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YOUNG SCIENTISTS AND THEIR WORK

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ABSTRACT

The idea that academically trained scientists have difficulty in adjusting to the demands of industrial employment is a commonplace both in sociological literature and public debate. Sociologists, following the influential work of Robert Merton, have developed the theory that scientists are socialised at university into a set of values — the 'ethos of science'. These stress the pursuit of knowledge for its own sake and are held to conflict with the utilitarian and competitive values of industry. As research has accumulated, the evidence for this value conflict had become increasingly sparse. Recent studies of B.Sc. graduates, in particular, have led to a radical questioning of this picture of the effects of scientific education. Nevertheless, overall, the evidence is not clear cut. The possibility remains that value conflict would be observed if studies were confined to Ph.D. scientists, because of their more prolonged exposure to the 'ethos of science'. Hence the present study.

It was in two stages. At stage one, 357 final year Ph.D. students in 34 different university departments of physics and chemistry returned a questionnaire on their attitudes to science and industry and their own employment preferences. A subsample of 25 of these were also interviewed. At stage two, 40 of the scientists who took

industrial jobs were interviewed at length. The details of their jobs and their locations in the country varied widely, but they had all been at work for about one year.

The results ran totally counter to the 'value conflict' theory. Detailed examination of the second stage interviews showed that the scientists were not attached to the 'ethos of pure science' and did not experience value conflicts. Instead they completely accepted industrial norms. They were eager to make a useful contribution to industry and took for granted its pragmatic, commercial and utilitarian values. Not only did they have the right values for industry, they also seemed to have the right skills. Their descriptions of their work and its relation to their Ph.D. research suggested that their training had endowed them with appropriate problem solving skills, which they were capable of deploying independently and flexibly.

The evidence from the first stage questionnaires indicated that these industrial scientists had not been attached to the 'ethos of science' even while at university and hence had not had to change their values on entering industry. This raised the question of whether they were an especially industrially orientated group. Had they perhaps resisted or rejected the 'ethos of pure science' ? The answer was negative. Comparison with the eventual academics showed that the two groups were very similar in all respects except their personal career preferences. Eventual academics were no more attached to the 'ethos of science' than the industrialists.

The two main conclusions of the study are thus: (1) that the gap in the evidence against Merton's value conflict thesis can now be closed; and (2) that scientific training is best seen as the transmission of cognitive factors rather than values. These are of two kinds: (a) the skills and knowledge of the scientist's trade; and (b) a 'cognitive map' of his social environment with a sense of the behaviour that is appropriate in different places. Insofar as values enter at all, the picture is the reverse of Merton's. The dominant values of industrial scientists are everyday utilitarian ones and these remain intact throughout academic training. Value conflict is possibly more of a danger for those who stay in academic life than for those who go into industry.

This study depended on the help and goodwill of the many scientists who returned questionnaires and were interviewed. To them, and to their university heads of department who originally enabled me to contact them, I am deeply indebted.

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CHAPTER 1

THE WORK ON INDUSTRIAL SCIENTISTS

It is widely believed that highly qualified scientists are narrowly specialised, lacking both the capacity and the inclination to move beyond their own esoteric research areas. Such inflexibility is often associated with the special qualities that are supposed to characterise scientists — their dedication, intellectual penetration and compulsive commitment to their work. In addition, their education is seen as reinforcing these psychological proclivities. Not only is a scientific education notoriously specialised; it is also held to imbue students with strong academic orientations. Thus successive government reports have pondered the question of how to lure the country's ablest young scientists away from universities and into industrial jobs.[1] The most common answer proferred by sociologists has been that industry should simulate the world of academic science as closely as possible. Alternatively, other commentators have suggested that university courses should be modified to take more account of the needs of industry.

Whatever the emphasis in the solution, the diagnosis has

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[1] Interim Report of the Working Group on Manpower Parameters for Scientific Growth, (Cmnd. 3102, 1966); The Flow into Employment of Scientists, Engineers and Technologists, (Cmnd. 3760, 1968); Postgraduate Education, Third Report from the Expenditure Committee (1973), HMSO 96-1.

been the same. Layman, sociologist and policy-maker alike have located the problem in the divergences between the worlds of academic science and industry. It has been assumed that a university education will have profound effects, leaving its graduates with values and orientations that are fundamentally at odds with the more worldly demands of an industrial job. Furthermore, these academic perspectives acquired during training are believed to achieve a certain degree of permanence. It is imagined that an academic, once moulded, will tend not to change much.

Such an image of the scientist is the focus of this study. It is about the training and subsequent employment of some Ph.D. scientists and their experience of conflict or continuity in making the transition from university to industry. At a specific level, it is an investigation of scientists' values, attitudes and orientations and what happens to them in changing circumstances. More generally, it is concerned with the way some very able young adults learn and change.

The suggestion that scientists do indeed change in adult life and are capable of adapting to a variety of differing situations has only recently been seriously entertained by sociologists. The idea that their ways are more likely to be set in the pattern of their academic training has been much more common. Indeed such a viewpoint has tended to dominate studies of scientists. It is embedded in a tradition of sociological analysis stemming from the very influential work of Robert Merton, but has since been elaborated, modified and empirically documented by many writers. In the last few years commentators have been increasingly critical of Merton's argument and its modifications and evidence has been accumulated which suggests that alternative accounts of the behaviour and attitudes of scientists might be more illuminating. Nevertheless, work is still being done in the traditional framework and support is still given to the view propounded by Merton.[2] A recent reviewer, for example, has characterised his work as "the sociology of science at its best".[3]

In the rest of this chapter, the debate over Merton's work will be explored in greater detail. Because his theory embodies all the important presuppositions of his followers' work, the presentation will begin with an examination of that theory. The main lines of development from his work will then be considered. Finally some more recent work will be presented which suggests the usefulness of a different approach.

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[2]For example, Jerry Gaston, "The Reward System in British Science", ASR XXXV (1970), pp. 718-32; Marlan Blisett, Politics in Science (Boston, Little Brown and Company, 1972); J.R. Cole and Stephen Cole, Social Stratification in Science (Chicago, University of Chicago Press, 1973).

[3]John Ziman reviewing Merton's The Sociology of Science: Theoretical and Empirical Investigations, (ed. N.W. Storer), (Chicago, University of Chicago Press, 1973), in "The Sociology of Science at its Best", Minerva, vol. Xll, no. 2, April 1974, pp. 283-6.

MERTON'S THEORY

Merton formulated his ideas on the sociology of science in a series of papers begun in the late thirties. His focus was science as a social institution — its internal workings and its relations with the wider social structure. His concern, in particular, was the functionalist one of identifying the factors most likely to forward or to interfere with the institutional goal of science, namely the extension of certified knowledge. His predominant interest however was not in the methodological canons which guide scientists, but in the more general beliefs and values which constrain their action. In other words, his approach to the understanding of science was to concentrate on the social processes that make up the institution, rather than the technical contents of scientific work.

The community of scientists, Merton argued, is characterised by a distinctive ethos :

> The ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science. The norms are expressed in the form of prescriptions, preferences and permissions. They are legitimatized in terms of institutional values. These imperatives, transmitted by precept and example and reenforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience or, if one prefers the latter-day phrase, his superego. Although the ethos of science has not been codified, it can be inferred from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed towards contraventions of the ethos.[4]

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[4] Robert K. Merton, Social Theory and Social Structure, revised and enlarged edition, (London, Collier-Macmillan Limited, 1957) pp. 551-2.

Furthermore, these mores, Merton stated, ". . . possess a methodologic rationale but they are binding, not only because they are procedurally efficient, but because they are believed right and good. They are moral as well as technical prescriptions."[5]

In his original discussion, Merton identified four sets of institutional imperatives which, he claimed, comprise the ethos of modern science. To describe them briefly, these were : —

Universalism: referring to the imperative that the validity of knowledge claims be determined by the use of pre-established objective criteria, irrespective of the personal characteristics of their protagonists.

Communism: referring to the imperative that no one scientist has property rights in any scientific finding, but that on the contrary, all scientific findings must be freely available for the rest of the scientific community.

Disinterestedness: referring to the imperative that scientists should not seek inappropriate rewards for doing science, such as, personal popularity or wealth.

Organized Scepticism: also a methodological canon, refers to the imperative that a scientist should be continuously critical in his scrutiny of his own and other people's research.

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[5] Ibid. p. 553.

These imperatives or norms are held to be personal in the sense that they impinge on the consciousness of individual scientists, but, as Merton frequently emphasised, they are also institutional norms. This means that, regardless of individual scientists' motivations for conforming to them, science is dependent on their operation for advancement of its institutional goal — the extension of knowledge.

The final explanatory link in this functionalist scheme was to identify the processes mediating between these institutional imperatives and scientists' motivation to conform to them. First, Merton envisaged a process of socialisation. He described the imperatives as being "transmitted by precept and example", and being "in varying degrees internalized by the scientist". In addition, he located the motive force behind conformity to the norms in the reward system of science.[6] He argued that scientists are motivated to make original contributions to knowledge by the recognition and esteem that accrues to them for it. Hence the importance of publication and the frequent and often bitter priority disputes in science.

Since outlining this basic theory, Merton has elaborated it to some extent.[7] In addition, many other people have developed and substantiated it with empirical evidence. Much of this research

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[6]This idea is developed more fully in a later paper, Priorities in Scientific Discovery: A Chapter in the Sociology of Science, reprinted in Bernard Barber and Walter Hirsch, eds., The Sociology of Science (New York, The Free Press, 1962).

[7]See Robert K. Merton, The Sociology of Science: Theoretical and Empirical Investigations (Chicago and London, The University of Chicago Press, 1973).

has been concerned with the way the norms may be violated when science enters the public domain, most notably industry. For example, the norm of communism is seen as being incompatible with the demands of a capitalist enterprise which defines certain technological findings as private property and protects them with patents. The scientist working under such restrictions will thus experience frustration, because his moral beliefs about the importance of communism will be antithetical to what is permitted him as an employee in an industrial company.

In his papers on scientists, Merton does not deal with the possible areas of conflict between the scientifc order and wider society in any systematic fashion. He merely illustrates the theme with examples. However, he has developed similar ideas in a different, but closely related context, that of the intellectual in bureaucracies. His essays on this topic[8] are interesting for two main reasons. First, they represent a more general application of his arguments about scientists, making explicit some of the points that are only hinted at in these earlier writings. Second, they contain the germ of an analysis of scientists in industry very different from the value conflict picture usually associated with his theory.

Merton's concern in these essays was to explore some of the areas of conflict between intellectuals and bureacracies.

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[8] "Bureaucratic Structure and Personality" and "Role of the Intellectual in Public Bureaucracy", in Social Theory and Social Structure, chs. Vl and Vll.

Intellectuals, he argued, "become imbued with values and standards which, they believe, are not consistent with a place in the business world". "Many intellectuals have become alienated from the assumptions, objectives and rewards of private enterprise."[9] In particular, conflicts are likely to arise over the aims and methods of the projects intellectuals are expected to carry out and over the implementation of the results. Faced with such value conflicts, the intellectual has three alternatives :

> (1) He can accommodate his own social values and special knowledge to the values of the policy-makers. (2) He can seek to alter the prevailing policies of the executives in the bureaucratic apparatus. (3) He can respond in terms of a schizoid dissociation between his own values and those of the bureaucracy, by regarding his function as purely technical and without value-implications.[10]

Merton supposed that the third response was the most common, that is, that the alienated intellectual would gradually be accommodated to the bureaucratic decision-makers by being transformed into an a-political technician. Their sentiments and values, he argued,

> . . . are broadly those of the prevailing power groups. The technicians conceive their role as merely that of implementing whichever policies are defined by policy-makers. The occupational code of the technician constrains him to accept a dependency-relation to the executive. This sense of dependency, which is hedged about with sentiment, is expressed in the formula: the policy-maker supplies the goals (ends, objectives) and we technicians, on the basis of expert knowledge, indicate alternative means for reaching these ends.[11]

Such a role would be congenial to the intellectual because it is supported by his occupational mores, that is: as men of science they do not indulge in value judgements.

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[9]Ibid. p. 212. [10]Ibid. p. 219. [11]Ibid. p. 213.

In developing this theme of value conflict and accommodation, Merton could easily have been writing about scientists rather than intellectuals in general, although he does in fact distinguish between the two groups fairly firmly.[12] Consequently, nowhere in his work does he make such a substitution or explore its consequences. This is regrettable because much of the evidence accumulated by workers after Merton has shown that some process of accommodation does occur when scientists go into industry. Its form and nature has been a matter for debate. Nevertheless, the notion that there is a ready-made role in industry for the scientist, as "technician", is one which has received little attention. Recent evidence has suggested increasingly that it is common for scientists to adjust to industry. If Merton's arguments about intellectuals being gradually transformed into technicians is applied to scientists in industry, then much of this evidence is explicable within that perspective.

Some further work will now be considered briefly to bring out its affinities with Merton's position. Contributions by Hagstrom, Storer, Barber and Kornhauser will be specifically considered.

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[12] *Ibid.* p.210.

OTHER 'VALUE CONFLICT' THEORISTS

W.O. Hagstrom's book The Scientific Community is one of the best known accounts of the workings of science. Hagstrom follows Merton in accepting from the outset that "the socialisation of scientists tends to produce persons who are so strongly committed to the central values of science that they unthinkingly accept them".[13] His main concern is to develop another strand of Merton's thought to do with the processes that maintain and reinforce these values. Hagstrom's thesis is that: "Social control in science is exercised in an exchange system, a system wherein gifts of information are exchanged for recognition from scientific colleagues."[14] Knowledge grows because of the contributions freely made to it by scientists who are motivated by the recognition they will get from other scientists. The process of exchange not only maintains the quality of work, but is also the route through which scientists pursue careers and accrue status in the scientific world. Thus, he says:

> The forms of recognition awarded in primary groups of scientists tend to make the institutional imperatives meaningful for day-to-day work. They typically reinforce the effects of institutional incentives, and without them scientists might conform less to the norms and values of science; they might be less disposed to work, to publish, or to select problems and techniques within the scope of their disciplines.[15]

The norms and values Hagstrom views as important are

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[13]W.O. Hagstrom, The Scientific Community (New York, Basic Books, 1965), p.9.

[14]Ibid. p. 52. [15]Ibid. p. 36.

the ones outlined by Merton, plus a norm he adds himself — the norm of independence. By this he refers to the belief, implicit in the ethos of science, but not specifically identified by Merton, that scientific creativity can only prosper when the scientist has freedom to select his own research problems and the methods and techniques to be applied to them. In most subsequent studies of scientists, the norms of science have been taken to refer to this independence-autonomy norm as well as to Merton's.

N. Storer similarly views the scientific community as a system of exchanges :

> The norms of science have their origins and their central importance in the maintenance of a social situation in which the commodity of honest, competent response to creative work can continue to be obtained by all members of this social system.[16]

While he, too, accepts Merton's thesis that the norms of science are functional for the advancement of science as an enterprise, he particularly emphasises Merton's argument that institutional and motivational levels of analysis must be distinguished. He raises the question of why scientists come to invest the norms with moral potency. He argues that :

> It is the occasional reinforcement given these norms by the scientist's awareness of their relevance to his own interest in obtaining competent response to his work rather than to the general goal of science, which I feel accounts for their continuing moral potency. The norms are important to scientists because they concern something in which scientists have an immediate stake, not because they are beneficial over the long-run to science as a whole.[17]

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[16] N.W. Storer, The Social System of Science (New York, Holt, Rinehart and Winston, 1966), p. 37. [17] Ibid. p.84.

The main focus of interest for both Hagstrom and Storer is the scientist working in an academic context. They do not discuss in detail the conflict allegedly experienced by the scientist working in industry. Nevertheless, they do believe it exists and see it centering specifically round the norms of disinterestedness and communism (or communality). Both writers suggest that some accommodation to the conflict is possible, though they imply that this will be at the expense of the purity and integrity of science. Hagstrom, for instance, says :

> Among scientists who remain in industry there will be a strong tendency for the incentives offered by the employing organizations — interpersonal approval as well as formal status and salary — to become more important than recognition by the larger scientific community.[18]

For Storer, the problem of disinterestedness focuses on the dichotomy between pure and applied science. He cites examples of the hostility with which pure scientists may view their applied counterparts and relates them to the exchange system in science :

> To work on problems that are primarily of importance to non-scientists (the government, the industrial employer, the lay public) would make one run the risk of being separated from the central scientific universe of discourse. Further, and perhaps more immediately important, one who seeks an inappropriate commodity in return for his research efforts is in effect denying the importance to himself of the appropriate commodity and is thereby indicating not only his lack of allegiance to science as a whole, but also, through setting a potential precedent for others, he is threatening the entire exchange system itself.[19]

Hagstrom and Storer then both develop the problem of disinterestedness in terms of Barber's formulation that the scientist is not expected to pursue personal gains like money or prestige :

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[18] Hagstrom, p. 37. [19] Storer, p. 89.

> There men are expected by their peers to achieve the self-interest they have in work satisfaction and in prestige through serving the community interest directly, and this is done through making contributions to the development of the conceptual schemes which are of the essence of science.[20]

For Barber, the norm of disinterestedness is intimately related to communality. He says :

> Without 'disinterestedness' as one of the rules of the game in science, it is unlikely that the value of 'communality' with regard to scientific innovations could prevail. If too many men should draw upon the scientific theories held in common only to use them for their own immediate purposes, for example, in the service of their own personal power rather than in the service of science itself, then the community property would cease growing and thereby lose its essential scientific characteristic.[21]

Storer likewise sees danger for science in the violation of the norm of communality, but instead of appealing to moral incentives, characteristically refers to the exchange system in which the scientists' personal interests lie.[22] Storer, in addition, is by no means as adamant as Hagstrom and Barber on the necessary persistence of the norms of science for the very existence of science. He concedes that :

> As a result [of more scientists doing applied work in industry] I would expect that a means will be developed by which a different set of norms more appropriate to the situation in which they are working may be legitimated for these scientists. The new normative structure, perhaps subsidiary to, but still capable of existing side by side with the traditional ones, will probably be similar to the professional ethics now characteristic of the legal and medical professions; it will focus more upon the attitude appropriate to the application of specialised knowledge and less upon the ultimate value of the knowledge itself.[23]

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[20] Bernard Barber, Science and The Social Order (London, Bradford and Dickens, 1953), p. 92.

[21] Ibid. [22] Storer, p. 127. [23] Ibid. p. 165

This argument is very similar to Merton's when he talks about intellectuals being transformed into 'technicians'. Just as Merton failed to apply the argument to scientists, Storer also fails to develop his argument.

William Kornhauser's Scientists in Industry[24] is also addressed to the processes of conflict and accommodation that occur when professionals are employed in large-scale bureaucratic organizations and deals explicitly with the issues that tend to remain implicit in Barber, Hagstrom and Storer's work. Their position on the basic conflict for scientists in industry are summarised by Kornhauser's formulation that :

> . . . the issue of basic versus applied research expresses the underlying tension between professional science and industrial organization. Professional science favors contributions to knowledge rather than to profits; high-quality research rather than low-cost research; long-range programs rather than short-term results; and so on. Industrial organization favors research services to operations and commercial development of research. These differences breed conflicts of values and goals; they also engender conflicting responsibilities and struggles for power.[25]

Kornhauser's argument centres around the conflict between organizational norms of control and professional norms of autonomy. For him :

> the basic dilemma in the social control of applied research centers on the need for executive coordination of research with other parts of the enterprise versus the need for professional autonomy in research to give wide scope to the scientist's 'controlled imagination' and motivation to make original contributions.[26]

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[24]Berkeley and Los Angeles, University of California Press, 1962.

[25]Ibid. p. 25. [26]Ibid. p. 45.

Again the norm of communality is posed as a major source of conflict for the scientist. "Not only has the scientist been taught to uphold norms of open communication which conflict with the requirements of organizational secrecy, but the existence of such secrecy deprives him of opportunities to win scientific recognition for his accomplishments."[27] The other major source of conflict according to Kornhauser is the independence-autonomy norm. "Professional science teaches that the researcher must be free to work on problems of interest to him and to follow new leads as they emerge from his work, if he is to make the maximum contribution to science."[28]

Although Kornhauser's main emphasis is on the conflicts and tensions between science and industrial organizations, he does allow that some scientists may come to tolerate secrecy and also that some organizations may have fairly liberal publication policies. He suggests that the more strongly the research worker is orientated to the world of pure science, the more acutely he will feel the conflict[29], but he does not explain how such differential orientation might occur. Nor does he lay as much stress on the scientists who do become accommodated to industry as he does on their more academically orientated counterparts. Kornhauser does suggest that as well as scientists varying in their preparedness to accommodate to industry, ". . . business firms seek to increase the commitment of their participants, so that the individual's main orientation, including his hopes for advancement, lies within the establishment".[30] Nevertheless, the implication remains that some degree of conflict cannot be avoided, for if —

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[27] Ibid. p. 74. [28] Ibid. p. 81. [29] Ibid. p. 80 [30] Ibid. p. 155.

