criticised on three grounds; that it is based on a mathematical error leading to an underestimate of the contribution from direct inhalation of airborne lead, that it ignores evidence for a massive indirect contribution from air through contamination of food, dust and children's fingers, that it overestimates the importance of water as a source of lead.

Concerning the health effects of lead on children the

Conservation Society report accuses Lawther of being, 'unconstructively hypercritical', with respect to the evidence, and of being selective in this critical approach.[11] The figure of 35 µg/dl is also criticised as having no real scientific or medical basis and as implying that a threshold exists when this is not the case, as the Lawther report admits.

It is important to note that one of the criticisms of the Lawther report presented by the Conservation Society is that it ignores large bodies of evidence. For example, it is claimed that the Lawther report ignores the evidence concerning the indirect contribution of airborne lead to body lead through contamination of food and dust, ignores evidence from biochemical and animal studies relevant to the effects of lead on children, and pays scant attention to evidence concerning pre-natal effects.

It is also worth noting that the Lawther report explicitly states in its introduction that it has not reported on evidence concerning clinical lead poisoning or on experimental work in animals and biochemical mechanisms.[12] The justification given for ignoring this work is that it throws light on the ways in which small amounts of lead might have an effect, but it does not determine whether they have effects of medical or social significance. Clearly, the two reports place different importance on these bodies of evidence.

In line with its criticisms of the Lawther report the Conservation Society comes to very different conclusions concerning the main issues. They argue that 90% of airborne lead comes from petrol lead additives, as does the Lawther report, but they also argue that airborne lead, through direct inhalation and through indirect intake by ingestion of food and dust contaminated by fall-out, provides a major source of body lead burden among the general population, and the main source for many people especially urban children. They argue that, in certain locations and situations, other sources can be important and they provide a list of sections of the population at greatest risk, putting special emphasis on the risk to pregnant women. The concept of thresholds of toxic effects is deemed probably untenable and the Conservation Society report's figure for the 'no-observed-adverse-effects levels' of lead in children is 5μg/g in dentine and 5 μg/dl in blood, the latter being exceeded by most UK children according to the report. The report concludes that neurotoxic effects in children can be produced by post-natal exposure and pre-natally by parental exposure, and makes further conclusions concerning the levels at which blood-forming processes and enzyme formation are affected by lead. Concerning clinical lead poisoning the report claims that the risk increases as body lead burden increases and that it occurs in children at blood lead levels above about 40 μg/dl, though with great variations in individual susceptibility. The risk to the unborn child is claimed to be probably greater than that to young children. The report also concludes that the Lawther report is 'deeply flawed and fragmentary'.[13] Finally, the report's general conclusion is that most UK children are now suffering 'an epidemic, even a pandemic,' of low-grade lead intoxication and this is blamed mostly, if not completely, on the addition of lead to petrol.[14]

The Conservation Society report's recommendations for action are to make the addition of lead to petrol illegal within a year, using economic measures to discourage its use in the interim period, and to prohibit; the use of lead as a constituent of any container for the preparation, storage, transportation or consumption of food or drink; its use in printing ink, in lubricants and the import, manufacture and sale of lead-containing cosmetics and 'medicines'. Further recommendations include; reducing the statutory limits for lead in food and extending limits on lead in paint, implementing and extending new blood lead level limits for industrial workers with lower limits for women by 1984 and reducing the maximum permitted air-borne lead level in industry from 150 $\mu g/m^3$ to 10 $\mu g/m^3$ by 1984.

It is clear that the tone of these recommendations is very different to that found in the Lawther report. Here there is an emphasis on the use of legislation with time limits and a number of specific levels of lead exposure explicitly included. Furthermore, in keeping with the different conclusions of the two reports, lead in petrol is the centre of attention in the Conservation Society report's recommendations and here it is to be eliminated by legislation in contrast to the 'progressive reduction' advocated in the Lawther report.

11.4 Results and Discussion

There are two groups of results to be discussed, the first consisting of a comparison of the use of evidence in the two reports to support a number of key arguments, the second being an analysis of some quantitative findings concerning the bodies of evidence on which the reports rely. The key arguments to be discussed concern the relative contribution of different sources of lead to the body lead burden of the population, the relation between lead exposure and hyperactivity, and the question of low level effects. Each set of results from the analysis of these key arguments illustrates a range of phenomena and, as for the two hypotheses under test, the evidence provides clear support for H1 but does not provide such clear evidence for H2.

11.4.1 Key Arguments

(a) Relative Contribution of Different Sources of Lead

One of the main conclusions in the Lawther report is that food is the most significant source of lead for humans and that water and air come next, in that order.[15] To counter this argument the Conservation Society report points out that it entails the implicit assumption that lead in air has a negligible impact on lead in food and that the contamination of dust with lead and its consequent ingestion by children is also insignificant.[16] The Conservation Society report criticises this assumption arguing that the fallout of lead from air significantly increases the concentration of lead in food and that dust ingestion by children is a significant source of lead. Here, then, are two directly conflicting arguments, one saying that the relative importance of different sources of lead is food > water > air, the contribution of lead from air to body lead burden of the general population being about 10%, and the other saying that the contribution from air is far more important than this and in some people is the major source of body lead burden. What evidence do these two arguments rely upon?

The Lawther report argument relies upon two large bodies of evidence, one consisting of surveys of lead content in various media, for example air, water and food surveys, the other consisting of studies relating to lead absorption, including isotope studies, balance studies and epidemiological studies.[17]

The Conservation Study argument, on the other hand, relies on firstly a small group of studies concerning the deposition of lead from air and a large group of studies concerning lead in dust.[18] In these two particular arguments there is no overlap in evidence at all. Each argument relies on a completely different set of evidence which is also from a different specialty. This appears to be a case which supports H1.

(b) <u>Hyperactivity</u>

One of the possible behavioural effects of lead which both reports discuss is hyperactivity. On this subject the Lawther report concludes that there is no satisfactory evidence to demonstrate an association between high body lead levels and hyperactivity in children.[19] They argue that the studies which address this problem are of a poor quality. The evidence which they cite to support this conclusion is a fairly small group of studies concerning hyperactivity and blood or dentine levels in children.