> . . . these participants [in business firms] are scientists or engineers, they also face demands for loyalty from their professions, which need member commitment in order to protect their own values and standards. As a result, interaction between professions and organizations produces competing orientations, career lines and incentive systems.[31]

In all the works considered so far, the social system of science has been presented as a discrete, identifiable community of practitioners, operating according to certain definite norms and values which they have internalised during their scientific training. These norms and values are taken to have an enduring potency and will thus continue to be significant even for the scientist who leaves the mainstream of academic science to take a position in industry. There is nowhere the explicit contention that any conflict between scientific and industrial norms will be absolute or insurmountable; implicit however is the notion that the prevalence of the norms and values, or ethos, of science is the normal (and even right) state of affairs and that any deviation from this represents something to be explained. The very use of such terms as "accommodation" suggests that here are two systems and that any intermediate state must be a compromise between them rather than a system in its own right.

All the writers just discussed share Merton's major theoretical presuppositions. They also develop his substantive claims. Indeed it is difficult to find elements in their work which are not derivative of him, although they have drawn out some of the implications of his work and presented them more systematically. They also share with Merton a tendency to write impressionistically: their claims bear only an attenuated claim to any evidence. As

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[31] Ibid.

Norman Ellis has shown in an excellent review of these and other similar contributions, they often lapse into circularity when they attempt to substantiate their claims. They frequently cite each others' work and Merton's impressions, regardless of the lack of systematic evidence to support them.[32]

Studies which have been based on more substantial empirical research have placed more emphasis on the theme of accommodation. These contributions by Avery, Marcson, Abrahamson and Box and Cotgrove will now be introduced.[33]

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[32] Norman Ellis, The Scientific Worker (unpublished Ph.D. thesis, University of Leeds, 1969), see especially notes 10 and 12, p. 39.

[33] but see also J.R. Hinrichs, "The Attitudes of Research Chemists", Journal of Applied Psychology, (1964) vol. 48, no. 5, who has produced similar evidence.

MODIFIED VALUE CONFLICT THEORISTS

Avery[34] considered evidence from interviews with over a hundred scientists in ten industrial laboratories to suggest that the new researcher in an industrial enterprise undergoes a learning experience in which he attempts to relate his technical competence to the needs of the company. He called this process "enculturation". Marcson[35], on the basis of a study of the central research laboratory of a large electronics firm, described a similar process which he called "acculturation". In the course of this process, mutual adaptation and learning occurred between the research organization and its scientific recruits. Marcson claimed however that the laboratory effected more changes in the scientists than vice versa. Abrahamson[36] carried out a study of industrial scientists in five laboratories of varying size and function, on the basis of which he argued that resocialisation occurs for academically trained scientists in industrial research. His results suggested that the more thorough the initial academic socialisation of the scientist had been, the more difficult he would find it to adjust to industry. The greatest conflicts focused around unfulfilled demands for autonomy. However, as resocialisation proceeded, the discrepancy between desired and received autonomy tended to diminish, producing

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[34] R.W. Avery, "Enculturation in Industrial Research", IRE Transactions on Engineering Management, EM-7 (1960), pp. 20-24.

[35] Simon Marcson, "Role Adaptations of Scientists in Industrial Research", IRE Transactions on Engineering Management, EM-7 (1960), pp. 159-66.

[36] M. Abrahamson, "The Integration of Industrial Scientists", Administrative Science Quarterly, 9(1964), pp. 208-18.

higher "integration" in the laboratory. Mutual readjustments occurred between the scientists and the laboratory management: the former came to want less autonomy and the latter to grant more.

The final study to be introduced in this 'modified view of conflict' tradition was carried out by Box and Cotgrove.[37] Taking their lead from Kornhauser, they argued that a process of differential socialisation occurs. On the basis of questionnaire responses from chemistry undergraduates, they attempted to measure differential commitment to the scientific values of autonomy, disciplinary communism and commitment to science. Their evidence showed that the values are linked together rather than being independent. They suggested accordingly a three-fold classification of scientists into public, private and instrumental types. The defining characteristics are summarised in their paper as follows :

Types of Scientist[38]

Attachment to value of : —

	Autonomy	Commitment	Disciplinary Communism
Public	+	+	+
Private	+	+	-
Instrumental	±	-	-

[37]Steven Box and Stephen Cotgrove, "Scientific Identity, Occupational Selection and Role Strain", BJS, XVll (March 1966), pp. 20-8.

[38]Ibid. p. 22.

Box and Cotgrove also presented evidence from a series of interviews in ten chemical and pharmaceutical companies to suggest that a process of occupational selection occurs on the part of both employing organizations and individuals. This means that public scientists committed to the ethos of science will tend to be filtered out. Thus, the overall amount of role strain, due to value conflicts, will be lessened. This selection mechanism works imperfectly so that both public and private scientists will be found in development work. The public scientists will experience the greater strain as evidenced by their dissatisfaction with publications and patents policies, supervision and autonomy.

Box in collaboration with Ford has also gone further than any of the other writers considered so far in producing an explanation for the differential socialisation of scientists that they allege occurs. They argue that working class science students are particularly likely to assume the identity of dedicated scientist.[39] This is because in the predominantly middle class environment of a university, the working class student will find himself socially marginal. He is therefore likely to experience an acute identity crisis. Taking on the identity of dedicated scientist — which, it is claimed, is relatively classless and depends on role models which are highly visible — can solve this identity crisis. This process is especially likely to occur if certain other conditions obtain: the student is, (1) first/only born; is non-religious and has experienced childhood isolation; (2) has a university reference

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[39] Steven Box and Julienne Ford, "Commitment to Science: A Solution to Student Marginality", Sociology, XVlll (Sept. 1967), pp. 225-38.

group favourable to academic socialisation; and (3) expects to have an academic career.

Box and Cotgrove develop a variation of this theory to explain the dedication of middle class students.[40] Such a student's inclination to assume a "scientific identity" arises from failure to develop social skills and consequent investment in things rather than people. These early experiences will make it more difficult for him to adjust to the university environment, thus producing a crisis and a solution not unlike that of the marginal working class student. Presumably, Box and Cotgrove would argue that working class students are more likely to experience identity crises and consequently become dedicated scientists, because their problems are the same as those experienced by the middle class student, but reinforced by extra problems deriving from their class origins.

As empirical studies in the area have accumulated, there has been a progressive shift in the extent to which conflict and strain have been seen as a characteristic part of the scientist's experience in industry. Nevertheless, all the studies considered so far have come to the same conclusion: that the scientific ethos will act in varying degrees as a barrier to adjustment for scientists in industry. Whether the severity of the conflict is maximised, as in Kornhauser's or Barber's work, or minimised, as in Avery's or Box and Cotgrove's work, it is taken for granted that such a conflict is inherent in the relationship between science and industry. If

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[40] Stephen Cotgrove and Steven Box, Science, Industry and Society (London, George Allen and Unwin Ltd., 1970), ch. 3.

scientists seem to be attached to academic norms and values, this is taken as evidence for the conflict; when there is no longer evidence of such attachment, the scientist is assumed to be "adapted", "enculturated", "integrated" or "accomodated".

So far, the discussion of Merton's theory and its modifications has proceeded as if it had been almost entirely without critics. The contrary has in fact been the case and, especially in recent years, commentators who dissent from what they call "the prevailing orthodoxy", "the traditional viewpoint" or "the Mertonian school of thought" have increasingly become the norm.[41]

An important source of such criticism has been the mounting evidence which suggests that industrial scientists are not committed to the ethos of science and do not suffer value conflicts. Two studies which report such findings will now be considered.

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[41]Work critical of the value conflict picture began to appear in 1960, but had little impact. For example, although Avery's and Marcson's work was published in 1960, it was largely ignored by Kornhauser, whose book came out in 1962. Leslie Sklair (Organized Knowledge, London, Hart-Davis, MacGibbon, 1973) argues with some justice (p. 152) that sociologists of science have been ignorant of the arguments of West and Krohn (which will be discussed later). Sklair himself fails to mention explicitly the work of Barnes or Mulkay (which will also be discussed later).

ALTERNATIVE APPROACHES TO INDUSTRIAL SCIENTISTS

First, Ellis carried out a study to examine the extent to which industrial scientists are committed to the ethos of science.[42] Using questionnaires and interviews with some four hundred scientists and engineers employed in universities, government and industrial research establishments, he found that the "community of scientists" is far too diverse a body for its members to embrace a single value system of any kind. Ellis found that the work orientations and frames of reference of research scientists are largely shaped by the varied contents and contexts of their work roles. Thus the industrial scientists in his sample had much more in common with the industrial engineers than either group had with its academic counterpart. Furthermore, he found no evidence for value conflicts. This is not to say that all the scientists in his sample were satisfied. Specific problems centred round their felt lack of promotion opportunities and the extent to which they were often cut off from the operations of the rest of the firm.

Similar evidence was exploited to much greater effect by S.B. Barnes.[43] Barnes asked how far undergraduate scientists accept a characteristically academic image of the scientist and his role, and to what extent such an acceptance produces conflict for those who take up industrial employment. During 1969, 281 final-year

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[42] The Scientific Worker. See also "The Occupation of Science", in Barry Barnes, ed., Sociology of Science: Selected Readings (Harmondsworth, Penguin Books Ltd., 1972).

[43] S.B. Barnes, "Making Out in Industrial Research", Science Studies, 1 (1971), pp. 157-75.

undergraduates were interviewed during practical sessions in nine different university departments of physics and chemistry. Of this group, 64 were traced into industrial employment and 53 were interviewed again.

Like Ellis, Barnes found that there was a close connection between the scientists' values and attitudes and their varying work situations. He made sense of the job adjustment of his sample by focusing on the concrete features of their work situations and the strategies they adopted to overcome their immediate problems or to exploit their possibilities. On the whole, they expected to have above average status; to find their work a major source of satisfaction and reward; and furthermore they hoped to be successful at it.

Barnes' findings are not easy to explain in the traditional perspective of value conflict. Instead, he applied Howard Becker's situational adjustment theory[44] to show how the scientists were attempting to "make out" in their industrial jobs. The scientists' attitudes to autonomy and communality — two allegedly key academic values — may help to illustrate the approach.

Concern with publishing was almost universally low and almost totally in accord with the perceived publishing norms of the firms the scientists were working for. This was true even of the graduates who were keen to emphasise the importance of publishing while at university. The number of graduates with such views who

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[44] Outlined in his paper, "Personal Change in Adult Life", Sociometry, XXVll (1964), pp. 40-53.

secured jobs where publishing was restricted was small, but nevertheless, the evidence for situational adjustment was strong in this respect — in no case did continued attachment to academic values prevent or hinder it.

Concern with autonomy was a more complex matter and was based on a distinction between what the scientists saw as legitimate and illegitimate direction of their work. The graduates' conception of their rightful areas of autonomy can only be conveyed descriptively since, as Barnes pointed out, it involved "no disembodied idea of research freedom". "Notions of autonomy were essentially a desire to accept only that authority which was grounded in appeals to knowledge, and which was prepared in consequence to acknowledge the graduates' own knowledge".[45]

Certainly there was concern here with autonomy, but to regard it as an effect of persisting attachment to academic norms and values (or even as peculiar to scientists) seemed most unsatisfactory. Why was the concern with autonomy so specific, while worry over other aspects of autonomy was not found ? The provision of project choice, flexible working hours and unlimited time and freedom for professional consultation were all non-issues. Similarly, there was no resentment of costing controls or other economic demands. Again, if the scientists' concern with this limited form of technical autonomy was due to academic values, why was there not attachment to other academic values as well — why

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[45] Barnes, Making Out, p. 162, original italics omitted.

autonomy and not communality ? And why was the concern not expressed as a general value, but geared to the work situation ? On the other hand, the concern that was found cannot be regarded as a simple response to what was expected in the work situation, for it sometimes led to clashes with authority and active dissatisfaction. Barnes argues that this form of concern with autonomy can be seen as part of an attempt to succeed — and to succeed along lines appropriate to the organisation. Only by having technical autonomy is the scientist able to prove his competence, legitimate his claims for status and justify his desire for advancement. In line with this, the scientist's degree and his expertise can be seen as a major resource in his attempt to get on in industry.

Barnes found that such a view made various aspects of his data intelligible: the way freedom was only invoked as a right in contexts which threatened the scientists' competence; the lack of interest in scientific publications; and the circumstances in which specifically scientific skills ceased to be of concern. On the whole, dissatisfaction amongst the sample occurred not because of any conflict between academic and industrial values, but because, in a sense, the scientists wanted to succeed in industrial terms too much. They were dissatisfied when the opportunities to use scientific skills and to get rewards for their successful implementation were restricted, whilst, at the same time, there were no alternative ways of getting recognition and rewards within the industrial organisation.

The picture of the industrial scientist developed by Barnes and Ellis contrasts markedly with that of the value conflict theorists and their modifiers. What response can be made to this apparent conflict in evidence ?

There are several possible explanations for the conflicting pictures of the relation between academic training and industrial employment. They may result from sampling the views of different types of scientists or from historical and geographical variations in the views of industrial scientists. More fundamentally, the conflict may arise from differing conceptions of the nature of academic training, or from the differing theoretical orientations of the studies. These explanations will be examined in turn.

SAMPLES

First, the problem of samples. Most studies of scientists in industry have included both B.Sc. and Ph.D. scientists indisriminately. There are reasons for believing that the two groups may differ considerably in their values and orientations and should therefore be considered separately.

It is plausible to suggest that the graduate student undergoes a very different educational experience in his research years from that of the undergraduate. An undergraduate science degree course typically involves long hours in the laboratory and lecture hall learning a body of textbook knowledge and the practical skills associated with it.[46] There may rarely be the opportunity to get

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[46] T.S. Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research", in C.W. Taylor and F. Barron, eds., Scientific Creativity (New York, Wiley, 1963).

first-hand experience of research or the necessity to consult original source material. Close contact of an individual kind with teachers may be minimal.[47] The graduate student, by contrast, will perhaps for the first time come into contact with the more contentious areas of his subject. He will be a novitiate in the way science is really done. Moreover, he will often be initiated into the practices of the trade via close contacts with university staff members.

If this picture is correct, then it could be argued that the conditions for successful academic socialisation are not present for the undergraduate, but only occur when the student begins to do research himself and have more contact with his supervisors. This rather different experience could well leave him with strong and persistent commitments to the scientific ethos. Thus Barnes' study which was confined to B.Sc. graduates, would not have found evidence of the effects of academic socialisation, because they would be only weak and diffuse at that stage. On the other hand, studies which fail to discriminate between Ph.D. and B.Sc. scientists would find evidence of value conflicts, but this would be because of the Ph.D. contingent in their samples. Here then are variables which have been confounded and which need to be controlled to resolve the conflicting accounts.

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[47] Barnes, Making Out, p. 174.

HISTORICAL CHANGES

A second possible source of the divergence is that there have been genuine historical changes in the attitudes of industrial scientists. Some commentators have suggested that major structural changes in the institution of science could have profound effects on the beliefs and attitudes scientists hold. For example, Krohn has argued that the increasing location of science in government and industry is transforming the way research is organized and the social role of the scientist.[48] The values of industrial scientists may, in the past have reflected those of their more prestigious counterparts in universities. However, if this situation changed significantly between the early and late 1960s, it could account for the discrepancy between most of the reported findings.

To assess this argument in general terms, it is worth asking if the traditional values of science have ever been prominent amongst industrial scientists. Ellis has looked at the founding of some of the professional associations of industrial scientists, in particular, the Royal Institute of Chemistry, which appeared in the late 1870s.[49] He argues that the origin and development of such bodies has been closely associated with the central concern of their members, which is and always has been the commercial application of scientific knowledge.

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[48] R.G. Krohn, "The Institutional Location of the Scientist and his Values", IRE Transactions on Engineering Management, 8 (1961a), pp. 133-9.

[49] Ellis, The Scientific Worker, ch. 5.

> They have accepted uncritically the fact that most of their members are employees, and that many of them are engaged upon applied research and development work. This is an economic reality which is never questioned; it is taken as a 'given', and the activities and purposes of these institutions start from this premise. Their aim is to provide a variety of services to practitioners: teachers, applied researchers, consultants, industrial managers, etc.[50]

Consequently, the ethos of such associations closely reflects the beliefs and values of the industrial scientists whom they represent. These include a heavy emphasis on service and respect for the interests of employers and are, in general, the very antithesis of the ethos of pure science that Merton has identified. They are, in fact, much more like the attitudes he associates with the role of technician.

Ellis' argument suggests that, far from industrial values being a recent acquisition for scientists outside universities, they are a long-standing tradition. It is therefore unlikely that the historical situation has changed so rapidly and markedly that it accounts for the divergent findings.

This suggestion can be reinforced from another direction. Not only does it seem that the industrial scientist may always have had his own appropriate scheme of values; it is also possible that this did not require a change on the part of any scientist moving from university into industry — the reason being that even academic scientists may not have been attached to Merton's norms.

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[50] Ibid. p. 173.

THE VALUES OF ACADEMIC SCIENTISTS

West interviewed 57 academic researchers to see how far the values traditionally associated with scientific research continued to be held.[51] He received a variety of responses with many deviations from the orthodox values. He finally attempted to see whether a scientist's ideology was related to his research behaviour, and he concluded that the classical morality of science was "only fortuitously associated with productive research".

West's evidence relates to another factor which complicates the issue. Most of the work reporting value conflict has been American, whilst the studies of Box and Cotgrove, Ellis and Barnes were carried out in British industry. The relation between the ethos of science and the ethos of industry may be different in the two countries. Although more comparative evidence would be needed to evaluate such a possibility, West's findings suggest that the American academic ethos is not antagonistic to industry for the reasons advanced by the value conflict theorists.

Several other writers have also focused on academic scientists. Their concern, in particular, has been to examine the adequacy of Merton's ethos of science as a description of the normative constraints upon scientists working in a university context. They argue that Merton has failed to identify a constant, specific, overriding normative structure within which the activity

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[51] S.S. West, "The Ideology of Academic Scientists", IRE Transactions on Engineering Management, EM-7 (1960), pp. 54-62.

of 'pure science' occurs. In forming an alternative account, they take their lead from the work of Thomas Kuhn.[52]

According to Kuhn, science is not a homogeneous, undifferentiated institution, but is made up of many widely differing groups, each committed to a "paradigm" — or exemplary scientific achievement or style. Each paradigm entails norms of a technical or cognitive nature. It is commitment to these norms which gives scientists their cohesion and consensus, not commitment to an overarching value system common to all scientists.

Mulkay[53] and Barnes and Dolby[54] have developed Kuhn's work as an alternative to Merton. Their claims can be summarised in three arguments.

(1) Barnes and Dolby distinguish between professed norms and norms actually observable as patterns of behaviour, claiming that it is the latter which are crucial to an understanding of science. Merton's evidence and examples merely draw attention to professed norms, and whereas these might coincide with behavioural norms, it is more likely that they will be uttered under particular circumstances, as justifications, rationalisations or celebrations.

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[52]Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago, University of Chicago Press, 1962).

[53]Michael Mulkay, "Some Aspects of Cultural Growth in the Natural Sciences", Social Research, 36, no. 1 (1969), pp. 22-52.

[54]S.B. Barnes and R.G.A. Dolby, "The Scientific Ethos: A deviant viewpoint", Archiv. europ. sociol., Xl (1970), pp. 3-25.

(2) If behaviour is examined, then there is mounting evidence, both contemporary and historical, of a considerable lack of conformity to Merton's norms of science. Mulkay has analysed the reaction of the scientific community to Velikovsky's work as "the most massive case of theoretical, methodological and 'social' non-conformity in the recent history of science".[55] He suggests that the violent reaction which met Velikovsky's thesis in the scientific community was due to its departure from the accepted paradigms of the time. "These paradigms act as norms. Not only do they supply cognitive and perceptual frameworks, they also provide standards for judging the acceptability of hypotheses."[56] Certainly, as Mulkay points out, there was a mild counter-reaction in which the norm of disinterestedness was invoked against Velikovsky's detractors. However what was noticeable was:

> . . . not this mild and uncertain reaction against the widespread failure within the scientific community to conform to the Mertonian norms but the persistent tendency of scientists both in the United States and elsewhere, through the medium of published reviews as well as personal contacts, to justify rejection of Velikovsky's claims simply by indicating the latter's departure from established beliefs.[57]

(3) Finally, Merton has been criticised for the claim that the norms he has delineated are peculiar to science and identifiable

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[55]Velikovsky proposed a revolutionary theory about the history of our solar system, which postulated radical instability and collision processes. Conventional theories had all stressed the historical uniformity of the solar system and the absence of catastrophic processes. Velikovsky also used controversial sources of evidence, such as myths. He did however, make a prediction about the atmosphere of Venus which, embarrassingly,came out right. See Alfred de Grazia et al., eds., The Velikovsky Affair (New York, University Books, 1966).