The Conservation Society report criticises this rejection of the evidence concerning hyperactivity and suggests that there is a cause effect relationship between lead and hyperactivity, though they also argue that in general the effect of lead on children seems better related to hypoinhibition rather than hyperactivity.[20] The evidence which the Conservation Society relies on for these conclusions is in two parts. The same body of evidence as used in the Lawther report is referred to and in addition a very large body of literature consisting of animal studies.

This case therefore illustrates two effects. It provides support for H1 in that evidence from different specialties is used in conflicting arguments. It further provides an illustration of the use of the same body of evidence to draw different conclusions. Furthermore, this is not so much an illustration of different interpretations of the same evidence as much as different assessments of the quality of the same evidence.

(c) Low-level effects

Concerning the neuropsychological effects of lead on children, the Lawther Report reviews a fairly large body of literature containing human studies. Concerning intellectual function they review evidence under the following headings:- 'Clinic-type studies of children with high lead levels'; 'Studies of mentally retarded children', 'Smelter studies', and 'General population studies of dental lead'.[21] Concerning all of these they emphasise the uncertainties, contradictory results and inadequate methodologies. In discussing the mental development of children they take a sub-set of these studies for comparison of the results concerning the effects of lead on IQ. This sub-set seems to represent a core of the literature on neuropsychological effects which the report focusses on. In discussing this sub-set of the literature in the context of effects on intelligence, the report again emphasises the limitations of methodology and the uncertainty of the results. Later, in the general conclusions the report claims that there is no convincing evidence of deleterious effectsat blood lead concentrations below about 35 μg/dl. This conclusion must be based on the evidence contained in the chapter on neuropsychological effects and the group of studies picked out in the discussion of IQ represents the core of these.

The Conservation Society report counters the claims of no evidence to show effects below 35 μg/dl with the argument that there is evidence of effects at low level exposures down to 5 μg/dl and they further argue that there is no 'no effect' threshold.[22] Of these two arguments the first directly conflicts with the Lawther argument and the second provides support for the first although it does not necessarily conflict directly with the Lawther argument. These arguments are supported by a very small group of studies of different types. The first argument relies on four papers, two of which are animal studies, the third an <u>in vitro</u> study, and the fourth a study of children. The second argument relies on the same two animal studies and the same study of children plus two studies of dentine lead in children.

This case is, perhaps, the most interesting and the most complex. First, it illustrates the use of the same evidence with a different interpretation, since all the studies of children used by the Conservation Society to support their case are also used by Lawther. Again the two reports seem to differ both in their interpretation of the studies' results and also on their quality and, thus, on how much weight to give to their results. Second, it illustrates the use of evidence from different specialties in that, out of the six different references used by the Conservation Society to support these arguments, three of them are either animal or <u>in vitro</u> studies, and the animal studies are used to support both of the arguments mentioned, whereas the Lawther report relies solely on human studies. Finally, the Lawther report reviews a large number of human studies whereas the Conservation Society relies on only a small sub-set of these. The Conservation Society report acknowledges the existence of further studies but only cites reviews of them, placing the main emphasis on just three papers of this type. It should also be noted that these three studies are also considered important by the Lawther report as evidenced by their high-citation in that report.

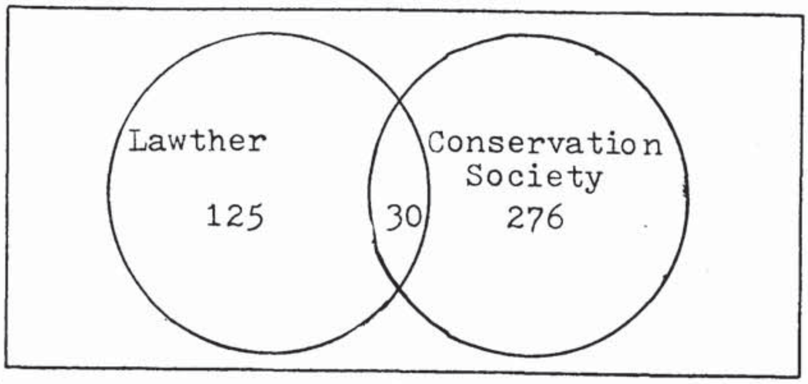
This case comes closest to supporting H2. What is not present is a straightforward case of the use of different sets of evidence from within the same specialty. But, what is present is the use of evidence from the same specialty although in one report coverage is of a large body of literature emphasising its inconsistency and inadequacies while in the other concentration is placed on just a very small sub-set of that same literature which is then interpreted as supporting a particular argument. This does suggest a certain selectivity in using evidence from the same specialty.

This case does provide further clear support of H1.

11.4.2 Citation Count

Two sets of figures concerning the bodies of evidence cited in the two reports are particularly interesting. First are the figures for the total citation in each report and the overlap of papers cited. The Conservation Society report covered a much larger body of evidence. This must be at least partially due to the inclusion of animal studies and work concerning biochemical mechanisms. The most interesting phenomena to note, however, is the relatively small amount of overlap between the bodies of evidence cited by each report. Out of 276 papers cited in the Conservation Society report and 125 cited in the Lawther report, only 30 are cited in both.

Fig.4. Total Citation of Individual Papers



This means that, out of a total of 371 different papers cited, only 30 of these appear in both reports. This finding certainly suggests that the two reports rely on different evidence, whether it be from the same or different specialties. However, it may be that the 30 papers cited in both reports are in fact the key evidence on which both reports rely, the rest of the evidence being rather peripheral. So it is interesting to examine the evidence which receives high-citation in each report, since it is certainly true that in both reports a large majority of papers are only cited once. After tabulating the number of citations given to each paper (see Table 2), it was decided that papers with four or more citations would be categorised as 'high-citation' papers. This gave a core of 19 papers in the Conservation Society report and 15 in the Lawther report. These two sets of high-citation papers were then compared.

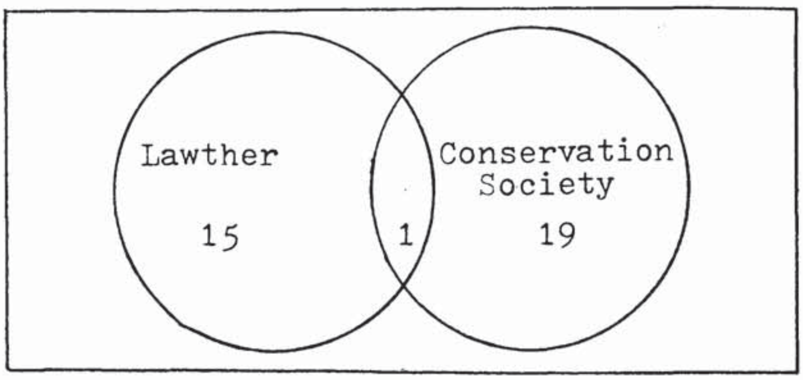
Table 2

Citation of Individual Papers

No. of Citations	No. of Papers: Conservation Society	No. of Papers: Lawther
1	179	88
2	57	11
3	21	11
4 (High-Citation)	7	5
5 (High-Citation)	5	5
6 (High-Citation)	3	3
7 (High-Citation)	1	-
8 (High-Citation)	-	-
9 (High-Citation)	2	-
10 (High-Citation)	1	-
>10 (High-Citation)	-	2

In comparing the two sets of high-citation papers it was found that only one paper fell into the high-citation category for both reports. Of the 19 high-citation papers in the Conservation Society report only 3 were cited at all in the Lawther report, and of the 15 high-citation papers in the Lawther report 7 were cited in the Conservation Society report.

Fig.5. High-Citation Papers



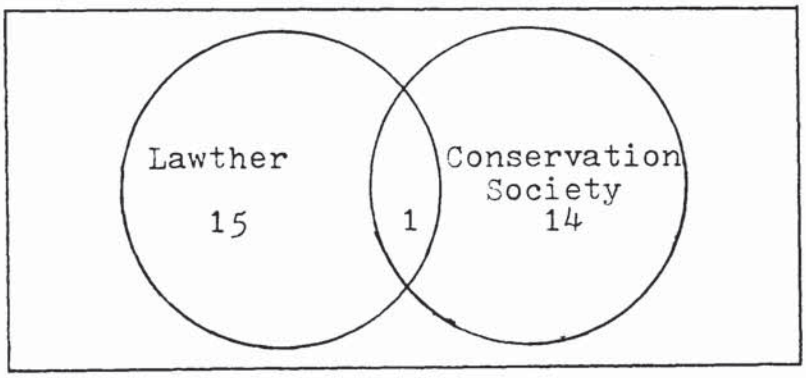
It appears from these figures that, not only did both reports refer to large bodies of evidence not referred to in the other report, but also the core of papers on which each report most relied were quite different. Of course, some reservations can be expressed about the implications drawn from high-citation of a paper. But these reservations do not detract from the assumption that a paper highly cited in a report is important to that report's conclusions, whether the citation is positive or negative. A further problem to investigate is that the papers which the Conservation Society report most relies on could have been produced after the publication of the Lawther report and this may account for their absence in that earlier report. On closer examination this possibility falls down since 12 of the Conservation Society's 19 high-citation papers were referenced in the bibliography produced by the DHSS as a pool of references for the Lawther committee to draw on.[23]

Of the remaining 7 references 3 have 1979 publication dates and it is therefore possible that they were published too late for inclusion in the Lawther report. One is an American report published in 1980 after the Lawther report, and one is a reference to a private communication which would not be expected to appear in another report. Finally one of the seven references which does not appear in the DHSS bibliography is cited in the Lawther report leaving only one reference among the seven published early enough to be included in the Lawther report, though cited neither there nor in the DHSS bibliography, this was a reference to the Conservation Society's memorandum of 1978.[24]

Therefore, out of the 19 high-citation papers from the Conservation Society report 14 were available when the Lawther report was prepared yet, of those 14 available, only 3 were cited in the Lawther report and only 1 of these was highly-cited.

Of the papers highly-cited in the Lawther report there is no reason why they should not be available to the authors of the Conservation Society report.

Fig.6 High-Citation Papers with adjustment for those not available to Lawther.



11.5 Summary and Conclusion

This investigation confirms the existence of the phenonemon of cross-specialty criticism. It also illustrates the different emphases which can be put on the same body of evidence by rival groups of experts. Again the rival experts have both different cognitive backgrounds and different institutional affiliations. The different tones of the two reports clearly reflect the different interests of the bodies sponsoring them, the DHSS and the Conservation Society. Finally, it illustrates that two reports dealing with the same practical problem but prepared from different perspectives have available to them such a large range of evidence that they can rely on almost completely different sets of evidence. This does suggest that, whether through the influence of different cognitive backgrounds, or through the availability of masses of evidence relating to practical problems, experts can, (not necessarily consciously), be selective in their choice of evidence in such a way as to present the strongest arguments in favour of their case.

P A R T   3

CHAPTER 12

CONCLUSIONS

CHAPTER 12. CONCLUSIONS

This chapter will be concerned with drawing conclusions with respect to the various theoretical issues raised in Part 1, in the light of the empirical case study material. There are three main aspects to the theoretical propositions developed in this study and they will be assessed in the following order. First, claims made concerning the content and context of disputes amongst experts and Collingridge's model of expert advice, as presented in Chapter 2, will be assessed in the light of the three empirical case studies. Next, the discussion will turn to the models of criticism, developed in Chapters 3 and 4, and their implications for the inter-action between policy and science. Finally, predictions made in Chapter 5 concerning the role which science plays in the policy process will be assessed in the light of both the empirical material and the theoretical developments concerning the role of criticism in science and policy.