[56]Mulkay, Cultural Growth, p. 32. [57]Ibid. p. 35.

independently of any scientific investigation. In their paper, Barnes and Dolby argue that universalism and organized scepticism (and rationality) are such general norms that they could not be of any practical use to a scientist in deciding how to behave in any particular situation. Furthermore, they do not even discriminate anything peculiar to academic pursuits in general, let alone science in particular. Kuhn's approach, on the other hand, by identifying groups of scientists, sharing commitment to a paradigm, can show why a particular scientist would take a certain course of action, and which situations would be likely to provoke normative conflicts for him.

The contention of these writers is that the best way to understand how science works is to concentrate on precisely those factors which Merton eschewed. These are the normative elements in the theories, methodologies and techniques that scientists use in the course of doing their scientific work.

In fact, no attempt has been made to extend such a perspective to industrial scientists, nor to develop its implicat-ions concerning the relationship between academic training and industrial work. These points will however be returned to later in the next chapter.

DIFFERING THEORETICAL ORIENTATIONS

A fourth possible explanation for the divergent conclusions of the studies lies in the different theoretical frameworks within which they were conducted. These would naturally lead researchers to ask different questions and to put different constructions on their empirical findings. That the different theoretical perspectives are significant in this way is strongly suggested by the fact that there is a larger measure of factual agreement than might be expected from the divergent conclusions drawn. These theoretical issues will now be examined in more detail.

CHAPTER 11

THEORETICAL ISSUES

THE VALUE CONFLICT THEORISTS' MODEL OF SOCIALISATION

Merton , his followers and his modifiers all base their accounts of scientists on a similar model of socialisation. This is not explicitly formulated by them, but can be inferred from their work. Since Merton himself, along with Box, Cotgrove and Ford provide the clearest statements of the position, the discussion will be concentrated on their contributions.

Merton's functionalist view of society entails that people become moulded in the shape that society needs. Social institutions are seen as providing institutional goals, which can be reached by socially prescribed means. Socialisation is the link between individuals and the social structure — it is the mechanism by which people are trained to do what is required of them for these goals to be achieved. On this account, social control, or institutional pressures upon people to conform, occur mainly at the normative level. People are not coerced, but do what is required of them because they have learnt to believe that it is right and good. Society's requirements are introjected into individuals and form their consciences or superegos.

The norms of science (the institutionally prescribed means for achieving the institutional goals of science) are said by Merton to be "transmitted by precept and example", and "in varying degrees internalised". The mechanisms of such a process are not examined, but it is possible that Merton based his assumptions about socialisation on Parson's work. Parsons describes two mechanisms — imitation and identification.[1] Imitation is involved when specific elements of culture are being learned, such as specific pieces of knowledge or technical skills. It "assumes only that alter provides a model for the specific pattern learned without being an object for a generalized cathectic attachment". Identification, on the other hand, is much more important in connection with learning more general patterns of orientation, such as standards of taste, philosophical or ethical outlooks or patterns of value orientation. The patterns are learned because of an emotional (cathectic) attachment to the person from whom the learning is being done. They will act as a model, "not only with respect to a specific pattern in a specific context of learning but also as a model in a generalized sense".

Identification is the mechanism of most relevance to Merton's model of socialisation, because, according to his theory, what are being learned are values, attitudes and sentiments, that is, generalized patterns of orientation. The scientists in Merton's account may be imitating specific pieces of knowledge, but more important in terms of their scientific socialisation will be their identification with academic staff and heroes of science both past and present.

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[1]Talcott Parsons and Edward A. Shils, eds., Towards a General Theory of Action (New York, Harper Torchbook, 1962), pp. 128-31.

If Merton's notion of socialisation is based on Parsons', then there are, nevertheless, certain significant differences. In Merton's imagery, institutions frequently exert heavy pressures on individuals towards conformity or nonconformity. For example, a constant theme in Merton's essays on bureaucracy are the heavy pressures organisations exert on their officials to attain "a high degree of reliability of behaviour, an unusual degree of conformity with prescribed patterns of action".[2] The overwhelming impression given by this essay is that bureaucrats identify with superior officials in an organisation, in much the same way as ducklings are said to imprint on their mothers. Thus:

> Discipline can be effective only if the ideal patterns are buttressed by strong sentiments which entail devotion to one's duties, a keen sense of the limitations of one's authority and competence, and methodical performance of routine activities. The efficacy of social structure depends ultimately on infusing group participants with appropriate attitudes and sentiments.[3]

Merton goes on to describe how a bureaucratic official may often respond to the pressures on him with more intense conformity than is technically necessary (i.e. for the smooth operation of the bureaucracy). Thus a bureaucracy is constantly at risk of producing "trained incapacity" in its officials. This is where "actions based upon trainings and skills which have been successfully applied in the past may result in inappropriate responses under changed conditions. An inadequate flexibility in the application of skills, will, in a changing milieu, result in more or less serious maladjustments".[4]

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[2] Social Theory and Social Structure, p. 198.

[3] Ibid. [4] Ibid.

A similar example of an overconforming response to the pressure of the organisation is the process of "displacement of goals". This is where the pressure on the bureaucrat to conform to the goals of the organisation leads him to adhere too rigidly to the particular details required by the rules. Thus the means become displaced and taken for the ends — "an instrumental value becomes a terminal value".

The discrepancy between the strength of attachment people may have to means and ends is a recurrent theme in Merton's work. Indeed his whole discussion and explanation of deviant behaviour is based upon it. There, an intense attachment to the cultural goals is postulated in cases where the institutional means for achieving those goals is absent — hence deviant means are adopted.

What is interesting about all these examples is the heavy emphasis on attachment and identification. People become, so to speak, imprinted in the mould of their society (or a particular sub-section of it). The way the process works is illustrated by the example of conditioning which Merton uses in his discussion of trained incapacity: ". . . chickens may be readily conditioned to interpret the sound of a bell as a signal for food. The same bell may now be used to summon the trained chickens to their doom as they assemble to suffer decapitation".[5] This clue and his more implicit usages suggest that Merton is operating with a fairly crude behaviourist model of socialisation. Values are seen as conditioned responses.

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[5] Ibid.

The fact that behaviourist models of socialisation do not have to be so crude is suggested by Parsons' treatment of socialisation, which is, by contrast, very sophisticated. If Merton did base his ideas on those of his teacher, then he omitted certain elements emphasised by Parsons. Most prominent amongst these are possibilities for innovation, choice and active manipulation. These possibilities occur because, in Parsons' view, a straightforward conforming response to social pressure is impossible. Social situations will always be more complex in scope, and the values in them richer, than can be incorporated in the response of a single individual. Even if there is a dominant value system or ethos, there will always be many lesser value systems alongside. Thus an individual will, of necessity, always be faced with choices between value elements.

> The culture of a personality, so far as it is more than a microcosm of a set of generalized patterns, is a particularized version, selected from a more comprehensive total pattern. Adding usually something of its own through interpretation and adaptation, it consists of the elements which are relevant and congenial to the particular actor in the light of his particular situations.[6]

In Parsons' scheme then, there is much more emphasis on cognitive elements — people can absorb information, process it, appraise situations, make choices and decide whether or not to act in a particular way. The link between stimulus and response is attenuated and by no means automatic or limited to one direction. In Merton's model, on the other hand, the individual has little choice but to respond and be buffetted back and forth by whatever stimuli happen to impinge upon him. In most cases, he will not be as battered as this image suggests, because institutional value

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[6] Ibid. p. 180.

systems are seen as sufficiently monolithic to exert only one consistent pressure. Conflicts in values are only likely to arise if the individual changes his circumstances or moves into a different situation. Then the tenacity of his habits of thought and his commitment to the values and sentiments into which he has earlier been socialised (conditioned) will become manifest. If they are different from those now required, value conflicts will be provoked.

What has become of Merton's value-conditioning model in the hands of those who have modified his theory ? Avery, Marcson and Abrahamson suggested that the conflict might be moderated by various processes of accommodation and experienced in varying degrees of intensity. The scientist in industry is seen as undergoing a process of secondary socialisation into new norms and values. Some scientists may be resistant to this subsequent socialisation, remain committed to the norms and values of science and therefore experience value conflicts. Others will succumb to the socialising pressures and thereby become "accommodated" to the industrial environment. But this is only to say that new conditionong processes can overlay old ones.

The other modification to Merton's theory was Cotgrove, Box and Ford's discussion of differential socialisation. Their central assumption is that value orientations (or ego-identities), once acquired, have stability and enduring potency. Therefore, although value conflict in industry is seen as being moderated by occupational selection, nevertheless, the scientist who finds himself in industry, is assumed to retain that commitment. Admittedly, Box and Cotgrove raise the possibility that scientists may undergo a process of resocialisation,[7] but their dominant emphasis is on the enduring effects of differentials in initial socialisation.

Cotgrove, Box and Ford's theory of 'identity crisis' may seem very different from the idea that values are conditioned responses, but what it adds is little more than an account of differential susceptibility to conditioning. Freudian identification, the ethologists' process of imprinting, Pavlovian conditioning and the anxious choice of identity all have one thing in common: they are inflexible bonds which fix people's consciousnesses in a certain stable emotional state.

Nevertheless, Cotgrove, Box and Ford's theiry is sufficiently different to raise some theoretical problems which are special to it and these will now be briefly discussed. First, it is not enough to assert that a working class student, by virtue of being in a middle class environment (the university) will thus be in a marginal situation. It is necessary to know how this 'middle class-ness' would impinge upon him. In some university science departments, working class students outnumber their middle class contemporaries. In addition, a science student's work confines him to the laboratory for a large part of the time and may give few opportunities to mingle with the more middle class students in other faculties. There is thus reason to wonder if the working class student will ever be confronted with his marginality, even if, in terms of the overall university population, he is marginal.

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[7] Science, Industry and Society, p. 124.

Box and Ford also claim that the image of a scientist tends to be a classless one, and "hence it can be embraced by a working class student without involving denial of his biographical self".[8] This may be the case, but no evidence is given. There seem to be as good grounds for suggesting the contrary. It is at least arguable that any role involving the production of knowledge for its own sake, rather than for recognisable practical ends, might be uncongenial to a working class student. Thus the role of industrial scientist, technologist or engineer might seem a more appropriate "bridging culture" for a working class student than the identity of academic scientist. The latter may be more closely associated with the conspicuous leisure of the middle or upper classes than the productive toil of the working classes.

To sum up: the major emphasis in all the accounts just discussed is on the internalisation of norms and values and on their stability and continuing potency over time and in different situations. Although slightly different languages are used, their fundamental ideas about socialisation and the learning processes that underly it seem to be substantially the same. They may be said to have value-conditioning presuppositions and they therefore lack any explicit emphasis on cognitive processes. Furthermore, abstract values, norms or identities are held to be the important determinants of behaviour, so the theories in no way engage with the constraints of any concrete situations.

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[8] Commitment to Science, p. 230.

Some alternative theoretical orientations will now be examined.

BECKER'S MODEL OF SOCIALISATION

Becker presents an approach which does not seek explanations of change or stability in terms of people's values, but looks to the effects of social situations on the structuring of experience. The process of situational adjustment, he argues, accounts for changes in people in adulthood.

> The person as he moves in and out of a variety of social situations, learns the requirements of continuing in each situation and of success in it. If he has a strong desire to continue, the ability to assess accurately what is required, and can deliver the required performance, the individual turns into the kind of person the situation requires.[9]

However, people also develop consistency as they move from situation to situation — they are not infinitely flexible. Becker accounts for this consistency in terms of the process of 'commitment'. This, "consists in the linking of previously extraneous and irrelevent lines of action and sets of reward to a particular line of action under study".[10] Becker describes this process as the making of side-bets. "A person may make side-bets producing commitment consciously and deliberately or he may have them made for him almost without his knowledge, becoming aware that he is committed only when he faces a difficult decision".[11] For instance, choosing a job or

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[9] Personal Change, p. 44. [10] Ibid. p. 50.

[11] "Notes on the Concept of Commitment", AJS, 66 (July 1960), pp. 32-40.

starting a family are all events which produce lasting commitments and constrain the person's behaviour in the future, affecting his adjustment to future changed situations.

It should be clear how well Becker's approach accords with Barnes' findings. Rather than looking for a set of values that students may be initiated into from generation to generation, the focus is on the concrete features of the situation each person finds himself in. Becker's approach demands a knowledge of the structural characteristics of the situation the scientist is in, as well as such information as: does the scientist want to continue working for that organisation ? How well does he want to do ? And finally, what other commitments has he made which may hinder or influence his adjustment ?

Becker's approach neatly incorporates the scheming and manipulating side of man that is wholly lacking in the value conflict theories. In the latter, people are seen as responding much more mechanically and passively to the expectations of significant others. Furthermore, Becker's approach enables people's statements, including the values they express, to be looked at as resources to be used in accounting for themselves and as counters to bargain with. Such an approach is also entirely compatible with Ellis' and West's data and would add bite to Ellis' rather passive 'emanationist' view that his scientists' attitudes reflected their situations.

There are problems however. One important problem concerns the background knowledge that is required to apply the approach.

The guiding assumption is that, given the necessary competence and ability to assess what is required of him, a person will adjust to a situation, if he wants to continue in it and has no other commitments which may hinder his adjustment. These qualifications have a large potential for mischief. What will determine whether people do want to continue in situations ? If the answer is given in terms of what people find rewarding, then the question arises: what do people find rewarding ? For example, what sort of job would a scientist find most rewarding ? Would he be looking for high pay, intrinsic work satisfaction, good career prospects or an opportunity to contribute to pure science ? Such questions are an important part of what is being investigated and their answers cannot simply be presupposed. Similarly, what would count as a commitment ? For instance, suppose a scientist who seemed to be strongly committed to Merton's norms took a job in industry and adjusted completely to its demands. It could be said that he had not really been committed to the norms and that was why he was able to make situational adjustments. If, on the other hand, he failed to make the necessary adjustments but experienced "value conflicts", it could be said that his adjustment was hindered by prior commitments. Here the theory seems capable of explaining everything and predicting nothing. How is it possible to tell when a commitment is going to be enduring in its effects — in other words, that it really is a commitment ? Also, what are the factors that would lie behind one scientist acquiring such commitments, while another did not ? The danger with Becker's theory of situational adjustment is that its escape clauses empty the theory of its content. The possibilities that are offered with the notion of adjustment are likely to be retracted with the notion of commitment.

Another difficulty lies with the theory's central economic assumptions. Seeing people as maximising their advantages is a useful corrective to seeing them as rigidly moulded by their socialisation. However, such a view can also lead to atomisation. People do not 'make out' in a vacuum, but have to use culturally defined rules. Although the particular complex of values people have as resources to exploit in a situation may not be of interest to a particular investigator, nevertheless, the fact that they do use one complex of values rather than another in a particular set of circumstances is surely significant. Insofar as the constellation of norms and values in any situation is not random, then considerations of normative patterning deserve to be taken into account. How such norms are institutionally patterned is still a legitimate and important focus of interest.

The use Barnes makes of Becker's theory suggests that it is a useful alternative to Merton, but its limitations make it less than a complete alternative. Basically, it is a theory about how individuals relate to their circumstances — they adjust or 'make out'. Thus any description of the overall social situation or institutional structure occurs by default. It has to be inferred from what is said about individual acts of adjustment. Systematic patterns may emerge of the problems and possibilities individuals encounter in different institutions, but how institutions, as such, work remains unilluminated. The result is that one of the important and central features of Merton's account — his characterisation of of the institution of science and its workings — has been lost.

In principle, a Mertonian account of the institution of science could be combined with a Beckerian model of the individual scientist and his adjustment in moving from university into industry. Merton's account would probably have assumed this form had his analysis followed the same route as that of the bureaucratic intellectual. However, on that theory, the intellectual adopts a detached 'technician's' ideology precisely to mitigate or to avoid a value conflict. The idea that scientists do not acquire such abstract values at all at university has already been introduced (pp. 31-4 above). If the alternative picture of science offered in Kuhn's work is right, the avoiding action involved in taking up the technician's role would be unnecessary: scientists do not hold abstract values of the sort to provoke value conflicts. If Kuhn's view of the institution of science is accepted, what would be its implications for the scientist who goes into industry ?

THE APPROACH FROM KUHN

One of the most important elements in Kuhn's account of the scientist is his stress on skills and competence. Above all, the scientist is trained as a puzzle-solver. The nature of normal science is such that it does not produce major novelties, but makes small additions to the scope and precision with which a paradigm can be applied. Thus the fascination of a normal research problem lies not in its outcome, which can usually be anticipated, but in the way of achieving that outcome. "Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual and mathematical puzzles."[12] It is skill in the solution of these puzzles that is inculcated in a scientific education: also a deep commitment to the particular way of seeing the world and practising science supplied by the paradigm. This commitment is itself constitutive of research. This is because the paradigm provides the scientist —

> . . . with the rules of the game, describes the pieces with which it must be played, and indicates the nature of the required outcome. His task is to manipulate those pieces within the rules in such a way that the required outcome is produced. If he fails, as most scientists do in at least their first attacks on any given problem, that failure speaks only of his lack of skill. It cannot call into question the rules which the paradigm has supplied, for without those rules, there would have been no problem with which to wrestle in the first place. No wonder, then, that the problems (or puzzles) which the practitioner of a mature science undertakes presuppose a deep commitment to a paradigm. And how fortunate it is that the commitment is not lightly given up. Experience shows that, in almost all cases, the reiterated efforts of the individual or of the professional group, do at last succeed in producing within the paradigm a solution to even the most stubborn problems.[13]

The Kuhnian scientist thus has strong commitments, but they are of this specific cognitive and technical kind. Therefore they are unlikely to provoke conflicts for him except in times of paradigm change. There seems no reason to suppose that the industrial scientist should be any different. Unless industry endorses different practices from those of the paradigm in which the

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[12] The Structure of Scientific Revolutions, p. 36.

[13] Thomas S. Kuhn, "Scientific Paradigms", p. 96, in Barry Barnes, ed., Sociology of Science.

scientist has been initiated, then the scientist's skills and commitments would be likely to be quite compatible with the demands that would be made of him in an industrial job.

Kuhn's theory has been distinguished from Merton's in terms of the contents of the norms and values transmitted. It can also be distinguished in terms of the underlying learning process envisaged. Scientists learn, Kuhn argues, "by finger exercises or by doing",[14] they:

> . . . never learn concepts, laws and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications. A new theory is always announced together with applications to some concrete range of natural phenomena; without them it would not even be a candidate for acceptance. After it has been accepted, these same applications or others accompany the theory into textbooks from which the future practitioner will learn his trade. They are not there merely as embroidery or even as documentation. On the contrary, the process of learning a theory depends upon the study of applications, including practice problem-solving both with a pencil and paper and with instruments in the laboratory.[15]

Kuhn then goes on to describe how this concretely located process of learning involves the successive application of old models of problem solving to new more complex and less completely precedented problems. The point is that scientists do not work with explicit or even discoverable rules: they work by generalising from particular models or exemplars. Thus Kuhn says:

> Though many scientists may talk easily and well about the particular individual hypotheses that underlie a concrete piece of current research, they are little better than laymen at characterizing the established bases of their field, its

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[14] <u>The Structure of Scientific Revolutions</u>, p. 47. [15] <u>Ibid</u>. p. 46.

legitimate problems and methods. If they have learned such abstractions at all, they show it mainly through their ability to do successful research. That ability can, however, be understood without recourse to hypothetical rules of the game.[16]

The process of learning being envisaged is thus one of constant and dogged imitation and practice. There is no easy route to success and no set of rules which can be abstracted and codified. If the scientist of Merton's theory is an idealist, motivated by his identification with certain abstract values, the scientist of Kuhn's theory, by contrast, is like a craftsman who belongs to a guild. He has served a long and demanding apprenticeship during which he has thoroughly mastered certain skills and practices. It might be assumed further that these will be recognised on the open market and tend to attract a fairly constant price. Thus as an indentured craftsman, the scientist will be able to take his skills to a variety of locations and will have confidence in his ability to practise his trade competently. Employers will know that a highly qualified scientist is what they rquire for the job and will consequently receive him appropriately.

To suggest that a scientist is like a craftsman is not to say that he will necessarily be satisfied in an industrial job. The point is that his scientific training, as such, is unlikely to leave him with attitudes and values that are antagonistic to industry. It will certainly leave him with cognitive and technical commitments and these may conflict with what is required of him in industry. Such conflicts will not be expressed in general terms, but will arise in connection

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[16] Ibid. p. 47.

only with the actual contents of his work. Otherwise, such general values as he does have will be shared with other members of society. He may, for example, be associated with a group negotiating more favourable wage levels, or object to private industry on the grounds of socialist principles, or be unwilling to work for some firms because of their pollution policies. Interests or values of this kind are widely distributed throughout society and are unlikely to be held simply by virtue of the fact that a man is a scientist.