12.1 The Content and Context of Debates

The relationship between the content of a debate amongst scientific experts which is part of a controversy over public policy and its context is clearly illustrated in the smoking and health case. Furthermore, the debate provides confirmation of Collingridge's Model 2, as outlined in Chapter 2, and conflicts with the positivist model or Model 1.[1] According to Model 1 if experts disagree over the policy implications of their work it must be because they hold conflicting values. Conflict on this model is therefore attributed to bias. Model 2 employs the distinction between the public debate and the motivations driving it, or as referred to earlier the content of the debate and its context. This model predicts that the content of debates amongst experts will primarily consist of arguments concerning the merits of different interpretations of scientific data. Normally values and motivations will not explicitly appear in this debate. These motivations are usually not publicly acknowledged. The values which need to be combined with the rival interpretations of data to produce conflicting policy options are relatively innocuous ones which would not be publicly challenged by either side. These values are implicit in the content of the debate. Underlying the different motivations of the rival participants in the debate there may well be different sets of conflicting values, but these never appear in the content of the debate.

The dispute amongst rival experts in the smoking case clearly centres on rival interpretations of data. The key issue which dominates the debate is the explanation of an association between smoking and reported lung cancer deaths as well as other diseases. Both sides agree that this association exists, but different theories are proposed to account for it. The orthodox view of the medical profession is that this association is best interpreted in causal terms. The view of the 'dissenters' is that the association does not represent a causal relation but is best interpreted in terms of a common constitutional or genetic predisposition. These rival interpretations, when combined with the relatively innocuous value judgement that, "If smoking is a cause of lung cancer and other diseases then it should be reduced", produce very different policy implications. A cancer epidemiologist, or member of the Royal College of Physicians, would argue that smoking causes lung cancer and other diseases so, given the above value judgement, smoking should be reduced. A statistician and geneticist with links to the tobacco industry would argue that smoking does not cause lung cancer and other diseases so, given the same value judgement, smoking should not be reduced. These are in fact the policy implications which the rival groups of experts do draw from the evidence in the smoking debate. The rival interpretations of data imply rival policy options. Since the content of the debate concerns rival interpretations of data this case confirms Model 2 and conflicts with Model 1.

Underlying the factual nature of the content of the debate are the rival motivations of the participants stemming from its context. On one side there were epidemiologists such as Sir Richard Doll who was a Fellow of the Royal College of Physicians, and Director of the Imperial Cancer Research Fund's epidemiological unit at Oxford University, and Richard Peto a member of the same unit. Their cognitive background in cancer epidemiology and their institutional affiliation to a research unit and a funding organisation devoted to stamping out cancer, combined with the position of Doll and other medics in the debate as physicians, must have provided a strong motivation to favour a causal interpretation of the evidence and the policy option that smoking should be reduced. On the other hand Sir Ronald Fisher was a geneticist and statistician with an interest in scientific inference and was scientific consultant to the TMSC. Similarly Eysenck was a psychologist with a particular interest in genetic explanations in psychology and his work was funded by the TMSC/TRC. A cognitive background in genetics and an institutional affiliation to the tobacco industry May have provided strong motivation to favour a genetic interpreation of the evidence and the policy option that smoking should not be reduced.

The context of the smoking debate provides further confirmation of Collingridge's Model 2 and also illustrates the role of cognitive and occupational differentiation in controversies as proposed by Robbins and Johnston.[2] Following Collingridge's model, the experts in this debate can be seen as advocates for particular policy options using technical arguments to defend their positions. Finally, with rival groups of highly motivated experts supporting different policy options through different interpretations of data, a situation of over-criticism is likely to occur where the technical debate becomes endless. The smoking debate illustrates this problem in that thirty years after the first findings indicating a link between smoking and lung cancer, the 'dissenters' are still arguing that the debate is unresolved. They still continue to present evidence in support of the genetic theory and have not given up their position on any of the main issues on which the debate has centred.

The disagreement over the health implications of lead in the environment also confirms Collingridge's Model 2 in that the content of the dispute amongst experts centres on the interpretation of data. Again rival interpretations of the data lead the rival groups of experts to draw different policy implications. A specific aspect of the controversy over the health effects of lead in the environment was used by Robbins and Johnston to develop their 'professional differences' approach to the interpretation of controversies.[3] The specific part of the debate on which the present study has concentrated, a different one from that which Robbins and Johnston investigated, appears to go some way in confirming their analysis. The phenomena of cognitive and occupational differentiation are both clearly observed. However, the link between these phenomena and the particular positions taken by the rival expert groups is a matter needing further investigation. Experts with primarily medical cognitive backgrounds sitting on a committee established to advise the DHSS interpreted the evidence which they reviewed as showing that lead in air was the least significant source of lead with respect to human intake. Experts trained in chemistry and affiliated with the Conservation Society interpreted the evidence which they reviewed as showing quite the opposite; that air lead was the most significant factor with respect to human intake. From these rival interpretations the policy recommendations made by the two groups were conflicting. While cognitive and occupational differentiation are clearly present in this case, the link between the experts' different backgrounds and the positions which they took in the debate, needs to be investigated.

In the later phase of the education issue, when conflicting views were expressed with respect to the validity of the assumption underlying mental testing, the debate similarly involved experts from different specialties with rival theories, concerning the nature of intelligence. For example, educational psychologists, particularly those based at University College, London, argued that intelligence was primarily determined by a person's genetic makeup. Social psychologists and sociologists, based particularly at the London School of Economics, argued that intelligence was primarily determined by environmental factors. From these rival theories different policy implications were drawn. The genetic theorists supported selective education whereas the environmental theorists supported comprehensive education. In this case, then, the theoretical orientation of different cognitive backgrounds led experts from different specialties to argue in favour of different policy options.

Finally, while still discussing the influence which policy implications have on the science involved in practical problems, it is worth mentioning aspects of the case study material which appear to show how the political context can directly influence the progress of the technical debate and the cognitive content of the science. In the education issue aspects of the cognitive content of educational psychology were shaped by the requirements of education policy. Specifically, the development of group tests of intelligence seems to have come largely in response to calls for an objective test to be used in selection for secondary education. This need arose because of the inadequacies of the existing free place examinations. Educational psychology was itself an applied specialty with many of its practitioners working in practical educational contexts, so that it would be particularly responsive to the imposition of goals from outside the specialty. Another influence from the political sphere occurred when the widespread application of tests to various fields, including education, either already underway or envisioned in the 1940s, prompted those psychologists with reservations about testing to make their criticisms heard publicly. This laid the seeds for a controversy over the assumptions and value of mental testing which came to fruition when, following the implementation of the selective education policy, studies into the impacts of the application of tests in educational selection were highly critical of mental testing. It may be argued therefore that, without the application of mental tests in the selective education policy implemented after the 1944 Education Act, the controversy over mental testing and the hereditability of intelligence may not have emerged in Britain at this time. Experimental psychologists may not have been motivated to criticise tests while they remained a tool of research and sociologists would not have been able to focus their criticism of selective education policy on the inadequacies of mental testing.

In the smoking and health case it is interesting to speculate that, if the tobacco industry had not decided to shelve the genetic argument against the medical profession's attack on smoking, the technical debate which has continued anyway may well have been more widespread and more vociferous. In this context a detailed comparison of the debate in Britain with that in the USA would make an informative topic for future research. As the technical argument has been pursued for a much longer time in the USA and after a lull has recently re-emerged, comparison with the British case may reveal more about the uses of science as a political tool and the effect which this can have on the development of science itself..

12.2 Criticism in Science and Policy-Making.

Confining ourselves to the decision-making phase of the policy process, the education case provides a clear illustration of the under-critical model. The psychometric school was a dominant feature in British psychology playing a crucial role in the institutionalisation and professionalisation of psychology and, particularly, of educational psychology. This school had no serious theoretical rivals in Britain during the period preceding the Second World War. Psychometrics and the concomitant theories about the nature of intelligence were not seriously challenged. In the absence of a rival school supporting a rival theory, psychometrics was under-criticsed. Furthermore, during this period there was a consensus in the policy arena on the major issue of differentiation.

No political groups seriously challenged the notion of providing different types of education for children of different 'ability'. The psychological evidence from psychometricians supported this policy consensus. Given this situation, there was no rival political group with the motivation to seek out dissenting scientific advice. Thus, with a consensus in the policy arena and a consensus in the scientific arena, psychological advice was not seriously criticised either at a political level or at a scientific level. The reliability of this advice was not adequately assessed therefore, while later developments have shown that it was in fact of particularly dubious reliability.

Following the implementation of this policy, the situation shifted from one of under-criticism to one of over-criticism. As the policy implications which the psychological advice supported began to have an impact, criticism of this advice emerged both in the scientific arena and in the political arena. Experimental psychologists at Cambridge, worried about the widespread application of testing, began to criticise over-emphasis on statistics and lack of experimentation in psychometrics. Social psychologists and sociologists based at the LSE studying the impacts of the policy of selection by differentiation were critical of this policy and of psychometrics which was seen as the theoretical rationale for the policy. Political groups supporting the rival policy option of comprehensive education without selection focussed their criticism on the foundations of psychometrics. In the late 1950s the issue of selection became party political.

By this stage, then, there was dissensus over policy and dissensus with respect to science. The debate over mental testing and the heritability of intelligence which emerged in Britain at this stage has been a long running one which still continues. Furthermore, the policy debate between supporters of comprehensive education and supporters of selection also continues to emerge in the political arena. This debate illustrates a number of the features of over-criticism described earlier. Groups of rival experts are involved with different cognitive and institutional backgrounds. Their rival technical theories support rival policy options. The technical debate appears endless and feeds into an equally endless policy debate. Psychometricians particularly focussed around University College, London, argue in favour of mental testing and the heritability of intelligence also supporting a selective system of education. Sociologists and social psychologists based at the London School of Economics argue in favour of intelligence being environmentally determined and support the policy of comprehensive education.

In the agenda formation phase the smoking and health debate fits the over-critical model. There was dissensus in the scientific arena with rival experts producing competing theories. Criticism was constructive in that it was based on rival theories, but unresolved since the debate has continued to the present day, with both sides claiming some evidence in support of their theory. As was seen in Chapter 4, deciding when an unresolved scientific debate can be described as over-critical is problematic. This is because there are no rules governing the length of debates and any unresolved debate may simply be running its natural course and will resolve in the near future. There are two characteristics of the debate amongst experts over the health effects of smoking which seem to point to its over-critical nature. First, none of the key issues on which it has centred have been interpreted in the same way by both sides; no key issue has resolved in this sense. Furthermore, as soon as any evidence is produced which seems decisive it is challenged. Second, thirty years may not be a long time for a scientific debate which is fairly peripheral to a particular scientific field or which has no relevance to policy. In this case, when policy decisions need scientific answers, this is a long time for a debate to go unresolved.

Also, at the agenda stage there was dissensus on policy with the rival policy options, to act with respect to smoking or not to act, being supported by the rival scientific theories. So political groups with interests in the outcome of the policy debate supported and publicised the scientific theory which reinforced their chosen policy option. With the tobacco industry highly motivated to keep smoking off the political agenda, and the medical profession motivated to get it onto the agenda, the rival genetic and causal theories were vigorously defended in the political arena.

The decision-making phase of the smoking case demands a modification to the over-critical model. The model suggests that if there is a policy dissensus then there will be rival political groups with the motivation to seek out, support, and publicise rival scientific viewpoints to support their case. It is implied that these rival technical arguments will be pursued vigorously in the policy arena unless there is a policy consensus, in which case the motivation to publicise dissenting scientific views will not be present. However, the smoking case illustrates that the existence of policy dissensus does not by itself ensure that the dissenting scientific views are publicised in the policy arena. Other factors act like filters to exclude dissenting views. First, one scientific view can gain acceptance because it is presented by experts who are taken to be legitimate, or who have formal channels of influence to government agencies. Second, the usefulness of a technical argument as a political tool may vary during a debate. When the technical argument becomes either weaker than other arguments, or if it is unnecessary then it may not be vigorously pursued in the political arena.

The decision-making phase of the smoking case still represents a case of over-criticism since the policy implications of the rival scientific theories and the continuing interest in these theories by political groups have kept the technical debate going from its beginnings in 1950 right up to the present day. This prolonged technical debate has not fed back into a confused policy debate in quite the manner expected because the industry is able to defend its interests adequately and the government are reluctant to act against smoking anyway because of economic considerations.