KUHN AND BECKER COMPARED

So far, Kuhn's theory has been contrasted with Merton's. How does it relate to Becker's approach ? Like Merton, but unlike Becker, Kuhn offers an overall account of the institution of science. But both Becker and Kuhn emphasise the importance of concrete situations rather than abstract values as determinants of behaviour. Science for them is not to be seen as a set of institutionalised values, but as a body of resources. Society impinges on the individual cognitively rather than emotionally. There are differences of emphasis between the two approaches however. One way of conveying the difference is by taking up again the analogy used in the previous section. While the Kuhnian craftsman is motivated to a large extent by intrinsic work involvement — the fascination of problem solving — Becker's conception of man is like an entrepreneur engaged in calculations designed to maximise marginal advantages.

Does this difference amount to anything more than a difference in 'tone': does it lead to any differing predictions ?

Becker's theory would predict that scientists would adjust to industry providing that they could 'make out' and use their qualifications for personal advancement. Where dissatisfaction and conflict is found, this would result from blocked opportunities for advancement. Kuhn's theory would also predict adjustment providing the scientist could practise his trade satisfactorily. The most likely source of conflict would be at the cognitive and technical level. The scientist's ambitions concerning pay and promotion would be closely linked to estimates of the 'normal', the 'reasonable' and the 'going' rate for the job. Aggressive attempts at 'making out' would be uncalled for, since the scientist's skills and competence would constitute a recognised commodity and be rewarded accordingly. The major empirical difference that would be expected is that the Beckerian scientist would be less concerned with the intrinsic satisfaction of work and more concerned with using his puzzle-solving skills as a major bargaining counter in his attempts to 'make out'.

This is the nearest that can be got to a direct conflict of predictions. Even here the issue is complicated by the fact that either theory could be cast into a form which would accommodate the rival predictions. Perhaps it is to misconstrue the two theories to look for a single piece of evidence which would corroborate the one while refuting the other. After all, one theory is primarily an overall description of science, whilst the other is about the tactics and choices of individuals. Kuhn describes the institution of science in the sense that he offers a general characterisation of it: he answers the question, what is science ? This helps to show what can be taken-for-granted by those who are scientists and, by implication, those who employ them. However, if this means that the two theories cannot decisively be tested one against another, it does not mean that they will equally illuminate any given findings. This will depend on the sheer prominence of individual efforts to 'make out' as compared to the exercise of competence and skills in taken-for-granted settings.

In the rest of this thesis, the results of an investigation of a sample of Ph.D. scientists who went into industry will be presented. First, the evidence for Merton's theory will be examined. This theory makes the clear prediction for Ph.D. scientists that, having had extensive socialisation into the ethos of science, they will tend both to be attached to it and in conflict with the values of industry. At the very least, this theory requires evidence that the norms of science are accepted by scientists while in academic life, even if a 'technician's' role is adopted subsequently to evade value conflicts. In the event of the predictions from Merton's theory failing, then the utility and value of the other two theories will be balanced against the evidence.

CHAPTER 111

METHODS

The study was designed to follow a cohort of scientists from their final-year as Ph.D. students (stage one) through to their first industrial jobs as graduates with doctorates (stage two).

Initially the hope had been to carry out intensive, loosely structured interviews with the scientists at both stages. However, sampling problems forced a compromise. The proportion of Ph.D. scientists who go into industry has typically been rather small — about fifteen per cent. So, to get an industrial sample of, say, fifty scientists, the initial university sample would have to be about four hundred. Since detailed interviews with this number of people was out of the question, an alternative strategy was necessary. The solution adopted was to gather the first stage information via a postal survey. A subsample of the people who returned questionnaires were also interviewed in order to provide a check on the survey material. The second stage of the study was carried out as originally planned by means of interviews.

RESEARCH PROCEDURES

<u>The Sample</u>. The sample was acquired by writing to the heads of various university departments of physics and chemistry with a brief outline of my research plans and a request for a list of the names of their final-year Ph.D. students. A copy of the letter is included in the appendix. The departments were restricted to physics and chemistry for simplicity, and were chosen from every main type of university — large civic, small civic, new, Welsh, Scottish, technological and Oxbridge. The exact procedure will not be elaborated here, since it would endanger the anonymity of the departments concerned. Heads of 49 different departments were approached and 34 very kindly sent me lists of names. The resulting sample size was 596.

<u>The First Stage Survey</u>. Questionnaires were sent to these 596 scientists early in March 1972. A specimen of the questionnaire is included in the appendix. The main presentation and analysis of the survey results occurs in Chapter Vl where specific problems of interpretation are discussed in context.

357 scientists returned completed questionnaires, making a response rate of 60 per cent. One reminder was sent out, but available funds did not permit a second.

In addition, a subsample of 25 (plus four foreign) scientists were interviewed between July and August 1972 in six different university departments. The interviews were loosely structured, their aim being to accumulate evidence on the scientists' attitudes and research training in their own terms. These interviews will also be discussed in Chapter Vl.

Second Stage of the Study. Of the original 357 people who returned questionnaires, 48 foreigners were excluded. This was because some of the material from the first stage of the study (the interviews conducted with the four foreign students and various comments made on the questionnaires) suggested that foreign science students might have rather different preoccupations and values from their British counterparts. For example, students from developing countries put particular stress on service to their home country. Although such differences are an interesting and important topic for investigation, they were not the object of my study. It therefore seemed better to avoid the possible complicating effects that the inclusion of such data might involve.

The remaining 309 people formed the sample for the second stage of the study. The final item on the questionnaire had been a request for a forwarding address. In May of the following year (1973) all the scientists who had given forwarding addresses in Britain were contacted again and asked for information about their jobs. A copy of the letter and enclosed reply form is included in the appendix. Twenty one were not contacted, either because they left no forwarding address, or my letter to them was returned by the G.P.O. Of the remaining 288 people, 252 replied, making a response rate of 88 per cent of contacts. These 252 scientists had obtained jobs as in Table 58. (This and all following tables bear the number of the page on which they appear.)

TABLE 58

FIRST EMPLOYMENT OF THE PH.D. SCIENTISTS

Industry	62	(25%)
British Universities	73	(29%)
Universities Abroad	21	(8%)
International Research Institutes	5	(2%)
Civil Service or Government Research Establishments	18	(7%)
Teacher / Teacher Training	33	(13%)
Other	18	(7%)
Unemployed (Ph.D. finished)	6	(3%)
Ph.D. not finished — some may have been finishing, some looking for jobs	16	(6%)
TOTAL :	252	

The next step in the inquiry was to try to arrange interviews with the scientists who had taken industrial jobs. The arrangements were made between August and October 1973 and it eventually proved possible to interview forty such people. The other twenty two with industrial jobs were not interviewed for various reasons. Eight failed to reply to my request for an interview and four refused. The remaining ten people were willing to be interviewed, but this subsequently proved impossible to arrange, largely because of financial constraints. (If more funds had been available, an attempt would also have been made to follow up and interview the scientists who took jobs in the civil service or in government research establishments.)

The Second Stage Interviews. These interviews with the Ph.D. graduates who had gone into industry took place between the beginning of October and the middle of December 1973. The scientists were in a wide variety of jobs, in a wide range of industries all over Britain. They had been working, on average, for about a year. Some of the interviews took place in working hours at their places of work, but most of them were carried out in the evening at their homes. The interviews were semi-structured and tended to be fairly protracted, lasting up to about four hours on occasions and only rarely taking less than two hours. All the interviews were tape-recorded (the scientists' permission having first been obtained) and were subsequently transcribed verbatim.

Specific problems of analysing the interviews will be discussed along with the presentation of results. There were, however, two recurring problems affecting both my interviewing strategy and the presentation of results which will be introduced here. The first arises from the conflict between the rules of standardised interviewing and the rules of normal conversation.

This dilemma was experienced acutely during the second stage interviews and the result was that, although the same topics were covered with everybody, the same questions were not always asked, nor always asked in the same form. The consequences are most manifest in Chapter lV where the data presented in tabular form is sometimes incomplete. Specific explanations are given for such omissions where they occur.

The second problem concerns presentation of results. The way the scientists discussed their jobs and thought about them, formed such a unity that separating out the elements for the purposes of exposition was bound to involve some distortion. Again, points of view which came over strongly in an interview taken as a whole are difficult to epitomise in a brief quotation. These facts have tantalising consequences: often my claims are not as well supported as they would be if the interviews could be given in full. This is impossible because of the sheer volume of the interviews — in total they amount to 1,339 pages of transcribed conversation. Naturally, illustrations have been selected because they are judged to be typical, but lacking the original interviews for comparison, there is no way the reader can assess typicality. However, the illustrations will be offered in conjunction with tables indicating the number of times similar responses occurred. Although the tables precede the interviews, obviously the meanings of the categories and groupings used in them will only become clear in the light of the illustrations themselves.

The results will be analysed in the following order: in the next chapter, CHAPTER lV, the second stage interviews will be used to see whether the scientists were indeed attached to Merton's norms of science and did experience value conflicts. In CHAPTER V, the narrow hypothesis-testing orientation will be abandonned in favour of a more inductive approach. The same second stage interviews will be used to provide a broader and more positive picture of the industrial scientist. CHAPTER Vl will utilise the first stage questionnaires to relate the attitudes of the scientists after they had arrived in industry to their attitudes whilst still at university. Also offered is a comparison of these eventual industrial scientists with the eventual academic scientists. The conclusions which emerge from the study will be summarised in CHAPTER Vll.

CHAPTER lV

THE SCIENTISTS IN INDUSTRY: THE EVIDENCE FOR ATTACHMENT TO THE ETHOS OF SCIENCE

The evidence from the second stage interviews to be presented in this chapter is concerned with two of the norms of science — autonomy and communality.[1] The scientists were asked about their attitudes to these matters and the way in which their attitudes conflicted or were in accord with the practices in their own work situations. A copy of the checklist of questions which guided the interviews is included in the appendix.

For the purpose of testing the value conflict theory, data will be analysed under the following headings : —

(1) Freedom to choose projects

(2) Freedom to work in own way

(3) Freedom to publish

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[1]This follows the practice of other recent researchers, e.g., Box and Cotgrove, Science, Industry and Society and Barnes, Making Out.

1. FREEDOM TO CHOOSE PROJECTS

The main results are summarised in TABLE 63, which deals with whether the scientists wanted freedom to choose projects and whether or not they had that freedom.

TABLE 63

NUMBER OF SCIENTISTS WANTING AND HAVING FREEDOM TO CHOOSE PROJECTS

		Wants freedom to choose projects				
		Yes	Qualified*	No	No Explicit Information	Total
Has freedom to choose projects	Yes	1	-	-	1	2
	Qualified*	2	16	3	7	28
	No	1	-	5	2	8
	No Explicit Information	-	1	1	-	2
	Total	4	17	9	10	40

*'Qualified' means: yes with reservations, e.g., consultation, discussion etc., or, no with reservations, i.e., some leeway.

The most striking fact about the results in TABLE 63 is the small number of people who said they wanted freedom to choose projects. An even smaller number had less freedom than they wanted (N=3). The majority of people neither wanted nor had complete freedom, but had considerable leeway and could help decide what projects they would do in consultation with other people (the 'Qualified' category). The people on whom information is lacking concerning whether they wanted freedom to choose projects are discussed later (p. 77).

The information in the table will be elaborated by giving selected extracts from the interviews to illustrate the views in the various categories. From the point of view of Merton's theory, the people who did want freedom to choose projects — or at least, wanted more freedom than they had — are the most interesting. The spirit in which they held such views is also important, since there are many reasons someone might want freedom to choose projects other than those in accordance with the ethos of science. The location of the illustrations in TABLE 63 is set out below. Naturally, the scientists have all been given pseudonyms.

TABLE 64

LOCATION OF ILLUSTRATIONS IN TABLE 63

		Wants freedom to choose projects		
		Yes	Qualified	No
Has freedom to choose projects	Yes	Dr. Merchiston	—	—
	Qualified	Dr. Bernard	Dr. Warriston Dr. Mayfield Dr. Carrington	Dr. Lauriston
	No	Dr. Lindoch	—	Dr. Inglis Dr. Hart

ILLUSTRATIONS

Scientists wanting freedom to choose projects

Dr. Merchiston: who both had and wanted freedom to choose projects

Well I think you get the best results when you're not under pressure . . . if you can play around with things, it is surprising what you do get out of it — just playing. That's all it is at work really — just playing. There's certain things that do come out of just messing about. You get ideas . . . yeh you get ideas that come out of this. If you're stuck in a path and told, "you've got to do this", you don't get any chance . . . I don't think you get any chance for outside thinking, right ? You've got a path laid out for you and you've got to follow that path. If you can just play around with things, there's no direct pressure to follow a certain path, then a hell of a lot more comes out of it, I'm sure of it. I mean, it's like old Einstein's work in the patent office when he worked out the theory of relativity. He didn't have any restraints on him at all; he wasn't told what to do; he just started in and sat down and said, "I wonder what would happen if . . . ". It was just a matter of . . . he was just playing essentially and look what came out of it — the theory of relativity. He didn't have anyone saying, "you've got to follow this path and you've got to do this and you've got to do that". I don't think you get anywhere like that. So I think freedom to think how you want to and do what you want to is really important — a hell of a lot comes out of it.

— Would you see a conflict here between what you think would be ideal for the way scientists should work and perhaps the needs, for instance, of industry ?

Yeh, there is a conflict; there has to be a conflict, but I think one way of solving it is what they've done at this place. I think they realise that certainly people who've done a Ph.D. are going to have a lot of independence — are going to be very independent people. So what they do is they say, "well ultimately we've got to design a commercial reactor and we want to know about certain things in this area, that area and that area", and they give someone a tremendous sort of wide brief, so that you've got a fair amount of freedom, you've got a wide brief so that you can range over and look at things within that area. That's one way of soving it and to keep everybody happy — yeh the industry and the scientist.

Dr. Bernard: who wanted freedom to choose projects, but did not have as much as he would have liked

I have to report immediately to my section head and occasionally directly to the head of department. Now he . . . I think the system is really that I'd like to have more freedom in what I do and where I do it and how I do it. We do tend to get told what to do all the time, which isn't very good and when you do start to get out of line, you sometimes get your knuckles rapped.

In fact, Dr. Bernard went on to say that he usually followed his own lines of interest regardless of his section head, because he felt that that was the way a scientist should work. I asked him in what sense he meant the word 'should'.

I think it's the way a scientist should work. If I find an interesting . . . when I say an interesting avenue, it's interesting if it's getting me to answer the question I'm involved with. Obviously I don't do things purely for something to do that'll amuse me for hours and not do the company good at all. If I think there's a possibility of something leading to an answer, then I'll follow that avenue and because I think it's going to lead somewhere, then I shall pursue it with great interest. This is the point . . . I like to . . . whereas a lot of people . . . you know I tend to get down to the basic research attitude inherent [illegible]. [illegible]

we've got to [illegible]

certain things [illegible]

[illegible]

[illegible]

[illegible]

serving [illegible]

[illegible].

[illegible]

[illegible]

[illegible]

again and whereas they would skip over it and say, "oh yes, that's very interesting, but it's not getting us anywhere", I'll say, "ah, but it is, because we might find something at the end of this avenue which explains all".

— So fundamentally you wouldn't see a conflict between you following what you find interesting and the company's interests ?

No, I don't think so. They may not see it that way . . . it would be more interesting to follow your thoughts and I think it would be more beneficial to the firm as well. I think a more fundamental approach would lead to better results in the long term . . . but we're not given the chance to try out any of this really unless you do it like I do and sort of take the consequences afterwards. I mean all the breakthroughs I've had at this firm have been when I've turned a deaf ear, and you sort of progress along your own lines.

Dr. Lindoch: who wanted freedom to choose projects, but was very limited by the set programme. She was thus even more constrained than the previous scientist

— Would you like more choice over what you actually work on ?

I think that would be nice. I mean I was never really asked if I wanted to do this work. I was told, "right, you're doing this", but it's difficult to chop and change between jobs. I mean now I'm set up in this, it's going to take me a couple of years I think . . . before the system . . . I may have some say then in what I do next. I shall certainly endeavour to if I'm still here.

— You say it would be 'nice'. Is it anything more than you think it would be nice; do you think you should have freedom ?

Oh yes, I think I ought . . . I mean they get the best out of you if you're doing something you enjoy, apart from you liking it more. Then in fact if you're doing a job you don't like, they'll probably lose you, because you'll go and get a job somewhere else. It's up to you I think.

— Do you think that scientists in general should have this sort of freedom ?

I think anybody should have some freedom to have a say in what they do.

Scientists with qualified answers about wanting freedom to choose projects

As can be seen from TABLE 63, these people formed the largest single category. Typically such people wanted some freedom to choose projects, but only as long as that was consistent with their firm's interests and concerns. These were almost always given priority and in no case was there any large discrepancy between the amount of freedom the scientist wanted to choose projects and the amount he had.

Dr. Warriston: who was happy with the restricted amount of freedom he had, but was not happy in his job

Should I have more freedom ? No, as far as industry is concerned, I don't think I should have.

— Why as far as industry is concerned ?

Because at the moment I shall get what interest I can get out of the subject with what I'm doing. If I'm given more freedom, I shall move farther away from what they want, because you know I'm genuinely not very interested in it. If I could become more interested in it, more satisfied by it, it'd solve a certain immediate problem anyway of job satisfaction. I'd be less useful to them though . . . I would misuse that freedom because I'm not interested, but other people who are interested would I think get more out of it. It's very difficult talking about freedom in structured confines. You have to have a certain amount of freedom not to feel too blinkered — too steered along a certain course. But then again, people above you, who are project leaders, will have to steer you: they can't allow you too much freedom.

Dr. Mayfield: who was satisfied with the restricted amount of freedom that she had

— Do you feel you have enough freedom ?

For what I want at the moment, yes, because of course doing a Ph.D. you have almost complete freedom to do what the heck you like and I think it's almost a pleasant change to be able to go the other way. I'm not the sort of person who . . . you know obviously I like to have a bit of freedom in what I'm doing, but I like to also feel that if somebody wants to know the answer, then . . . Now, if you've got complete freedom to do just what you feel like doing, the chances are that probably no one is really interested in the answers except perhaps yourself. So at the moment, I'm quite happy with the sort of restrictions that are placed on me; I don't really think that I would ask for more freedom, no.

— Do you think scientists in general should have freedom ?

I think anybody should have it if they want it, but in order to find it, they've probably got to go for academic life. Let's face it, I mean [name of her company] are not going to pay me to do what the heck I like. They're going to pay me to do what they want me to do. O.K., if I like to have a bright idea and say, "I'd like to do this, don't you think it'd be useful ?", then probably they would be quite willing for me to do it and in that respect I have got freedom, but I think if you want complete freedom, you've got to go to the academic type of life in order to have it. The only restriction probably that is placed on people like that is whether they can get the money to buy the apparatus to do the experiment they want to. So I think the answer to this is yes, you should have freedom if you want it, but you've got to accept the fact that people may not be willing to pay you to do whatever you want to do.

— In a sense then, I suppose the answer is 'no', or would you want to say that you think there should be more facilities for you to exercise your right to choose ?

It's pretty hard to say. I don't think . . . you can't justify paying people £60,000 on a piece of apparatus just because they want to do that particular experiment. I think you've got to have some justification behind what you're doing. No, I don't think the answer is no, I don't think anybody should have freedom; I think they can have it, but they should accept the fact that they may not be paid for it. That's the crux of the matter isn't it in most cases.

Dr. Carrington: who was also completely satisfied with his restricted amount of freedom

I've got no complaints about the freedom we've got at work at all. I'm free to do what I please in the work respect, although you wouldn't exercise that freedom. I mean, if I suddenly decided I wanted to do something, then I'd always discuss it with my supervisor, because there's the simple fact that he might be able to point something out that'd say it was a waste of time. If he comes up with constructive arguments against it, well that's fair enough. He's saving your time and saving wasting the firm's, so it's worth discussing with someone. So I've got as much freedom as I want, yeh.

— Do you think you should have freedom and would you object if you didn't ?

Strongly, because I think if you didn't have freedom, I would take it as a slight on your capacity not to misuse it. I'd be very upset if the freedom I've got now was impaired in any way. I'm a qualified, responsible person and therefore I should be allowed it. Obviously, if you misuse it, you should be pulled up.

— But in any case, you wouldn't see any conflict in what you want to do and what the interests of the firm would be, because you would want to comply with them ?

Oh I see — if I wanted to do long-term research or a specific project — wanted to do something that was too long and just didn't have time to do it ? No my loyalties are completely with the firm.

Scientists not wanting freedom to choose projects

Dr. Lauriston: who had some leeway in his choice of projects, but claimed not to want any

I see my function being to carry out what the firm wants me to carry out and if they said, "look, we don't want you to bother with that; we're only interested in this", then fair enough. Perhaps if I wasn't particularly well-paid I might think differently. I might think, hell they're only paying me so-and-so, they can only get so much work out of me, but I'm conscious of the fact that I ought to do what I'm here for and if they say, "O.K., this is what we want you to do", then fair enough. I think that's only fair under any circumstances.

— Would you have any sympathy with scientists who felt they should be more free ?