The modification suggested is as follows:-

Fig.7. Over-Critical Model

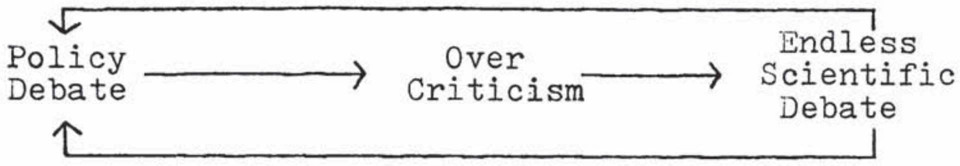
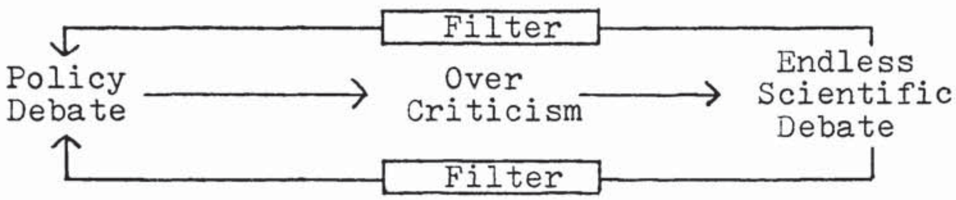


Fig.8 Modified Over-Critical Model



Filter = Formal advisory channels
Value of advice as political tool.

Finally, the policy debate in the decision-making phase of the smoking controversy has still had a technical character to it since it has centred on the new issue of the effects of specific policy options.

The disagreement over the health effects of lead in the environment illustrates some of the specific mechanisms of over-criticism. The lack of shared tacit knowledge inherent in assessments of evidence made from the perspective of different specialties is illustrated by the completely different assessments of the value of animal studies which the groups producing the two reports made. This is further illustrated by differences in the assessment of the quality of particular studies and bodies of evidence. Of particular interest in this case is the availability of such large quantities of evidence pertaining to a practical problem that two reports on the same topic can rely on almost completely different bodies of evidence which are each, in themselves, fairly large. Since practical problems are often complex and broad, so that they span a number of scientific specialties, and they receive a lot of attention from public bodies and political groups which produce large quantities of funds for research, enormous bodies of evidence relating to these problems are to be expected. This mass of evidence will, as the example of the lead controversy shows, be a rich source for supporting conflicting scientific theories and arguments which imply different policy options.

The study of the lead controversy shows particularly clearly the use of bodies of evidence from one specialty to criticise conclusions drawn from evidence from another specialty. Since practical problems almost invariably transcend the boundaries of scientific specialties, this is a feature which is likely to be common in controversies over practical problems. The study by Pinch, discussed in Chapter 3, shows that cross-specialty problems do occur in esoteric areas of science.[4] Notwithstanding this example, it might be expected that in esoteric areas of science, where the goals of work on a problem are defined 'internally' and the function of the problem solution is merely to contribute new results for the advancement of the field, problems would be taken on which can be solved within a single specialty. While cross-specialty criticism might be expected to be the exception in esoteric areas of science when science becomes involved with practical problems it is likely to be the rule. The lack of shared tacit knowledge which characterises cross-specialty criticism will make the agreements necessary to achieve a resolution of conflict extremely difficult to achieve. The lead controversy example confirms these expectations as do the cases of controversy over smoking and health and education policy. With an enormous range of evidence to choose from, combined with the problems associated with cross-specialty criticism, the controversy over the health effects of lead in the environment exhibits some of the main characteristics of the over-critical model. A topic for further investigation would be the question of the way this over-critical technical debate feeds back into the policy debate to see if this process fits the predictions of the over-critical model. These predictions are that rival political groups supporting different policy options will use the rival technical arguments to support their case. As the technical arguments are vigorously pursued in the policy arena, the endless technical debate far from helping to resolve the policy issue will tend to produce an endless policy debate.

12.3 The Role of Science in the Policy Process

The case studies clearly confirm that the role which science plays in the policy process is different at different stages of that process. In the smoking case it was scientific research which raised the problem of a link between smoking and lung cancer. In this way science played an initiating role in the way that Weingart has described. But merely raising the problem will not ensure that a decision is made on it. For this the problem must reach issue status, that is, it must get onto the political agenda. Here medical experts producing more and more research into the smoking/lung cancer relationship used technical arguments in various forums, including both advisory committees and public media, to push smoking onto the political agenda. At the same time other rival experts used technical arguments in an attempt to keep smoking off the agenda. Since smoking did get onto the political agenda the role of science at this stage of the process was both to raise the initial problem and to provide the ammunition which led to the problem reaching issue status.

In the decison-making phase of the policy debate concerning smoking and health, medical experts kept the issue on the agenda. This they did by applying political pressure through various means, including pressure group politics. However these political campaigns were backed up by continuing scientific research which began to concentrate on the relationship between smoking and various diseases other than lung cancer. Thus scientific theories and research had some role to play in keeping the issue on the agenda. However, it had little influence on the actual choice among policy options. While medical experts did make recommendations with respect to policy choices, the actual choices made were determined more by government attitudes to smoking and by the major role which the tobacco industry played in influencing or co-determining policy through negotiations with the DHSS and the ensuing voluntary agreements.

The broad character of government policy has been to favour negotiation with the industry to legislation. Science had no role in determining this general character of policy. Specific policy measures, with the exception of health education, have been worked out through the negotiations for voluntary agreements. Here the RCP have had some role in making recommendations for action which have at least been considered by governments. However, economic considerations and the industry's role in policy formation have been the determinants of the options which have actually emerged as policy. Finally, the decision to favour health education rather than direct action to reduce smoking is a political decision uninfluenced by science or scientific experts.