Um . . . no, I don't think so, especially not nowadays. I might sound rather cynical, but there's so many scientists now that if they can get a reasonable job that's reasonably well-paid, then I think they ought to feel pretty lucky without starting to turn round and say, "well I ought to be able to do this, that and the other". Even in the civil service, there is a big row going on now because scientists are considerably poorer paid than the administrative equivalents, and on top of that, the administrative equivalents have just had another big pay rise — the reason being that they can recruit scientists now ninety to the dozen. So why should they pay them any more ? So I think one has to be a bit careful about expecting freedoms and expecting the sort of privileges that scientists were used to ten years ago and nowadays are not. They're just . . . you know, another job and I think one has to be very careful to guard against this.

<u>Dr. Inglis</u>: <u>who neither had nor wanted freedom to choose projects</u>

— Are you satisfied with the amount of freedom you have in your work ?

Well, as I said, it's just got to be an understanding of the constraints that are put on you. You don't always know the full reason why a decision is made and so you can envisage . . . well not resentment, but you would like to know why a project's been stopped and obviously the further you are up the ladder, the more fully you are in the picture and I'm in a position to be fairly well in the picture — you know, not completely; I can't appreciate all the factors, but I can at least be told what they are. I mean provided you understand why there are constraints on your freedom and are willing to accept their necessity in the environment you're in, then obviously, that curtailment of freedom is reasonable.

— Even though you accept it as reasonable, would you in fact like more ? For instance, would you like more freedom to choose your own projects ?

No. It's hard even to envisage. When I say there are these decision points when it has to be decided whether the thing becomes a drug candidate — whether it's going to be taken beyond certain stages, there are fairly strict criteria laid down — it's not at all a nebulous thing. The parameters are there and a decision is made and it's got to be a clearcut yes or no decision. You know, you either stop work or you carry on work. If you carry on work, you carry on work as if you're going to get it to the market — you carry on full steam ahead. It's a definite yes/no thing. The criteria have to be fairly rigid and you can't really tamper with them. Obviously you can try and assess whether they're worthwhile criteria, as criteria, but you can't pretend the criteria don't exist and carry on regardless. You know, at the moment I'm not really in a position to examine their system of drug progression and decide whether their criteria are the best possible criteria . . . at the moment I'm only just becoming aware of what the sytem is and what the constraints are and more-or-less having to accept them. And I'm fairly happy at this stage to do that.

Dr. Hart: who neither had nor wanted freedom to choose projects

— Would you like more freedom, for instance, to choose the projects that you work on ?

Only if I had a better overall view of the way in which the company is going and some insight into the economics of things. I think it is a hell of a decision for someone in my position to say, "oh yes, I think I'll work on this new fibre, because I think it could look promising". I think it would need . . . I think there should be people who're in this position, yuh, but I think you'd need some years of experience in the company to get an idea of what the . . . more of an idea . . . I think you need more than experience of research, but experience of a lot of things — in marketing, economics, because I think businesses and firms are so complicated these days that this would be absolutely essential. I mean, I know that the nylon fibre industry was started by a guy who was employed to sit in a lab and think, well this looks promising; I'll work on this, and told the department so, but that was forty years ago and things have changed a hell of a lot since then. I think the textile industry and industries in general are so capital intensive, and research costs such a hell of a lot of money, I personally wouldn't be happy to take the decision, no. I'd certainly like freedom to do the problem as I see fit, but you're given that freedom. But as far as picking your own problems — mm . . . no.

DISCUSSION: FREEDOM TO CHOOSE PROJECTS

In terms of sheer numbers, the support for this aspect of the value conflict theory is not impressive. Only four scientists wanted freedom to choose projects. Of these, one had all the freedom he wanted, two had less freedom than they wanted and only one had no freedom to choose projects at all. The spirit in which they held these views must now be considered.

The views of three of the people who did want freedom to choose projects have been illustrated (Drs. Merchiston, Bernard and Lindoch) and they do seem to be in the spirit of the 'ethos of science'. That is, they express an ideal, the essence of which is freedom. The extracts from Dr. Merchiston's interview seem archetypal in this respect and are even illustrated by one of the heroes of scientific history. The other person, Dr. Marchmont, whose views are not illustrated here, thought he should have more freedom to choose projects, but did not express himself in idealistic terms. His felt lack of freedom did not derive from him seeing himself as a scientist. He attributed it to an interfering boss, who he felt was inadequate by any standards. He assumed the problem would be removed if the boss was changed.

There is then only very limited support for Merton's thesis here in the way three of the scientists expressed their views. However, even then, not one of them wanted nor expected nor had <u>complete</u> freedom to choose projects. While they saw it as a possible focus for conflict, they conceived of their freedom as existing within the contexts of their workplaces and recognised the importance of its constraints. In one case indeed (Dr. Bernard) considerable emphasis was laid on how having such freedom would enable him to do better work for his firm.

Of the larger numbers of scientists who wanted some freedom to choose projects, but in a much more qualified way, the views of only two people showed any glimmer of the 'ethos of science'. Their views have both been illustrated (Drs. Warriston and Mayfield). Both make vague assumptions about the linkage between science and freedom, but these are not as fully expressed as in the earlier examples and there is a much heavier emphasis laid upon the requirements of industry. The example of Dr. Carrington, who wanted some freedom, but would always be prepared to make decisions in conjunction with his supervisor, was much more typical. He would strongly object to any curtailment of his freedom, but this was because of his qualifications which he equated with being a responsible person. The view that scientists should have freedom because they have been trained and have gained qualifications was a very common one. Another scientist, Dr. Wemyss, speaking in a similar context, put it like this :

> A scientist is not a magical person who can solve problems: he's only someone who can apply what he learns and perhaps put in a bit of intuition and guesswork as well. Obviously, if you've got six years of intense chemical education behind you, there's no point in employing you to be a lab attendant.

In other words, the reason for giving a scientist responsibility and freedom is because he is a trained man. There is no suggestion in these statements that any necessity for freedom lies in the nature of science. Hence such statements are not in the spirit of the 'ethos of science'. They are more in the spirit of the expectations of anyone in our society with years of training behind him.

In fact, all the people who made qualified responses about wanting freedom to choose projects (except one) emphasised the interests of their firms. Typically, they would make decisions in consultation with other people — colleagues and/or management — and the needs of the firm would be given the utmost priority. Wanting freedom to pursue individual scientific interests was, for the most part, just not seen as a relevant ambition in the circumstances. Even where it was, it was completely secondary to doing the work ~~the work~~ the scientists felt they were being paid to do.

Of the people who did not want freedom to choose projects, little needs to be said at this stage, since their views were the antithesis of the 'ethos of science'. Indeed two of them asserted not merely that they did not want freedom, but that scientists should not expect it. The views of one of them, Dr. Lauriston, were illustrated. The other one, Dr. Ravelston, was quite scornful about the notion of academic freedom, which he described as "the freedom to do nothing".

A few remarks must be made about the final category of people who did not say explicitly whether they wanted freedom to choose projects or not (the 'no explicit information' category). The way in which these scientists discussed their work seemed to rule out the question: "do you want freedom to choose projects ?" The natural course of the conversation simply did not yield answers to this point. Often (in six out of the ten cases) the scientists described the way they got their work and the amount of choice they had in the process and then asserted that they were quite happy with this state of affairs. The inference to the degree of freedom they wanted may have been obvious, but strictly speaking, the question had not been posed directly. The other four people, all of whom had some freedom to choose projects (in the 'Qualified' category), again described factually how they got their work and how much choice they had, but did not comment on this particular aspect of their freedom. They all made comments about the extent to which they were satisfied with the amount of freedom they had in general and from these, it would probably be reasonable to infer that they were satisfied with the amount of project choice they had. Again they did not say so explicitly.

A characteristic of all these replies was their extreme situational contingency. The discussion was in terms of the details of their own situations and, in this context, hypothetical questions about how much freedom they would like did not have any place. The situations they were in were taken-for-granted and did not, as far as could be seen, cause them any discomfort or unhappiness or act as a focus for any grievances.

These people's views did not appear to give any support implicitly to the value conflict thesis. Having autonomy over choice of projects was a non-issue for them, and insofar as their freedom in this respect was constrained, they saw these constraints as entirely legitimate and as being in the nature of things.

11. FREEDOM TO WORK IN OWN WAY

The main results on this aspect of freedom are again summarised in tabular form.

TABLE 79

NUMBER OF SCIENTISTS WANTING AND HAVING FREEDOM TO WORK IN OWN WAY

		Wants freedom to work in own way			
		Yes	Qualified*	No explicit information	Total
Has freedom to work in own way	Yes	13	10	5	28
	Qualified*	2	8	2	12
	Total	15	18	7	40

*'Qualified' means: yes in some respects and no in others.

The contrast between TABLE 63 and TABLE 79 is striking. Whereas few scientists either had or wanted freedom to choose projects, the majority wanted at least some freedom to work in their own way. Two people had less freedom to work in their own way than they would have liked, but everyone else had as much, if not more freedom than he wanted in this respect. Indeed eleven people explicitly mentioned that they would not object if they had less freedom and some of these would actually have preferred less.

The sheer numbers of scientists wanting some freedom to work in their own way would seem to represent considerable support for the value conflict thesis.[2] Before that conclusion can be reached, however, the way in which such wants were expressed must be examined in more detail. Do they reflect the 'ethos of science' or is the motivation behind them rather different ? Extracts from the interviews will be presented to shed light on this. The location of the illustrative material is given in TABLE 80.

TABLE 80

LOCATION OF ILLUSTRATIONS IN TABLE 79

		Wants freedom to work in own way: Yes	Wants freedom to work in own way: Qualified
Has freedom to work in own way	Yes	Dr. Grange Dr. Albany Dr. King Dr. Buckstone	Dr. Brunswick Dr. Newington Dr. Montgomery Dr. Clerk
	Qualified	Dr. Merchiston Dr. Bernard	Dr. Nicholson Dr. Gilmerton Dr. Chucklie

[2] What is at issue here is the extent to which the scientists were attached to the 'ethos of science', rather than, as yet, whether these values conflicted with industry's. For simplicity, I shall speak of evidence for attachment to the norms (or ethos) of science as evidence for the 'value conflict theory'. Any necessary distinction between attachment and conflict should be clear from the context.

ILLUSTRATIONS

Scientists who both had and wanted freedom to work in their own way

Dr. Grange

I wouldn't like a job where I was told, "you must do this to-day; get it done for this afternoon", sort of thing. I would only like a job if I had a reasonably long-term aim — sort of a month, two months, six months — so that I could plan my programme myself. The industrial environment . . . it's a bit of a fallacy to say you're worked like stink. Several days at work, I can quite honestly say I've done nothing and that I've perhaps read a couple of magazine articles — admittedly technical, but magazine articles I've been meaning to read for quite a long time — and that I happened to be wandering through one of the wings and met a bloke I knew working on a rig, chatted to him about it for a long time and had coffee with him. You certainly wouldn't be able to do that if you were watched every day. But, on the other hand, there have been days when I've come in at 8.0. clock in the morning and I've worked through and haven't had lunch and haven't left till 7.0. clock . . . two days, I think. There is that freedom of when I can work which I wouldn't like to give up.

— When you say you wouldn't like, is it just a question of not liking — your personal preference — or do you think scientists in general should have that freedom ?

I think if scientists are going to produce their best work, they should have that freedom, because as soon as you constrain . . . not necessarily a scientist, but as soon as you constrain anybody any way and they feel they're being constrained, their work . . . well obviously it would suffer because they're worrying about the fact that they're constrained. Only a masochist doesn't mind being constrained. Scientists particularly so, because scientists seem to have a reputation for being pretty odd birds and they work at peculiar times and they just don't — almost by tradition — don't work nine till five. You can't call up creative scientific work to order, or at least, I don't think you can.

This was not Dr. Grange's last word on the subject. He went on to talk of the advantages of having a job where you work to strict deadlines and where there is no call to take work home with you, or think about it out of hours. Such a job, contrasting markedly as it would with his own, also clearly appealed to him. Unfortunately, his discussion of it is too lengthy to present.

Dr. Albany

Yes, I should get very upset if I was told how to do it — very upset. I'm not though . . . as long as there are three-monthly reports and if I'm not getting on, it's discussed, you know, but we're not told to do it this way. I do it my own way.

— Can you say why you would get upset ?

Sheerly pride. I reckon I'm the best one to decide how to do it. I don't appreciate being told what to do by somebody I consider knows less about it than I do.

— Is this likely in your situation ?

If they did tell me how to do it, yes.

— But wouldn't it be somebody like your section leader who is highly experienced and qualified — a high flyer from what you said ?

No, simply because it's . . . what, he's got . . . shall we say, six or seven people of my standing under him. He's also got the general administration; he's got the politics. He also trots around the place a bit and round the country — this kind of thing. He hasn't got the time. I mean, I consider that I know more about my Ph.D. than my supervisor. It's just a function of the time you spend on it. You've got the experience; you've got the intelligence. He talks about it . . . what ? . . . shall we say about half an hour a week about it ? So you're working at it 39½ hours a week more than he, you see, so you're bound to know more about it than he does. He can suggest things that maybe you haven't come across, that you don't know. They may have a machine in the research department that he'll know because his view is broader. Certainly you'll expect that kind of feed-in, because it's his job to scan the journals overall for any new developments, as well as ours, and he'd tell you about that. But to tell me that I ought to try putting y in, then x, and then z, then heat it up to 120 degrees, then . . . well, you know, I'd just be a pair of hands and I didn't go through six years to be a pair of hands.

— But in any case, this is not a likely occurrence.

No. It's just not a likely occurrence.

Dr. King

— Do you have enough freedom ?

Yes, oh yes. That's one thing we do have — plenty. If you've got a job to do, you should spend quite a bit of time thinking about how you're going to do it and the best way to do it and talking with people about the way you should do it and whether it's been done before that way and anything like that. You certainly have the freedom to do that. You're given a project and they come with a scrap of paper saying, "so-and-so says this; what about trying that", and it's up to you to work the project through — you know, how you're going to do it — go right through and finish it, but again because you haven't got as much time as you ought, as much time setting it up as you ought, it doesn't get done quite as well. But you certainly have the freedom to do it the way you want to do it, the way you think is best. You're certainly responsible for it at the end. If you've done it the wrong way, then you've wasted your time for six months and . . . It's very useful to have that freedom, because it at least gives you the feeling that having done a degree and a Ph.D. that someone at least regards you as having the ability to look after yourself, which is one thing.

— Yes. Would you in fact object very much if you didn't have as much freedom ?

Yeh, yes I would definitely.

— And would it be on those sorts of grounds or . . . why would you object precisely ?

Simply because I'm old enough and feel I know enough about general things — about work, you know, the way things should be tackled that I ought to be able to run my own project and having someone putting on the line exactly what you're to do day-to-day is just too much. Even . . . well doing a Ph.D. you don't get that; you have a lot of freedom really to think what you want to do. Admittedly, the thing's set up for you in the first place, but as things go through, it's up to you to think of different ideas, new ideas, and I think it's a retrogressive step if you find yourself being told exactly what to do hour to hour. So I would object. I suppose maybe a little more rigidity would be acceptable, but not much.

Scientists who wanted freedom to work in their own way, but whose freedom was in some way restricted

Dr. Merchiston

. . . the main frustration there is the management . . . yeh well certainly middle management. There doesn't seem to be any direction . . . we're supposed to be getting data and so on and so forth for the commercial reactor, but they don't seem to be directing . . . I don't think they are directing the effort in the right direction. There's not . . . although there's a lot of interaction between research officers in different groups and different divisions, there's not really a lot of interaction between division heads and section leaders and what tends to happen is that you get duplication of effort and, for example, you get people doing work in [naming his field] who should never be doing work in [the field] . . . and we're in a position now where we've got a section leader from another division who's got a lot of problems following what we do, who ultimately decides whether we can get apparatus built or not . . . and it's very frustrating to think that not only another section leader, but someone — a section leader in a completely division — can influence the way that you work and how quickly you work.

— Does he actually ?

Yes, he does in sort of respect of apparatus and getting apparatus built and designed.

— How does that work quite: is it that he's not sufficiently in touch with what you're doing, or . . . ?

No, you see the thing is he's been made responsible for [name of field] safety and he knows less about [name of field] safety than anybody else in the building probably and consequently you see, what tends to happen is you design a piece of apparatus and get it vetted for safety, O.K., and you've got to take the design and drawing of the thing, you know, to the guy and he says whether you can run the piece of apparatus or not — on safety grounds. As I said, he probably knows less about it than anybody else, but he's got that control over whether your apparatus can be built or whether it's got to be modified and so on and so forth, you see — safety reasons. And it's very frustrating because the general feeling is he knows less about it than anyone else. It's a petty — these are petty things that are peculiar to . . . this job and you get these little petty frustrations, you know.

— Are they things that have a petty effect or is the effect quite serious ?

Oh it is quite serious; it's not a petty effect. It can put me back three months, submitting designs and modifications and everything else you know. It can certainly put the work back.

— So in fact, this is a . . . you were saying before that you had quite a lot of freedom to go your own way about things, but this would act as a quite severe constraint on your freedom.

Yeh, that's it — a constraint on you. You've got the freedom to define what work you're going to do; you can say what work you're going to do; design apparatus, prior to vetting by this guy and . . . yeh, that's the one major constraint at the moment.

Dr. Bernard

. . . my section head, in particular, is, I think, a bit dubious, in that he doesn't really understand that people like myself — people with Ph.Ds — he doesn't really understand that they really want to work from a research point of view and have a lot more freedom than they have at the present time. He thinks, you know, we're like other graduates except we're a bit more experienced perhaps and that we take orders just like anyone else does.

— And do these orders go further than just what you're to do — do they go as far as the ways, methods and techniques you should use for tackling the problem ?

They do quite a lot, yes, they do. I must say they probably do to some extent . . . we tend to have more-or-less directives about how we should go about problems. We're virtually told what we ought to do, which isn't very good, I don't think. I think it'd be done much better if we could do what we think ought to be done.

— And is this, in fact, because you know rather better than your section head what ought to be done ?

Well he's got such a lot of experience that he ought to know what's to be done, but he tends to adopt the short-term attitude, while you're always adopting the long-term. He's used to short answers, whereas we always , . . well myself, I always plump for the long-term one. So there's a bit of . . . there's no friction about it, no upset, we don't get at each other or anything like that. It's just that we prefer to do things in our own ways and I must admit that from time to time, I do what I think I'm going to do regardless and let him get on with it afterwards.

— Does this have repercussions ?

I quite often . . . well a couple of times I've been told that I should have done what I was supposed to have done and shouldn't have done something else, but it doesn't worry me particularly. As long as I get good results, I don't care.

Scientists with mixed feelings about wanting freedom to work in their own way, but who had such freedom

Dr. Brunswick

I'm not a scientist, not now. I'm an engineer. I'll rephrase: I'm not working as a scientist. A scientist is someone who investigates the world. I'm not investigating the world; I'm building systems. That makes me an engineer.

— Do you in fact think that has large implications for the amount of freedom you should have ?

It has crucial implications. There's a case for saying that science should be free and scientists should be free to follow the truth where it leads them. I don't say that I necessarily hold this view, but it's a view. There's no case for saying this of engineers. Engineers must build the systems that society wants building and if we want a Channel Tunnel, then the engineers must build it. They must not build it if we decide we don't want it. I overdraw. For instance, if we had to deal with Jensen and his work on black I.Q., I might want to say something a bit different, but that's the way I'd draw it. And so, I'm not in the game to which that sort of freedom is relevant. The sort of freedom which I do have, which I value, is that no one comes round and says: "get yer hair cut", you see and no one says, "you must be in at 9.0. clock and you will stay until 5.0. clock and woe betide you if you don't". Nobody says, when I take an hour and a half for lunch, which I do every day, says, "you're only allowed an hour for lunch — you really . . . ". You know, I'm free of all this and this is nice and I think, to me, the important freedoms in what I do, and they're freedoms which I get in [name of his employing organisation] which I would get in some other places, but which would be lacking in some other commercial jobs.

— That covers, I suppose, freedom to do what you want to do: what about freedom to do it in the way you want to do it ? Where does your scientist/engineer distinction come in there ?

That's not an easy one. I do in fact have a fair amount of freedom on this one as I think I've indicated.

His Wife: But it's rather a matter of chance that you have. It's not necessarily within the framework, is it ?

It isn't, no it isn't necessarily within the framework; it's the consequence of a dynamic — of who you are and where you are — that some environments are tolerant. On the other hand, some people are very easily cowed by a slight pressure in a tolerant environment and someone who is in a tolerant environment and who is, you know, fairly opinionated and forceful like me, is able to shape his own work to a substantial extent. Now I think I'm also lucky in this, in that because I'm good at the job, my superiors have been prepared to say, "O.K. we'll let you develop this in your own way". If I were not good at the job; if I were slow, then there would be a tendency to say, "well you're only doing a little bit of the job and therefore someone else must tale overall responsibility". For instance, I am responsible for four suites of programmes and one set of data entry equipment in its functionning in the system and a few other oddments as well, but that's essentially it. If I were responsible for half a suite of programmes, all the important decisions on the suite would probably be taken by my boss. Because I'm responsible for that suite of programmes, I take the decisions. I don't of course take them entirely freely; I have to take them in consultation, but I take them.

— So you would put it, would you, very much in personal terms, rather than in terms of general principles that can be enunciated for different groups of people ?

Are you asking me how much freedom I should have in this type of job, or how much freedom I do have ?

— How much . . . well whether you'd be prepared to talk in terms of how much people should have, or whether it's very much something that has to be worked out pragmatically according to people's individual differences ?

No. I think there is the beginnings of a general answer, but it's not an easy one and I suppose what I really believe in is the abolition of the hierarchy I work in, because I . . . well I think this is what I believe. And the abolition of the hierarchy would need to be associated with very complex political changes, the result of which would be to change people in the system.

— You mean political changes on a societal level ?

Yuh. I mean, I'm talking about workers' control. When you are talking about workers' control in data processing, you can't just talk about that; you've got to talk about the people who are doing the manual jobs and the people who are being displaced from jobs and you've got to talk about it on a very broad level . . . but that's the sort of answer I'd give. Now, if you want to ask me what level of freedom is functional in terms of the present level of [this firm's] organisation, now that's a separate question.

Dr. Newington

I was quite closely supervised during my Ph.D. which was quite good in some respects, but in other respects bad, because I think if you're left on your own for three years, with obviosly some guidance, you learn how to do research, rather than . . . or as well as learning the subject you're doing. But I was guided so strongly that I didn't really learn how to do research. I merely learnt a range of techniques, which I applied in set ways, so I . . . I think the fact that I was so closely supervised was a bit unfortunate. I probably did more work than I would have otherwise and learnt more things, but I still felt a bit shakey about how to go about a research problem at the end of the three years. The more valuable aspects were just the techniques I'm afraid — nothing very deep about that.

— In that case, would it be true to say that you've got more independent since leaving university in your work ?

In terms of my immediate sort of guidance, yes. Yes, I'm much more on my own now. With the original project, my boss didn't really understand what I was doing, which made life a bit difficult. It meant if I had a problem, I couldn't go to him. When I was about to do something, I had to explain to him, "now I'm going to do this, because this, this and this". He didn't really follow. That wasn't particularly bad; it didn't produce any friction or anything; it just meant that I was more out on my own . . . but in terms of immediate guidance, I'm more independent this year than I was last year, yes.

— Do you appreciate that ?

No I didn't really, no. Yes, I'm more independent in terms of guidance, but in terms of the path I've got to go along, I'm less independent. I've got to do certain things this year. When I did my Ph.D., I could look around for systems I wanted to look at and do them and then get strong guidance. But now, I've sort of been told, "do that", and then, "do it on your own basically, because we can't help you". I feel that more training and more direction from the company would be a help — more company involvement. That would help me; I don't suppose it would help the company too much in the short-term, though in the long-term it might. I don't really know.

Dr. Montgomery

— You mentioned that you have considerable freedom: do you feel that you have enough ?

Sometimes I can have too much. In the next six months, I'll be responsible for £100,000 worth of billing. Now, if anything goes wrong with that billing, I'll call the group manager in, but apart from that, I'll just submit reports. That makes me gulp. And that isn't selling a piece of hardware that costs £98,000 and the other £2,000 on top. That is staff time and machine time. I, on my own initiative, have been putting in computer runs for £1,000 for some time now. I decide that it's got to be done and that's it; I put it in and it's done and it's charged. Well that's, I think, quite a responsibility to have. In this sense I think [naming his firm] has been very good for me. I've had quite an advancement that I wouldn't have had in a much larger organisation, I'm sure. So it's been very good experience; very exacting experience too.

Scientists limited in the freedom they had to work in their own way, but who did not want complete freedom

Dr. Nicholson

The basic project you're told what to do. For example, I've been told to work on this existing process and I'm at the moment following up lines that are carrying on from what was done before and I'm extending it. In a way yeh, I think I'm left pretty much to myself to decide what to do and what not to do. Obviously you've got to be sensible in this choice. I mean from a scientific point of view, you might want to look at certain things out of interest, but you can't do that; you've got to realise that your object is to make a better product, so you've got to direct your enquiry along these sorts of lines. I think I'm left pretty much to myself as to what to do, although you go to meetings and you discuss it with other people and they perhaps suggest ways that you should go — you know, it's perhaps a mutual thing.

Dr. Gilmerton

Basically I'm the only one working in my particular bit of the job, so I sort of supervise myself at that level. Again, as I say, there is overall control to make sure things are keeping to cost and schedule. Certainly I'm not too closely supervised and . . . I myself know what I'm trying to achieve in the sense that there's a certain project, it's got to be completed by a certain time and preferably at a certain cost. I must sort out for myself what the particular goals are. I find it most satisfactory. I'm due to be transferred to a somewhat larger project and I don't know how that'll work out from that point of view. Obviously the larger the project and the more people working on it, to keep control, it's probably necessary to have much closer supervision.

— Is that something that worries you very much ? Do you particularly want freedom in your work ?

Um. . . I think the . . . I can see there is a degree of control necessary. I think depending on the people in charge — again I've no experience of working with them, so can't say what it's like — depending on the people in charge, this can be done at a minute level, which I personally would object to, because there'd be no freedom left to do anything. It'd be a case of sort of do this and hand it over and have it marked out of ten sort of thing. I'm not interested in that. If I got too much of that, I'd probably ask for a change or move to some other job. But I think it will be possible — depending on the person in charge — to separate out the different parts of the job and still leave it fairly free for individuals. I certainly think it's possible. whether or not it'll happen, I don't know.

— Yes, I was going to say, how likely is this very high level of supervision ?

I think a very high level is unlikely. I certainly hope so. If I did reach the level where I was simply told exactly what to do, every single decision I made had to be yes or no'd from someone higher up, I'd stick it for a while, then object and either ask to be transferred to a different part of [naming the company] or look for another job outside. I'm sure that people appreciate this as well and try to keep a fair degree of freedom. You see, I've not come across any problems in being too closely supervised so far — I can't complain.

— When you say you'd object, is that because you, on a personal level, want to have some independence, or is it something more to do with your scientific training — that you think, as a scientist, you should have a certain amount of freedom ?

Oh I don't think it's anything to do with scientific training as such. It's probably got a lot to do with having worked on my own during my Ph.D., but I wouldn't think this would be linked to science. It would be that social science and arts Ph.Ds — not that there are many of them — would find it irksome to have everything they did supervised — simply at the level that it's almost an insult to intelligence. You know, providing you can see a reason for what's being done, then fair enough, you're willing to accept decisions. But if it is simply a case of, "do it because I say so", continually, then I feel that . . . well certainly most people I knew at university would object to it and eventually would reach the level where they'd walk out.

Dr. Chucklie

They seem to rely totally on my judgement for anything they give me to do within certain bounds, you know, like they might give me an industrial oil they want testing for some reason or other and they'll tell me the type of thing to look for. Then it's up to me to go away and do it. It's very much that I'm left alone, if not almost entirely and in fact, there's like solders — a new solder will come in and they won't have had it before and they'll hand it to me and this chap'll say, "knock out a procedure for analysing it", so I just mess around having it in acid and so on — they call it a procedure; I call it a bit of a mess . . . so it's left almost entirely to myself. There seems to be an incredible — to my way of thinking — blind faith in a chap that's got letters after his name. You know, I've not done anything like this in my life before. I don't know any more . . . I know far less about this than they do. I mean, if they want some theory on quantum mechanics, I can help them out . . .

(Later in the interview talking about his work)

Well I'm not sure if freedom comes into it. The work is there and there's no sort of creativity in it except perhaps if I make up my own procedure. The work's there and it's got to be done. There's not much scope there for imagination. The methods are pretty well tried and tested and, in fact, there's things called 'British Standards', where standard procedures are laid down on how to do the work. You are supposed to do the work according to these standards in order to test it thoroughly . . . there's no leeway there.

— Is this another thing you regret ?

No, it's quite nice in a way to have it all written down. You just follow the instructions — or at least some of the time anyway. This is only for materials this 'British Standards' — like there's no 'British Standard' for this solder of mine or I'd naturally have followed that.

DISCUSSION: FREEDOM TO WORK IN THE WAY THE SCIENTIST HIMSELF WANTS

The fifteen people (see TABLE 79) who wanted freedom to work in their own way may appear to provide support for the value conflict thesis. The sort of freedom they wanted and the way in which they wanted it must now be considered to see whether this is the case.

Of these fifteen scientists, eleven can be dismissed straightaway as providing no support for the value conflict theory. They expressed their views in ways which did not at all conform to the spirit of the 'ethos of science'. They placed heavy emphasis on the needs and interests of their firms; conforming to these requirements was seen as a primary good. Freedom to work in their own way was wanted insofar as it did not conflict with this goal. They saw themselves as being employed precisely because of their capacity to carry out high level work without supervision. Freedom from supervision was a reflection of their industrial status and utility to their employers. Freedom, in their view, did not inhere in the nature of science, but rather in their role as specialised industrial scientists. Their claims in this direction were based on the length of their training and the esoteric knowledge they possessed, not on any moral values allegedly associated with the acquisition of that knowledge.

This leaves four scientists whose views were expressed in terms of the 'ethos of science'. These were Drs. Merchiston, Bernard, Grange and Buckstone. Dr. Merchiston's views have been quoted in this and the previous section. He also expressed his ideals about scientific freedom in another place in his interview when talking about the reasons for having had academic ambitions. He admits that he did not have a "burning ambition to teach", but that the attraction was the amount of freedom you have in research. One of the noteworthy aspects of his views about wanting freedom to work in his own way is their concreteness. His complaints concern a particular manager in particular circumstances and are not based on appeals to general principles or ideals.

Dr. Bernard's views are more general. He wants freedom so that he can pursue particular approaches to problems — namely a more long-term fundamental approach. This he thinks (elsewhere in his interview) would make his work more interesting and give better results for the firm (see also p. 66 above). This facet of freedom — being able to take a particular approach and follow potentially fruitful leads — was often referred to by the scientists and will be discussed further later in this chapter.

Dr. Grange's views provide a fairly straightforward expression in Mertonian terms of the desire to have freedom to work in the way you want. Interestingly, he talks as though he were thinking of a stereotype, rather than responding to events in his own work experience. He says, "scientists seem to have a reputation", and that they do certain things "almost by tradition". In other words, some of the time he is not asserting how scientists are and how they must be; he is talking about how they are sometimes seen to be. Thus his expression of the norm is general and rather abstract, although certainly real enough to him to justify his claim that you cannot call up creative scientific work to order.

The scientist whose views have not been illustrated (Dr. Buckstone), like Dr. Grange, had as much freedom as he wanted to work in his own way and rather more than he would have expected. He thought scientists should have "some flexibility" in how they work, or otherwise they probably wouldn't enjoy the work and in the long-run probably would not be very productive either. In thus linking productivity with freedom, or at least, "some flexibility", he gives implicit assent to the notion that freedom is necessary for science and therefore his views can be taken as a weak expression of the norm.

Two of these four scientists just discussed also emphasised their company's interests. Both Dr. Bernard and Dr. Buckstone saw the freedom they wanted as having a place in the context of industry and probably contributing to industry by maximising the chances that the science would be good. Dr. Bernard thought that in principle the interests of good science and industry were not in conflict. In practice, however, scientists (with Ph.Ds at least) wanted to follow different approaches from those advocated by industrialists and mutual misunderstandings sometimes resulted.

So far, four scientists whose values conform to the 'ethos of science' have been located amongst the fifteen who said they definitely wanted freedom to work in their own way. From the overall sample of forty, this leaves the twenty five who were much more guarded and qualified about even wanting this freedom at all. From amongst this twenty five, two scientists can be found whose views contain elements of the kind expected from Merton's theory. The views of these two scientists, Drs. Clerk and Brunswick, should perhaps be examined in order to ensure that no fragment of support for the value conflict theory is ignored.

Dr. Clerk's views have not previously been illustrated. He expressed himself in terms which could be described as 'Mertonian'. He felt that "freedom breeds interest" and therefore letting scientists be free was likely to mean that they would do better work. This would be good for the firm as well as more interesting for the scientist. To the extent that he makes a link in principle between freedom and good scientific work, his views are Mertonian. His attitude to freedom was, however, still very context bound. He could not envisage any conflict occurring between him following his own interests and the firms interests. This was because, firstly his brief was that, as long as he could justify the scientific interest of the work he was doing, he could go ahead. Secondly, he thought that any reasonable person would follow his own interests only within reason. His view of the place of freedom for the industrial scientist was based so much on the assumption that the scientist would only want to exercise his freedom within the context of the firm's requirements that he would not be drawn, even hypothetically, into assessing priorities in the case of any conflict of interests. The very specific, context bound nature of this scientist's views was in fact a common quality of the views of the sample as a whole.

The other scientist in this category, Dr. Brunswick, begins by presenting the academic archetype of the scientist freely pursuing truth. But he immediately adds that this picture, if it has any relevance at all, does not have relevance for him, because he no longer considers himself to be working as a scientist. Rather, the freedoms he values in practice are rather banal everyday ones to do with hours and conditions of work. His discussion of freedom in work is characteristically concrete and emphasises individuality — that is, he should have freedom because he is good enough at his job to make proper use of it. Other people might be less good and so should not have such freedom. In fact he is only prepared to formulate an answer in general terms in connection with wider political issues and visualises the only meaningful context for greater autonomy as being that of workers' control. His general views about freedom and its desirability appear to stem more from his socialist principles than from any notions about the intrinsic necessity in science for freedom.

TABLE 79 shows that seven scientists did not say explicitly whether they wanted freedom to work in their own way or not (the 'no explicit information' category). The situation here was similar to the case of the people who did not say explicitly whether they wanted freedom to choose projects or not. That is, they discussed the matter in very situationally contingent terms, such that the abstract issue of how much freedom they wanted did not arise. They all described factually the way in which they worked and the amount of freedom they had and then expressed their satisfaction with this state of affairs — or, at least, their feeling that the amount of freedom they had was reasonable.[3] Since these scientists do not express general opinions, let alone moral principles, about how much freedom they would like to work in their own way, and given their general satisfaction, their views do not constitute support for the value conflict thesis.

OTHER ASPECTS OF FREEDOM IN WORK

The scientists did express concern with certain other aspects of freedom not covered by the discussion so far. In order to follow the policy of displaying prominently any evidence at all that favours the value conflict theory, this evidence will now be examined. The other freedoms which mattered to some of the scientists broke down almost entirely into two types. One concerned hours, the other work content.

Eleven of the forty scientists mentioned hours when freedom was being discussed and said how they disliked 'nine to five days'. These statements were all made spontaneously and varied considerably in their intensity. Three of them objected very strongly indeed. Dr. Hart argued, "you just can't constrain physical laws to operate between 8.30. and 5.0. so that they time in nicely and give the results at 5.0. clock". He then went on to draw morals from the fact that important discoveries made in universities had always

[3]It is possible that had any of them had less freedom to work to work in his own way,he would have given this issue more explicit consideration and possibly in Mertonian terms. However, given the fact that in the rest of the sample, only two people had less freedom than they wanted, the likelihood of this hypothetical situation arising does not seem very great.

been made, he thought, by people who were utterly unconstrained and could work when fancy took them. The other two (Drs. Ainslie and Marchmont) found their greatest difficulties in "switching off" at 5.30. Dr. Ainslie could, in fact, work late, but still had to be in on time next morning. This also applied to Dr. Hart and they both felt disinclined to work late if they were not going to get hours off in lieu. Freedom to work when you wanted was thus linked explicitly with the nature of scientific work and so is in the spirit of the norms of science.

The other eight disliked 'nine to five' hours more-or-less strongly, but were more philosophical in their acceptance of them. They viewed them as "difficult to get into", or "something that they'd get used to", or "something that made them unaccountably tired", but not as a major source of distraction or discomfort. They were seen very much as part of the problem of adjusting to industrial life and comparisons with university life were wistful memories rather than fervent statements of value, bearing on the very nature of scientific work.

The other aspect of freedom which emerged prominently in the way the scientists talked was variously referred to as: "being able to follow your own ideas", "being able to go off on interesting tangents", "being able to pursue one's own independent approach", and "being able to initiate one's own ideas". Eleven scientists referred to this freedom from various points of view. Four mentioned approvingly that they had plenty of opportunities to follow their own ideas. Three said they missed not being able to follow their own ideas or go off at tangents, but thought that in the context of their firm's requirements, that this was quite reasonable. A final four felt more strongly that they really wanted more freedom in this respect, though of these, only one spoke in very strong terms. He (Dr. Bernard) was prepared to defy management to do what he wanted, although he insisted that this was for the ultimate good of the company and not just for his own gratification.

The essence of this freedom seems in the spirit of the 'ethos of science' and it is reasonable to say that its existence does provide support for the value conflict thesis. There is thus some evidence for the independence-autonomy norm in what the scientists said about hours of work and freedom to pursue their own ideas. Like the evidence discussed earlier, however, it is both weak and ambiguous.

The discussion will now move on to the topic of publishing and the evidence found for attachment to the norm of communality.

111. FREEDOM TO PUBLISH

The main results on this topic are summarised in the table below.

TABLE 104

NUMBER OF SCIENTISTS WANTING AND HAVING FREEDOM TO PUBLISH

		Wants freedom to publish			
		Yes	Qualified*	No	Total
Has freedom to publish	Yes	2	7	0	9
	Qualified*	1	11	8	20
	No	0	2	4	6
	Not Applicable	0	2	3	5
	Total	3	22	15	40

*'Qualified' means: yes in some ways, no in others.

The only obviously striking feature of TABLE 104 is the small number of scientists who wanted freedom to publish (N=3). A far larger number either wanted it to some extent, or in some circumstances, or did not want such freedom at all. However, the details of their views and their reasons for either wanting or not wanting freedom to publish were often quite involved and contained curious twists. Illustrative quotations will now be given. As in the previous two sections, the presentation will begin with those most likely to provide support for Merton's thesis.

The illustrations come from the categories in TABLE 104 as tabulated below.

TABLE 105

LOCATION OF ILLUSTRATIONS IN TABLE 104

		Wants freedom to publish		
		Yes	Qualified	No
Has freedom to publish	Yes	Dr. Merchiston Dr. Dundonald	Dr. Grange Dr. Babberton Dr. Lindoch	—
	Qualified	Dr. Marchmont	Dr. Buckstone Dr. Inglis Dr. Andrew	Dr. Forres Dr. Heriot
	No	—	—	Dr. Ainslie

ILLUSTRATIONS

Scientists who both wanted and had freedom to publish

Dr. Merchiston

— You were also saying about publishing that you've got freedom to publish and this is something you value. Do you think again that this is something scientists should have ?

Yuh, I think so. It puts your work out to a wider audience and it allows for criticism of the work, which is always useful, because sort of departments in universities or sections in industry, they can become inbred and you know you get certain ways of thinking in certain groups and you get used to those ways and if you do a piece of work, you look at it in that particular way. If you publish it, that always gives you a chance to get to other people's ideas — you know, you can't necessarily talk to them. You get comments from various people on the piece of work and it gives a chance for that to happen. It's always useful in that respect.

— Are there circumstances in which you think the restriction of publications might be legitimate ?

Oh yeh, I can see that.

— Can you say what sort of situations you think those would be ?

Well obviously classified situations. If you're working for government and the work's classified, then that's it, you can't do anything about it. You can't publish it in the open literature and you have to settle for that.

— What about industrial secrets — that sort of thing ?

Yeh, well that comes into it, doesn't it.

— And you'd think that was reasonable, would you ?

No, I don't know. See, I can see a distinction between industry and government, if you like. Don't know whether it's right, but when you do . . . I couldn't see myself withholding something when someone is making a profit from it. So if you've got industry and . . . well basically I don't like the capitalist system and I can see . . . I think all these ideas of industrial secrets and all this sort of stuff . . . the pure . . . the only reason for withholding information in that sort of respect is just so that the company can make a profit. To me, that's wrong. I can see a situation where if you're working for government, certain things couldn't be published then. Perhaps it's more right to withhold information in that set of circumstances than in industry, you know, where things are withheld just for pure profit . . . It's funny . . . yeh I probably would accept it in government, although ideally . . . the ideal situation is obviously to publish the work and to get it out to get comment and all this.

<u>Dr. Dundonald</u>

He explained that although he had freedom to publish, he usually had to wait anything up to eighteen months before he could do so. This was to protect the interests of his employers and he thought it was quite legitimate and reasonable of them. Nevertheless, he did still find it annoying having to wait and wished the system were "slicker".

— Can you say why this annoys or irritates you ?

One is judged in the outside world, that is, scientifically, either by the numbers or the weight of the publications one has. There are only a few ways of making a reputation when one is very young, such as I am — comparatively speaking — and one is by publishing a lot of good material and the other is by personal contact. Now, personal contact is pretty mucky — you either meet people who matter, or you don't, but when it comes to publication and advertising yourself and your work, then you can do a lot about it. I feel quite strongly that if I do anything that is worthwhile — now I'm not saying which I judge as worthwhile, but which my superiors will judge worthwhile — I'd prefer to get it published as quickly as possible, because since I don't regard [naming his employers] as a long-term job, I've got to think of my future — you know, what am I doing now which is going to help me in years to come ? As I say, personal contacts are luck — you don't know who you're going to meet or who you're going to impress, but as far as publications are concerned, you certainly can do quite a lot about that. Again this is my personal feeling. Many of my colleagues disagree, but . . .