Particularly clear in the smoking and health case is the way in which what have here been called 'stages' of the policy process, while representing different functional activities, do not necessarily follow a neat chronological sequence. Within the general government policy of favouring negotiation to legislation in the smoking case the specific details of policy have largely been established through the voluntary agreements. Since new voluntary agreements are negotiated at regular intervals the processes of decision-making and implementation have followed an iterative cycle. An agreement has been followed by some measure of implementation which has then been followed by a negotiation to produce a new agreement, leading to further implementation. It may be noted here that, while the industry have had a major role in determining the content of the agreements, implementation of these agreements is solely their responsibility. In some cases this has meant that elements of an agreement have not been implemented or have been implemented in a form different to that intended. In this context medical experts have played some role in monitoring the implementation of the agreements. However, scientific theory or research has played no role in this activity.

The other strand of government policy with respect to smoking, health education, has been undertaken by the Health Education Council. This has been implemented as a series of anti-smoking campaigns in various media. The content of these campaigns has been drawn from medical evidence. Furthermore, social surveys have been performed to assess the impacts of specific campaigns. So medical and other research has provided the content for health education campaigns and has performed a monitoring role in their implementation. Individual medical experts have been particularly closely involved with the implementation of this aspect of smoking policy. For example, Fletcher who was chairman of the HEC. In conclusion, it can be seen that the tobacco industry have played a major role in policy implementation while medical research has provided the material for health education campaigns. Furthermore, medical and other experts have played a monitoring role in the implementation process.

In the case of education policy psychology and psychogists played no role in getting the problem of selection and secondary education onto the political agenda. The need for educational reform was forced onto the agenda by the competitive pressure on the free place examinations and by political campaigns for more equality in the education system. As was seen earlier, even the definition of the problem was not determined by the emphasis in psychology on the 'ability' of the child, nor by the development of the tools of selection. The problem was already focussed on selection before adequate tools were developed and the notion of 'ability' which officials were employing was quite different from that of the psychologists.

The case of education policy again illustrates the chronological overlap between different stages of the policy process. Between 1918 and 1944 the main ideas concerning the form which secondary education for all would eventually take were developed. In this sense the period can be seen as the decision-making phase, leading up to the 1944 Act. However, this must be qualified by saying that throughout the period various policies were being pursued and some of the aspects of selective secondary education were already being implemented in some areas of the country. Furthermore, of particular interest in this case is the way in which the implementation of certain measures influenced the decision-making with respect to future policy. For example, the policy already implemented of selecting children to take up free places in secondary schools at age 11+ influenced decisions about the age for selection when the notion of secondary education was to be expanded. During this period some local authorities implemented new ideas while others did not. Some of this implementation influenced the formation of official ideas about the structure of education. However, throughout the period the main policy of the Board of Education, often forced upon it by the Treasury, was one of restricting any educational reform through economic cuts. It is the Board's pursuance of this policy which enables us to see the period in question as the one in which future policy was formed or decided upon. The educational reforms discussed in this period were not fully implemented until after the 1944 Act.

During this policy development stage of the policy process the role of psychology was to provide a justificatory rationale for policy options already favoured on other grounds. The policy of tripartite selective secondary education was influenced more by the existing structure of educational organisation and official views about the education needs of 'types' of children destined to enter particular types of employment than by psychological theory or practice. On the major issue of the provision of different types of secondary education there was an existing consensus amongst educationists which psychological advice merely reinforced. Psychology played some role in the implementation of the policy of differentiation by providing a tool for using in the selection process. However, the adoption of psychological tests was slow and uneven, varying amongst different local authorities.

As predicted by the models of criticism and as Ezrahi has argued, the role which science plays in the policy process is influenced by the situation of consensus and criticism.[5] When there is consensus on policy and an under-critical situation arises, science is found to play a legitimating role. This is illustrated by the case of education policy. With an existing consensus on policy and a consensus amongst scientists which supported the policy consensus, science was used to provide justificatory support for a policy already favoured on other grounds. When there is dissensus on policy and an over-critical situation arises, science is likely to hinder policy-making. This is because policy-makers are confronted with conflicting scientific advice instead of the straightforward answer which positivist views of science lead them to expect. This phenomenon has lead some policy-makers to call for 'one-armed scientists'.[6] In the smoking case it may be hypothesised that without the scientific conflict in the agenda phase, for example if the genetic theory had not been proposed, the tobacco industry would have had no case for keeping smoking off the agenda. In this situation it is possible that it may not have been the 7 to 12 years which it did take for smoking to achieve issue status. Again it may be hypothesised that, if the industry had chosen to continue to vigorously pursue the genetic argument in the decision-making phase of the process and if the dissenting experts had not been marginalised by the government's acceptance of the medical profession as the legitimate experts, decisions on smoking policy would have been more confused. Or, more likely health considerations would have been given even less weight against economic considerations than they have been because the health issue would have been seen as unresolved. In the decision-making phase of the smoking debate science, and particularly scientific controversy, has not hindered policy-making because it has been largely ignored. The health question has not been to the fore because it has been outweighed by economic considerations. The scientific controversy has been ignored for the reasons we have already seen, the medical profession's role as legitimate experts and the decision of the industry to pursue other tactics.

The prediction drawn from the criticism models that either science plays no role in actual policy-formation but merely legitimates decisions already made, or, if it is influential in policy, it hinders decision making through disagreement amongst experts, seems to be borne out by the empirical case studies.under discussion. In the smoking case the hindering effect of scientific controversy has been moderated by the filters described earlier which have not allowed the scientific controversy to feed into the policy debate in quite the way that was anticipated by the over-critical model.