— In what respect ?

They disagree with this attitude which I and some of my colleagues have of pushing on with work — trying to get things finished, trying to get topics completed. You see, I'm quite happy to take a project for six months or a year, get an answer, publish it and forget all about it. Now this is not the attitude that many of the older members of staff have. They've been working on one project for ten years, sometimes fifteen years. They want continuity; so they're not quite so flexible as they might be; so they hate to jump like a grasshopper from one thing to another. I don't mind; I don't see why one shouldn't be adaptable enough to do this.

A scientist who wanted freedom to publish, but who would not necessarily be free to do so

Dr. Marchmont

. . . usually most of the work that's done in our group is not confidential. Perhaps this stress corrosion work could be and you wouldn't be allowed to publish it, becuase it's relevant to [naming the firm]'s problems.

— Would that be a disappointment to you ?

It would, yeh. I've not thought very much about that, in fact, but I think initially, probably some of the work you did would be publishable, but I certainly wouldn't fancy being on a very closed project and not allowed to go and discuss it with anybody, for instance, anyone who comes here — you know, you can't show them the work you've been doing. I certainly wouldn't fancy that at all.

— Is that because of not being able to discuss it, or have you particular reasons for . . . ?

No, I think it would be this discussion really. I find this rather annoying when if a visitor is coming, people say, "well don't tell them about this or else they'll start working on it". One aspect is security and the other is simply people like to keep things quiet till it's published. Some scientists work like that, but I find it despicable. You know, if someone comes along to me and they'd been working on the same problem as I had, I'd just openly talk about anything and put the problem first, but I think quite a few people wouldn't. So if my job drifted over to a more confidential nature, I'd probably be very disappointed. Then again, if this happened very slowly, possibly you wouldn't notice it, though I probably feel very strongly at the moment.

— Can you say why you'd be disappointed ?

Well it's hard to say. I probably feel quite strongly that science goes beyond international barriers and so on and I feel quite strongly that these Russian scientists that want to go to Israel or somewhere and can't get on with their work — you know, I feel quite strongly about this type of thing which, I think, is related. Working in Cambridge in the group I was in, there were probably about five or six different nationalities and it just seems incredible to me that this kind of thing can go on — I mean, science is international. If you have a problem and someone else is working on it, then I think you should be allowed to discuss it freely and so on. At conferences — if you go to a conference and there are a wide variety of people there from different countries, I think it really enriches the thing. You often get totally different views of the problem. I couldn't actually say why; it's just a feeling that's probably developed.

— Do you see a conflict here between your allegiance to science, in a sense, and the company's interests where they wouldn't want you to publish ?

Yes. What would I do ? It's quite interesting. If it didn't involve a total ban on the work I was doing, providing I could publish something, then I probably wouldn't worry too much. I did at one time consider a job at Aldermaston, because I'd worked for the Atomic Energy Authority and signed the Official Secrets Act and it's not a very nice working atmosphere. It's a one-way process, because you can be asking other people questions and what work have they done and pick all their ideas and you're applying it to some particular problem and you're not allowed then to discuss the work you're doing. I just don't like the feel of that at all. So if it happened here . . . ? I don't know. I'd have to seriously think about it. At the moment, I'd say I'd avoid it really and if all my work was suppressed, I'd probably in the end . . . I would leave.

Scientists who were mixed in their feelings about wanting to publish, but who did have that freedom

Dr. Grange

If you're going to be a professional scientist, you've got to publish. There's no professional scientist's job I can think of where you can maintain your reputation without publishing. You've got to communicate your work to other people in order to let them recognise your genius and you just haven't got enough time to do it by word of mouth. And besides, if you're really a genius, people need to read your words and mull it over two or three times before they can understand it or before they can get the full import of it. So you've got to write up because it's fundamental to the dissemination of scientific knowledge. If you don't write up, nobody knows what you've been doing and they therefore assume you haven't been doing anything.

— Suppose you'd got a job where it was restricted: would you have objected ?

Yes. I don't think I would have accepted a job which was specifically connected with defence and things like that, although of course, when you say the job is restricted — i.e. if it's under the Official Secrets Act — you're prohibited from publishing outside, but you're not prohibited from publishing internal laboratory reports, but they of course come under the Official Secrets Act. So you're still able to publish in that sense. If you then apply for a university job, in fact, I think because most of the defence establishments are civil service, they have an arrangement with the universities so that your publications can be reviewed. So it's no real disadvantage in that sense.

— What's become of your claim, in those circumstances, that it would act as a communication with other scientists ?

Well you're communicating with other scientists in your lab and your other defence establishments. Your reputation extends over that region and if you're a scientist working in that particular topic area, the main thing is that you want your reputation to be spread amongst other scientists who're doing the same work as you.

— What about in industry ?

What from the point of view of industrial secrets ? Mm . . . that's a difficult one, because almost by definition they are new work, new devices that are being produced and somehow the thrill of the race, of getting something out before your competitor, is quite attractive. I think that in many respects, once you've brought out the thing that's been secret and if there is no longer a secret, then possibly the kudos of having been connected with it, or possibly having thought it up in the first place . . . the kudos of having done that, when it's come out is quite high, particularly if it's a success, and that may make up for the inability to publish in the meantime. As it is, in the large majority of scientific projects, the hardware of the thing is there before you write it up, so unless it was an extremely long-term job in industry, where you were only doing a tiny little bit of it, the publishing problem wouldn't really arise. If it was so valuable that you couldn't let it out, one would assume that your firm was paying you the earth anyway and you'd be quite happy.

Dr. Babberton

— Do you want to publish things ?

It's not really important quite honestly. It's always nice to see your name in print, but it wouldn't be any bother to me if they turned round and said you couldn't, because what often happens with papers is they're published, somebody reads the journal, they read it once, then that's it, they never read it again. It gets lost in the racks of some library and then dredged out maybe twice a year. It's alright, but I don't see any great advantage in it. The only advantage is that people in your field get to know your name, so if you go anywhere, they'll say, "oh yes, I remember your papers", which I suppose in a way must be quite nice.

— Would it be any advantage for you career-wise to have publications ?

I don't think so quite honestly, because there are a lot of firms who don't publish things in journals, so you know, just because you haven't had anything published, doesn't mean to say you haven't done any good work. It just might be that you work for a company where their policy is against publishing things. There are companies who do that, but there are companies who publish a lot. See, what happens is that if you publish something, it is a completely free press, so if you publish it, you have no further control over it. Once it's gone out in a scientific journal, anyone can use what you publish. It's not like a patent, where if you take out a patent and someone wants to use your invention, they've got to pay you. If you publish something in a scientific journal and somebody wants to do a complete replica of your experiments, they could, because you lose any sort of copyright on the thing as soon as it goes out, which again isn't very important, but I suppose some people would bother about this. They also publish patents as well. If they've got anything they think is worth putting a patent on, they put a patent on it, but there's no great compulsion either way. If you've got something you want to publish, they let you do it. If they want you to publish something and you don't reckon you've got enough on it, then they'll not force you. It's just that about every year, they'll come round and see if you want to present anything at a conference and if you do, well fair enough, you write your paper and present it, but nobody's going to force you to do it.

Dr. Lindoch

. . . I certainly would like to publish papers. People do. I mean it depends very much on the sort of work you're doing — whether it's of any outside interest.

— Would yours be ?

Probably not. I would probably just write reports so that it could be used within the industry.

— Again, do you think freedom to publish is something that scientists should have ?

No, I think that's definitely wrong. I mean if you're working for someone and they don't want this knowledge imparted to other rivals or anybody, then it's fair enough that you shouldn't publish. If you wanted to publish, then you have to watch what sort of job you're going in for and what sort of work it is. I think it's fair enough that they should restrict publication.

Scientists with mixed feelings about wanting to publish, whose freedom to publish was restricted to some extent

Dr. Buckstone

I'm not really working on general enough problems normally to have any success or be any value publishing. And also, as I say, it is to some extent classified and we wouldn't be allowed to publish it.

— If it were more the type of thing that were publishable, would you want to ?

I think so, because you do publish a lot internally — it is circulated throughout the labs in [naming the company] and to some extent, not I hope a hundred per cent, but to some extent, at the end of the year when you're judged — how well you're getting on — you're judged on what you've done — meaning what you've published. You know, they sort of look through this and say, "this is what you've done and it's quite good, but you haven't done very much in the past three years if this is all you've managed to publish. What else have you been doing ?", and you have to sort of justify having done that amount of work. If you have something published, if it's a report — that's a full-sized report — to get it published in the first place, you have to go through a panel internally. X people who are as knowledgeable in the subject as they can find, will question you on this, that and the other — why you didn't do this instead of that, and generally put you through your paces. So if you get something published internally, it's some kind of achievement and you ought to get some kind of satisfaction out of it. I think it's the same if you publish externally; it's just that it's available to more people, so that there ought to be more satisfaction out of it I suppose and, you know, as you go through the external people before you . . . To some extent, if you're in a big organisation, you can generate your own work and your own reports and masses of data and all the rest of it and you can go off the right track if you don't pay attention to what other people are doing too. But if you get something published externally, it shows it's something more general than to [naming his company] in general and that other people think what you're doing is valuable and on the main track.

— Do you think in general scientists should be free to publish their work ?

Well yes, as a principle, you have to agree that they should be free to publish it. Again I think you have also to say that some kind of information has to be kept back. Take an extreme case: you don't want to publish to the world at large what's the exact weight of how much uranium is required to make an atomic bomb.

That's a bit silly, you know, but there's probably a lot of material which could be published, but which isn't just because people are scared that it could be useful to somebody in a . . . top security point of view, but in fact, it's not often, I'm sure.

— Do you think there are ever circumstances — well apart from those particular ones — say in industry, when withholding publications is legitimate ?

There are several reasons for not allowing things to be published. There's safety reasons — some information generally known isn't safe. There's I suppose national security which could be allied with the safety one, but say if you like, like private information about some ministers or something like that — you know, not necessarily technical information, but technical information could come into it — you know, some information about a military range or something like that. There's also commercial reasons. I think you can justify not publishing material, if you've done work for a company and got some results out of it which commercially could be quite valuable, they don't want you to publish it — well the company don't allow you to publish it and I think reasonably don't allow you to publish it until they've got a patent on it or something like that. I think that's quite reasonable and because obviously they want to be safe rather than sorry, they probably restrict people unnecessarily. I suppose most companies will do that.

<u>Dr. Inglis</u>

. . . they do encourage publication. They are . . . as I say, their work is fairly similar to academic work and they like having their work in journals just to advertise . . . it's just advertising really. Obviously reading papers at symposia, anything like this is publicity, a chance to wave the flag, to make your name more widely known, to advertise yourself and the sort of work you do to people who're still in academic circles and it is in fact encouraged, with this proviso that you've got to be careful about not endangering your patent position.

— And are you satisfied with this policy ?

Well yes. Having seen what patents do and don't cover, I'm happy about the need to be careful with publications — to make sure it's patented first. In a commercial environment you've got to safeguard yourself that way, so there can't really be any quibbles about that part of it. When publishing papers, obviously, as far as the company is concerned, patents come first anyway. Our aim should be to get patentable processes and patentable drugs before we think about getting papers. Papers are a nice spin-off which, whenever possible they like to get, but it's patents that really count within the department — within the firm.

[and later on after he had explained at length that he thought being able to publish was a luxury, but nevertheless one which he would probably value] :

— So you do view it as a luxury, something that's an indulgence rather than perhaps a right that scientists should have ?

What free publication of what we do ? Oh yes. The point is everything we do is not of interest to everyone else. It's got to make a reasonable story, it's got to be . . . the volume of publications in the chemical journals at the moment is enormous and a lot of it is second rate, if not worse, and I don't see any point in adding to it. It might be of some interest to other people, but it's only marginal and unless you get something that's very worthwhile, there's no point you jumping into print with it. I think having gone through a Ph.D., having worked for somebody who's got a reputation world-wide and who was loath to go into print unless he was pretty certain of something and prepared to stand behind it and feel it was worthwhile putting his name to, you tend to be rather conservative about publishing second-rate material and you know it would only be satisfying if you were, to some extent, proud of it. So I don't think everyone should have the right to publish everything they do — that is unreasonable, because most of it isn't worthy of publication.

Dr. Andrew

He was not allowed by his company to publish. He explained that this did not worry him because the work he was doing probably would interest only his firm's competitors and that, other than that, it was not of any real use to mankind. He did think that if his work became of any more general scientific interest, then he would like to publish it, although . . .

. . . I mean a lot of cases, publishing things is just an ego-trip anyway. The people who publish — to a certain extent I'm guilty of the same thing I suppose — so it's not altogether clear that I would be annoyed at not being able to publish something; I don't know.

— Can you say when it's not an 'ego-trip' — are there circumstances when it's not just egotistical of someone to want to publish something ?

Well I suppose I can imagine situations when people are concerned with the advancement of their particular science rather than the advancement of themselves. In my experience, the latter seems to be the predominant motivation, in physics anyway. Maybe I've been working with a particularly poor bunch of physicists, but that seems to me . . . people are basically playing the system, publishing as much as they can to try and get on as far as they can, I suppose. But presumably, there are cases when people publish because they think it's useful to other people to actually see what they've done. I'm sure the majority of people would claim that's why they publish, but I'm not convinced.

— So what would be your attitude, for instance, to a situation where there was a scientist doing some fundamental research in industry which, under your definition, would be worth making known to other scientists because of its scientific interest, who wasn't allowed to publish ?

If it really was for scientific interest, I'd be very unhappy not publishing, but I think there are probably very few pieces of work that are crucial — where that situation would genuinely arise. I think the person would like to publish in other cases, but whether it was actually of paramount importance to the progress of the particular field that it was published, I'm not sure. You know, my suspicion is that it probably wouldn't make a ha'pworth of difference with the majority of pieces of work, because most of the progress is a sort of bulk effect, where everybody shuffles along and they all gradually shuffle forward There are few Einsteins and such people around who actually make a leap forward in one go.

Scientists who did not want freedom to publish

Dr. Forres: who did in fact have some freedom to publish

— Does the question of publishing ever arise ?

Well it hasn't yet, though I'm sure they encourage people to publish.

— Do you want to ?

No, I'm pretty indifferent to it.

— Can you say why ?

Well because if you want to get on in a university career, you have to publish, but it's just more work really, isn't it. I'd rather spend the time doing something else.

— Are you satisfied with their publishing policies ?

Do you mean, would they give you time to write papers ? Yes, they do. Yeh, I mean some people do publish from time to time. Yeh, I think it would be difficult to actually publish things I'm working on, because if we came up with anything that is significant in that sense, we just patent it — either that or try and keep quiet about it anyway. But it doesn't worry me.

— Would there ever be problems for instance if you went to a conference and had to keep quiet on the sort of work you're doing ?

Yes, I mean it does.

— Is it a problem for you or would it be ?

I mean it's a problem to the extent that sometimes it can be a bit niggling. In fact I probably wouldn't have any qualms about telling university people anyway. I mean obviously you wouldn't go bursting out to them exactly what you're working on, but I think that's common sense probably. I'm not a hundred per cent a company man, but I'm that much of a company man that I wouldn't go shouting my mouth off about what I do to other people.

Dr. Heriot: whose freedom to publish was rather limited

[Publishing] does arise. Obviously it's a lot more limited than in an academic environment. It's all got to be protected. I suppose if anything is published, then it means it's a failure, but things are published within the company. If a topic is in such a state that it would normally be published, then it's written up in the form of a report and circulated round the company and maybe, patent rights allowing, it would be published.

— Do you want to publish ?

I feel no great desire to get my name in print, no. Obviously I want to publish in the sense that any worthwhile scientific work is published. I want to do worthwhile scientific work, but the fact whether it's published or not doesn't really bother me.

— You mean publishing could be used as a criterion of whether the work was worthwhile scientifically ?

Yes. It could, yeh.

— Is it likely to arise ?

It does arise quite frequently, yeh. People do publish their work.

— And are you satisfied with the company's policies on publishing ?

Yes, I think so.

— You don't think there may be occasions when you will want to publish something and they won't permit it.

I can't envisage any such occasions, no.

— What about other people ? Are you aware of any problems about this within the company ?

No. Several people have had articles published and I don't know of anyone who's been refused permission to publish. I think if they were refused permission to publish, they'd be quite happy, because it would mean that the compound they've made is potentially useful and heading for the market and if so, that's much more worthwhile than publishing papers. And I suppose if a compound is found not to be clinically useful, then you can go back and publish what you did and it'll just have to be a few years later than it would have been originally.

Dr. Ainslie: who neither had nor wanted freedom to publish

[Publishing] has been known — not recently. If it's general basic scientific work, yes.

— And what about your work: will you . . . ?

Not any of the work I'm doing at the moment; can't see that at all. I don't find that a great worry at all. It never really bothered me at university the number of papers I could get out of it. I saw enough people who were what I call 'handle-turners', and I had friends who worked for them and all they did was just extract the maximum amount of work to gain papers to enhance their scientific reputation, without particularly looking after the people who did the work for them, and that was probably what turned me against that type of approach. And within an industrial concern, your work is . . . well usually of limited . . . certainly of quite a lot of commercial interest. So if I was to go to another company now, quite a lot of the work I'm doing would go down quite well. Scientific interest ? No. Science seems to be . . . I have the impression that universities seem to be interested in science for science's sake — not for industry's sake. You know, the fact that you can use a certain enzyme to culture and make a yoghourt does nor seem to be of particular scientific interest. The actual behaviour of that culture with temperature PH and other properties is; but no, it doesn't bother me at all. It quite honestly wouldn't bother me if I never had another paper out in my name.

— And are you satisfied with their publishing policies ?

Yeh, yeh. We're in an industrial research laboratory to put ourselves in a technically better position than our competitors and if we're told that by publishing, we endanger that position by which we're earning our living, yeh, I agree completely. You know, you're sort of biting off your own hand. A lot of the work outside our section . . . which is of general . . . they have groups which are equivalent of any university group you can think of. They've got world leaders in several fields — bacteriology, structure of water, polysaccharide chemistry. They're world leaders: they can go to any university department and talk to the profs and give lectures. You know, they need this for their type of area. The work they're doing is of pure scientific interest and, as such, they do get quite a few coming out as published work. Not me personally.

DISCUSSION: FREEDOM TO PUBLISH

Of all the norms in the 'ethos of science', freedom to publish is the one that has been given greatest emphasis in the literature. Moreover the imperative to publish and communicate freely with other scientists has been presented as being the greatest source of normative conflict between the industrial scientist and his employing organisation. The findings of this study that only three out of the forty scientists wanted more-or-less complete freedom to publish contrast strikingly with most of this previous research.[3A]

The illustrations from the interviews show that the reasons the scientists gave for wanting or not wanting to publish could be quite involved. To what extent did these reasons accord with the 'ethos of science' ? Were they expressions of the norm of communality ?

First of all, not one of the scientists thought that scientists should have complete freedom to publish their work in all circumstances. Even the scientists who most strongly upheld the ideal of freedom to publish could imagine circumstances where they thought the restriction of publishing would be legitimate. For example, Dr. Merchiston conceded a right to government to classify some of its work as secret and similarly Dr. Marchmont also tacitly accepted that right, although he would not himself want to work under such circumstances.

[3A] e.g. Box and Cotgrove who report 31% of their sample attaching importance to freedom to publish. See table 5.2, p. 95, in Science, Industry and Society.

There were three people with less freedom to publish than they would have liked ideally, though in the case of two of them, the discrepancy was very narrow indeed. These were the cases where 'value conflict' would be predicted by Merton's theory. And indeed in all three cases, the reasons they proposed for wanting freedom to publish were in line with the 'ethos of science'. The views of two of them, Dr. Marchmont and Dr. Andrew, are illustrated above.

The purity of the expression of the norm of communality is particularly noteworthy in the illustration from Dr. Marchmont's interview, in the way he emphasises the international character of science. In the other two cases, the expression of the norm is much more attenuated. Dr. Andrew gives some expression to it insofar as he envisages scientists publishing because of their concern for the advancement of science. Much more conspicuous in his view, however, are the numbers of scientists for whom publishing is just an "ego-trip" — a way of advancing themselves. In a sense, this view is thoroughly Mertonian in its emphasis on personal disinterestedness. The other scientist, Dr. Nicholson, whose views have not been quoted in this context, would have liked more freedom to publish, because he felt communication with other scientists would be interesting and increase understanding of a problem that was not well understood. However, while this desire seemed thoroughly Mertonian in its motivation, it was only a desire. Dr. Nicholson in fact laid considerable stress on the right of the company to have first claim on any work he did. By this he meant that "they should be able to apply it and make money out of it. It'd be quite wrong", he said, "if you did all the work, published it and someone else took it and made all the money out of it". Such respect for the company's interests is, in fact, very typical of the vast majority of the sample regardless of their desires concerning publishing. Indeed, far from merely having a place in their views, the company's interests usually had first priority. The priority of the company's interests were given prominence in the replies of 36 out of the 40 scientists interviewed.