So the role which science plays in the policy process is different at different stages of that process and is influenced by the situation of consensus and criticism both amongst scientists and with respect to policy. The role of science is also influenced by more general aspects of the political context. For example the power and interests of the groups involved in the policy debate can influence the role which science plays in the process. In the smoking and health issue the tobacco industry have had a powerful role in influencing policy and their interests have not been seriously threatened by policy with respect to smoking. In this context they have not vigorously used the technical controversy in the policy arena when this was perfectly possible. On the other hand, while the government appear to have accepted the medical case against smoking they have not followed its implications through vigorous policies to reduce the habit. This is because economic considerations have outweighed health considerations and the relative strength of government agencies pursuing them, that is the Health Department and the Treasury, have helped define the influence which scientific arguments have had on government policy. A feature of the administrative structure of government which may also have affected the influence which science has had on policy is the lack of a central unit to co-ordinate policy on smoking. With inter-departmental conflict and lack of co-ordination within the administration, the role which scientific arguments can play in influencing policy is bound to be limited. Finally, the smoking issue clearly illustrates the way that the role which science plays in policy debates is largely determined by its use as a political weapon. For the tobacco industry supporting and publicising the views of experts who argued that the association between smoking and lung cancer was based on genetic predisposition was a useful political tactic in the attempt to keep smoking off the political agenda. The genetic theory was a useful political tool for the industry in the agenda formation period. However, once the government accepted the causal theory, the rival theory became a less useful political tool.

So, while this argument was not completely abandoned, it was relegated to the margins of the policy debate and other tactics were pursued. This suggests that, so long as scientific arguments are useful as a political tool, they will be used in the political arena. When they cease to be useful as a political tool they are dropped in favour of more effective tactics.

As Lasswell and Lerner have hypothesised, "To the degree that political power is sought and is perceived to depend on the use of knowledge, knowledge will be sought and applied".[7]

As can be seen from the above discussions of the various factors influencing the role which science plays in policy-making, if changes occur in these various factors then the role of science will also change. This confirms and extends the point made by Ezrahi in reference to the influence of the situation of consensus amongst scientists and amongst politicians. The argument that changes in the situation of consensus will produce shifts in the role of science and of scientists in the policy process is confirmed in this study. Furthermore, the same can be said for the other influences, the stages of the policy process and the political context. As a practical problem moves from one stage of the policy process to the next, the role which science plays will shift. Similarly, as the political context of power, interests, administrative structure and political usefulness of scientific arguments changes, so the role of science will change.

The discussion so far has been particularly concerned with the role of science in policy-making. Science in this context is meant to refer to scientific ideas, theories and arguments, not so much to the institutions of science or to individual scientific experts. It is important to note at this point that the role which scientific ideas or the implications drawn from scientific research may have on policy-making can be different from the role which individual scientific advisers may have. For example the claims of a particular expert may be accepted because of his standing as a respected scientist. It is implied by this that the claims which the expert makes are derived from the assessment of scientific research and theory. However, it is quite possible for experts in positions of apparent influence to make all sorts of recommendations which do not necessarily follow from their assessment of scientific research. An example of this occurred in the case of the development of education policy. In evidence presented to educational advisory committees Cyril Burt argued that from the evidence collected by educational psychologists it was clear that individual differences in intelligence increased as children grew older. According to Burt this evidence leads to the conclusion that, by the age of 11+, children of different ability should be educated in different types of school. However, Godfrey Thomson who agreed with Burt that differences in children's ability increased as they grew older, did not agree that this called for different types of school. He was in favour of the multilateral school which would provide different types of education for children of different ability in the same school. Now this does not represent a difference of interpretation of scientific evidence since Burt and Thomson agreed on this point. The difference is over the policy implications drawn from the evidence. It may be asked, 'Do their different assessments of the policy implications of psychological theory derive from some aspect of their scientific expertise?' A clear answer to this question is provided by Thomson who argues that the only grounds for supporting the multilateral concepts are social and political, based on the concept of equality.[8] While psychological theories at the time supported the policy of providing different education for children of different abilities, it was quite consistent with this theory to argue for either a policy of providing different types of school or a policy of providing different types of education within one school. In the light of this difference between the advice of Burt and Thomson it seems clear that Burt's advice to the advisory committee was not all based on psychological theory. (He might not claim this since he might claim that it was based on psychological theory and practical educational experience).

In confirmation of Collingridge's model of expert advice discussed in Chapter 2 and contrary to the positivist model, it can be seen from the present study that experts do not confine themselves to presenting and interpreting scientific information. In all the case studies experts are keen to draw policy implications from their work. Furthermore, they become involved in the policy debate as activists pushing for particular policy options. This role is particularly clear in the smoking case where, in the agenda phase, medical experts involved themselves not just with making recommendations as members of advisory committees, but with public anti-smoking campaigns. In the policy development phase the establishment of ASH saw medical experts getting involved in pressure group political activities.

In the education example, the above discussion of Burt's role on advisory committees provides evidence of an expert providing more than just information derived from scientific theory and research.

12.4 Summary

In summary the three case studies confirm Collingridge's model of expert advice. Disputes amongst experts centre on the interpretation of data. The context of these disputes provide strong motivations to vigorously pursue those rival interpretations which support different policy options. Experts can be seen as advocates for particular policy options using technical arguments to support their case. The 'professional differences' approach of Robbins and Johnston provides a useful tool in explaining the motivations driving these debates. The case studies also confirm the models of criticism developed in Chapters 3 and 4. It appears that, when there is dissensus on policy and dissensus amongst scientific experts, an over-critical situation will develop, producing an endless technical debate which, far from helping the resolution of the policy debate, merely helps prolong it. When there is consensus on policy an under-critical situation will arise, in which the reliability of scientific advice is not adequately assessed. However, in this situation with an existing policy consensus, that advice provides only legitimatory or justificatory support for policy options already favoured on other grounds, and is not directly influential in the formation of policy. Finally, the role which science plays in the policy process is determined by a number of factors. These determining factors include:- what stage the problem has reached in the policy process; the existence and level of consensus and criticism in both the policy and the scientific arenas; the political context of power and interests of the groups involved. This role can shift as these various determining factors change.

The present study leaves us with a very different image of the relation between science and policy from the traditional view of neutral scientific experts providing straightforward information which policy-makers can utilise in a way that makes decision-making for policy more rational. The study illustrates the value of criticism in ensuring that the reliability of expert's claims is not overestimated. It also shows that this criticism is difficult to control and is likely to make science of little use to policy-makers who demand simple answers. The study leads to the demand that the complexity of practical problems be recognised and the myth that science is able to solve them in a relatively unproblematic way be rejected. It further suggests that the role which science can play in the policy process is much smaller than is usually realised.

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