The reasons the scientists gave for wanting to publish fell into three main types:

(1) There were Mertonian reasons: e.g., publishing is fundamental to the dissemination of knowledge and only by publishing can you make your work available for the critical scrutiny of other scientists (Dr. Merchiston). Also in this category were less elaborated comments linking science and publishing, but not on the basis of any clearly articulated reasons.

(2) There were reasons to do with recognition and career advancement. Very often the way such reasons were expressed were quite alien to the spirit of Merton's norms (e.g. Dr. Dundonald) and whether or not they are adduced as support for the norm of communality is a matter of argument. Merton himself specifically warns against confusing institutional and motivational levels of analysis and emphasises the institutional nature of the norms of science.[4] In this view, the reasons a scientist had for conforming to a norm would be immaterial and his conformity as such and its

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[4] Social Theory and Social Structure, p. 558.

institutional consequences would be the important point. In fact, having made this point, Merton goes on to imply that, as a matter of fact, scientists usually have internalised the norm of disinterestedness and made it part of their motivation. Certainly later writers, such as Barber and Storer have developed the theme of disinterested behaviour in such a way as to rule out behaviour that is self interested in its motivation. In their terms, Dr. Dundonald's motivations could not be said to conform to the 'ethos of science, even though his behaviour might.

(3) The final type of reason the scientists put forward for wanting to publish was utilitarian, for instance, that it would be good publicity for their company or that the information they had would be useful to somebody in practical terms.

Overleaf is a table of the number of times each reason for wanting to publish was cited. They are broken down according to the extent to which the scientist citing them himself wanted to publish.

What is perhaps surprising about TABLE 128 is its relative sparseness. From the evidence of previous writers on the subject, it might have been expected that out of a total of forty academically trained scientists, more than twenty of them would put forward positive reasons for wanting to publish. Even then, only seven of them proposed Mertonian reasons. The rest while being consistent with the norm of communality were not contributed in the spirit of it.

TABLE 128

FREQUENCY WITH WHICH DIFFERENT REASONS FOR WANTING TO PUBLISH WERE CITED

Reasons for wanting to publish	Whether scientist wanted to publish			
	Yes	Qualified	No	Total
'Mertonian'	2	4	1	7
Career advancement, recognition, etc.	1	2	2	5
For company's sake, e.g. it's good publicity	-	4	1	5
Practical Utility	-	4	-	4
Total	3	14	4	21

(Note: only twenty scientists put forward positive reasons for wanting to publish. One of them appears twice in the table, having given both Mertonian and career reasons.)

In addition to these twenty scientists, there were a further five who expressed some desire to publish, but who gave no explicit reasons for wanting to do so. In every case, such concern as they had with publishing was overlaid with a greater concern for the interests of their employers. Their views on publishing were expressed in a weak form: "it would be nice to publish" or "I should like to publish if . . . " it did not endanger the firm's position. There was thus no moral imperative involved, but rather the idea that publishing was a pleasant side-line.

The reasons the scientists gave for not wanting to publish are given below in TABLE 129.

TABLE 129

FREQUENCY WITH WHICH REASONS FOR NOT PUBLISHING WERE CITED

Reasons for not wanting to publish	Whether scientist wants to publish			
	Yes	Qualified	No	Total
Apparently Mertonian: e.g. too much gets published nowadays, shouldn't add needlessly to piles of verbiage, amount published is bad for science.	-	7	6	13
Work not suitable: e.g. not good enough, not academic enough, not polished enough, not interesting to anyone else.	-	10	3	13
Against Industry's Interests: e.g. don't want to give away firm's secrets or endanger their competitive position, publishing not consistent with capitalist system.	-	16	11	27
Just not relevant to Industry: e.g. just more work with no advantage, most work that is significant in industrial terms is not published.	-	6	3	9
Career: e.g. publishing is all wrapped up with academic advancement, not industrial advancement.	-	4	6	10
Security: e.g. Official Secrets Act, etc.	2	1	3	6
Total	2	44	32	78

It will be noticed that some of these reasons for not publishing have been classed as 'Mertonian' — that is, based on the ethos of science. Some of the scientists argued that some publication does not further the development of knowledge, but hinders it by clogging the channels of communication. In the discussion these cases will be dubbed 'paradoxical' and this term will cover the first two classes of reason cited in the table.

The frequency with which the scientists gave reasons against publishing was far greater than the frequency with which they were given in its favour, amounting to 78 in all. Looking at the reasons in more detail reinforces the overwhelming impression that the way the scientists thought about publishing was far removed from the ethos of science.

The table shows that the reasons fell into six groups which were used to reinforce each other. Numerically the largest group of reasons were those emphasising the company's interests and the importance of not endangering them (for example, Dr. Inglis, p. 116; Dr. Heriot, p. 121; Dr. Ainslie, p. 122). Closely related to these, but slightly different in their emphasis were the reasons stressing the irrelevance of publishing to the idiom of industry (for example, Dr. Forres, p. 119; Dr. Heriot, p. 121). In fact, the assumption that one would not want to do anything against the company's interests is implicit in this reason and utterly taken for granted — the implication being that these scientists wanted to succeed in industrial terms and publishing was not the way to do that. The same type of motivation was present and formulated more explicitly in the 'career' reasons. The people who gave this reason saw publishing purely as a way of getting on in the academic world and not an appropriate way of getting recognition to help advancement in industry. All these reasons concerning the interests and idiom of industry and career considerations would seem to be antithetical to the norm of communalism. Not only is the imperative not to publish, but the reasons for not doing so are thoroughly worldly, giving as they do the greatest priority to the competitive positions of capitalist enterprises and the career concerns of these particular employees.

Standing alone as a reason was that of national security and work coming under the Official Secrets Act. Two of the scientists who wanted freedom to publish conceded this as the only reason for not publishing that might be legitimate (for example, Dr. Merchiston, p. 106). The other four scientists who put forward this reason (one example, Dr. Buckstone, p. 114) were less grudging in their admission of this reason to their class of legitimate reasons for not publishing and one, in fact, gave it great emphasis.

The only feature of these results which might seem to go against their overwhelmingly negative significance for Merton's theory are the 'paradoxical' cases mentioned above. Here reasons which appear to be in the spirit of communality are presented in favour of not publishing — that is, not doing precisely what has usually been associated with communality. Some further illustrations will help to give the flavour of these cases.

Dr. Drummond (who spoke for two pages on the subject)

. . . now I firmly believe that while there are some people doing useful work and publishing it . . . well I suppose we're using vague terms: 'useful' — useful to whom ? and for what ? — but there are people doing good work and publishing it and important scientifically, technically, commercially etc., but for each one of these, there must be thousands churning out the most irrelevant rubbish, solely to pursue their academic careers in universities.

Dr. Ashley (this extract was part of a five page development of the theme.)

. . . people at university, certainly the people I've come into contact with — I admit it's a limited contact, because I've only been there three years [his first degree was a Grad.R.I.C. done on day release] — but they've got to publish. To publish is the important thing — it doesn't really matter about the quality of the work. Obviously there are some very scrupulous people about who will only publish sound scientific facts, but other people publish very rapidly and very quickly. As soon as they discover one thing, they publish. In the course of the same project, if anything else comes, they'll publish another paper and this is because their sole existence depends on publishing. I'm sure that when people come up for jobs at universities, they say, "how many papers have you published ?", not, "what sort of papers have you published ?", but, "how many papers have you published in the last year ?" In fact this is one of the questions on their application forms and that's not good for the people concerned; it's not good for the universities; it's not good for science at all.

Dr. Warriston

One of the reasons I went into industry was that I was frustrated in that initially I went into research with high ideals of eventually going on into academic life and after one year of academic research, I was disillusioned and didn't . . .

— Why was that ?

Mainly because . . . it stems again from the type of work . . . well you know it was a lot of things — the type of work that was involved was totally academic and people were just engrossed in one small field. Also the amount of back-biting that goes on . . . well I say within academic departments. I know people in other departments who say this too, but I can only speak personally of one department. The kind of promotion stakes, the publish or be damned attitude, the way generally supervisors treat their students — you know, as stepping stones for their own success, because their success lies with their research students, because it lies with research papers — the number they publish, you know, and the quality, I suppose — reflects on them, and only through academic success, rather than through teaching ability, interest in the subject, interest in what chemistry has to offer to an undergraduate, what it should offer, what it should give as an education at degree level — it doesn't really matter.

Dr. Wemyss

. . . I think in chemistry alone there's an average of three or more journals come on the shelves every year, so they're creating journals all the time. One of the difficulties is that it makes getting the interesting work hard. Interesting papers, useful papers become difficult — physically difficult to find — you know, you just wade through so much. Another thing, as I say, is that it's just a ~~sheer~~ waste of time — a sheer waste of time.

People are writing papers and other people are vetting them and sending them back and then they're writing them again and vetting them. It's all a waste of time; the end result is pretty trivial.

TABLE 129 shows that as well as giving idealistic reasons to justify non-publication, some scientists did not want to publish because they felt their work was in some way inappropriate. This was not, however, because they felt it was lacking in ultimate quality or significance, but merely that its significance was practical and local and, they felt, unlikely to be of interest to academic scientists.

Are these 'paradoxical' reasons in the spirit of the ethos of science ? To some extent, yes, because a primary concern was with the extension of knowledge. However, an even more significant feature of them was their condemnation of academic science with its production of knowledge for knowledge's sake. The activities of pure academic scientists tended to be seen as trivial. On closer examination then, these replies appear to be a vindication of the practical and utilitarian ethos of industrial science, rather than stemming from an unqualified concern with the extension of knowledge as such. The moral overtones in these replies are directed against the very institutions which are usually held to embody the ethos of science. This suggests that the 'paradoxical' cases should perhaps be seen not so much as a surprising inversion of the ethos of science, but as expressions of a more utilitarian moral viewpoint which stands in its own right.

lV. SUMMARY

In this chapter, the interviews with forty industrial scientists were analysed to see what support they gave to the thesis that scientists subscribe to the ethos of science. Attachment to the norms was searched for by looking at the scientists' attitudes to: autonomy, that is (1) freedom to choose projects and (2) freedom to work in their own way; and (3) communality, that is, freedom to publish.

The results were overwhelmingly unfavourable to the idea that scientists do subscribe to these norms. The primary values that emerged were pragmatic, commercial and utilitarian. Every effort has been made to locate and exhibit evidence in favour of the scientists being attached to the norms of science. In an effort to be fair to Merton's theory, undue emphasis has perhaps been given to isolated individuals and isolated utterances. What has emerged overall is that, at most, two out of the forty scientists (Drs. Marchmont and Merchiston) could be fairly characterised as having values in accord with the stereotype derived from Merton's theory. A summary table listing all forty scientists is given below as TABLE 136. It contains entries for every instance of a scientist subscribing to the norms of autonomy and/or communality in what appeared to be the appropriate 'moral spirit'. Only ten scientists have any entries at all by their names. The thirty blank rows constitute evidence against Merton's theory.

TABLE 136

NUMBER OF SCIENTISTS ATTACHED TO NORMS OF SCIENCE

Giving reasons in Mertonian spirit for wanting freedom : —

Individual Scientists	to choose projects	to work in own way	from fixed hours	to follow own ideas	to publish
Dr. Marchmont		+	+		+
Dr. Merchiston	+	+		+	+
Dr. Bernard	+	+		+	
Dr. Andrew				+	
Dr. Grange		+			
Dr. Lindoch	+				
Dr. Nicholson				+	
Dr. Ainslie			+		
Dr. Hart			+		
Dr. Buckstone		+			
Dr. Albany					
Dr. Annandale					
Dr. Ashley					
Dr. Babberton					
Dr. Brunswick					
Dr. Calton					
Dr. Carrington					
Dr. Chucklie					
Dr. Clarendon					
Dr. Clerk					
Dr. Dalry					
Dr. Drummond					
Dr. Dundonald					
Dr. Esslemont					
Dr. Forres					
Dr. Gilmerton					
Dr. Granby					
Dr. Heriot					
Dr. Inglis					
Dr. King					
Dr. Lauriston					
Dr. Mayfield					
Dr. Montgomery					
Dr. Moray					
Dr. Newington					
Dr. Polwarth					
Dr. Raeburn					
Dr. Ravelston					
Dr. Warriston					
Dr. Wemyss					

The weight of evidence against the value conflict theory can be further reinforced by looking at these results from another perspective. The great majority of the scientists in the sample were satisfied with their jobs as is listed below in TABLE 137, column 1. In column 2 of the table, the numbers in each category (of degree of satisfaction with work) are listed who had difficulties in their jobs which, according to any liberal definition, could be associated with the norms of science — that is, 'value conflicts'. Column 3 is the most important column in terms of rejecting the value conflict theory. It lists the numbers of scientists for whom (2) 'value conflicts' were the major element in their dissatisfaction. TABLE 137 shows that for scientists who were 'dissatisfied' or who had 'strong reservations' about their work, 'value conflicts' were not the major source of dissatisfaction; and where 'value conflicts' were the major element in a scientist's dissatisfaction, they, in fact, caused only mild dissatisfaction.

TABLE 137

CONNECTION BETWEEN 'VALUE CONFLICTS' AND DISSATISFACTION IN WORK

	1	2	3
Degree of satisfaction with work	Numbers of scientists	Scientists with difficulties associated with norms of science	Those for whom (2) was a major element in their dissatisfaction
Dissatisfied	2	1	-
Having strong reservations	4	2	-
Having mild reservations	8	3	3
Satisfied	26	0	N.A.

The conclusion must be that looking at industrial scientists as being under the sway of a system of abstract values derived from their previous academic training is simply unprofitable. In particular, there is no evidence that this sample of Ph.D. scientists were more effectively socialised into the values in virtue of their greater exposure to the alleged agents of socialisation, than were the mixed or first degree samples used in the previous studies which have reported equally negative findings.

The result of searching the data for any utterance or attitude which might accord with the ethos of science is that no positive picture has emerged of how the industrial scientists do operate. The effect of adopting the perspective of the value conflict theory has been that the internal coherence and pattern of the data has not emerged. The categories of the abstract norms so cut across the reality of the thoughts and utterances expressed in the interviews that — in a manner of speaking — the data falls to pieces in one's hand. The aim of the next chapter will be to lay aside the theoretical perspective adopted so far and approach the data more inductively, that is to say: without preconceptions about the values most functional for the production of knowledge or about the priority of academic science. The aim will simply be to see how industrial scientists do operate.

CHAPTER V

THE INDUSTRIAL SCIENTIST AND HIS WORK

The aim of this chapter is to provide a positive picture of the industrial scientist and his work. Anyone who has encountered the industrial scientist only through the writings of the value conflict theorists and who thinks of science only in terms of the ethos of science will be totally unprepared for the reality of industrial science — its diversity; its intrinsic satisfaction for its practitioners; the character of its routines and its subtle links with academic science. Some attempt must be made to convey this reality, in order to understand the results of the last chapter — why, for example, only two out of forty of the scientists were attached to the norms of science in any wholehearted way; why there was so little dissatisfaction, and why this was not in any case connected with the norms.

This will be done under four headings which will now be briefly outlined. First, how does the industrial scientist get into his job and how does he view that job ? Does he arrive in industry reluctantly, having failed to stay in the academic world and anxious to escape at the first opportunity, or does he arrive willingly and with every intention of pursuing a career of the sort provide by industry ? Data will be presented to show that the latter is the case.

Second, having arrived in industry, how does the industrial scientist work — in particular, what is it about his work which makes questions of freedom to choose projects of so lttle interest and freedom to publish irrelevant ? Here it is necessary to appreciate the diversity of roles covered by the title 'industrial scientist'. If the details of the scientists' real working situations are examined, then it becomes apparent why academic values are so irrelevant. Detailed descriptions of individual cases will be offered in an attempt to break down the stereotypical view of the industrial scientist derived from the work of the value conflict theorists.

The third point concerns the recognition or rewards that motivate, steer and reinforce the behaviour of the industrial scientist. Data will be presented to show that these industrial scientists are autonomous, skilled specialists who work largely without explicit recognition to reward or motivate them. The theoretical implications of this claim will be worked out in the final section of the chapter. For the moment, the point to notice is that their independence is a natural concomitant of their possession of marketable and useful, esoteric knowledge. Explicit reward processes may be appropriately sought and found in trying to understand less highly qualified scientists, but such is the competence and skill of Ph.D. scientists that they can be seen as 'self-regulating mechanisms'.

The fourth point to be made concerns the matter of precisely what the resources are with which a Ph.D. training endows a scientist. The stereotype of the Ph.D. bringing to industry an inappropriate academic bias and a desire to stay near to his special field has already been noted. In a similar vein, Box and Cotgrove focus on the extent to which a scientist is "committed to science" as distinct from merely "instrumentally involved" in his work. Furthermore they gratuitously align this distinction with that between pure and applied scientific work.[1A] According to the findings of this study, commitment is a very complex phenomenon and has little to do with the pure/applied dichotomy. Industrial scientists can be as dedicated to solving the problems of industrial science as any academic is to those of pure science. An attempt will be made to convey the style of these scientists' fascination with their work.

[1A] See Science, Industry and Society, pp. 19, 27, 31, 26 and especially the preface, p. vii.

1. JOB CHOICE AND CAREER ORIENTATIONS

The way the scientists talked about choosing a job shared two features with the way they talked in general. First, what they said was very dependent upon the situation they were in. For the most part, they did not talk in terms of some ideal set of requirements they would look for in a job. Their preferences were integrally related to their perceptions of the jobs that were available and that they stood a chance of getting. Asking them what job they would have preferred ideally, given a better job market situation, was often felt by them to be an irrelevant question. They saw such an issue as idle and unreal and were not prone to entertain thoughts about it. The second striking feature of the way they talked, which is partly a consequence of the first, is that it can be very difficult to separate out what they said about any one issue and consider it in isolation, because most of the elements in their views were related to other elements. In this connection, for instance, they discussed the business of choosing and getting a job in relation to how the job had subsequently turned out; how they saw the role of their Ph.D. training and its influence in the process; their thoughts about pure versus applied science and how university research compares with industrial research, and at what point in the spectrum they wanted to be. They also discussed the initial choice in relation to any overall career strategy they might have. Above all, they discussed the state of the job market. For these reasons, the presentation of results inevitably contains considerable simplifications.

TABLE 143 shows how many of the scientists thought, on looking back, that they had got the job they wanted at the time.

TABLE 143

EXTENT TO WHICH PREFERRED JOB WAS OBTAINED

Numbers of scientists getting : —	
(1) Exactly the sort of job they wanted.	10
(2) Very close to the sort of job they wanted.	10
(3) Near enough sort of job wanted, but some reservations about it.	15
(4) Not really the sort of job wanted at all.	5
TOTAL	40

This table does not refer to how satisfactory their jobs turned out to be — merely the extent to which they remembered them being what they wanted at the time. When differences occurred between the initial preference and how the job subsequently turned out, these were, except in one case, in the direction of the job being better than expected. Five of the people who had felt reservations about their jobs at the time of accepting them, subsequently found them more satisfactory than they had expected.

As might be expected in a sample of Ph.D. scientists, the possibilities and attractions of academic posts cropped up fairly frequently, but not however, always in favourable terms. There was only one strongly disappointed would-be academic in the sample and five other people who would ideally have preferred an academic job, but expressed this preference only weakly. Everyone else thought of university posts either very much on a par with other jobs and not particularly more or less attractive, or did not consider such jobs at all. The preferences of the majority were overwhelmingly in the direction of having an industrial job.

The reservations of the people who did not get precisely the job they wanted were both fairly specific and expressed in terms of a comparison with the job they got. To show their specificity and situational dependency these are listed below.

1. Would really have preferred an academic psot, but did not fancy fellowships because of their insecurity.

2. Wanted to do armaments research, rather than artificial fibres — not more related to his Ph.D. — just something in which he had always had an interest (he was in fact offered such a job, but turned it down, because he felt he could not afford to live in the South-East).

3. Was offered a fellowship in America which he would have preferred, but the security and salary of this industrial job were too tempting to be turned down.

4. Would have preferred an industrial research job or the factory inspectorate (was working in technical service).

5. Would ideally have liked to do instrument development (rather than dairy research).

6. Wanted to do similar work, but either with a nationalised industry or possibly in a university (was working for private industry).

7. Would really have preferred a university post or to do research on drugs for arthritis.

8. No definite preferences, but might have preferred a teaching job (was doing research for an oil company).

9. Wanted a similar job in technical service, but in the chemical industry (he was working in the packaging material industry).

10. Would really have preferred a job closer to his Ph.D.

11. Did not really want to work for private industry.

12. Wanted very similar job, but in a more specialised firm.

13. Failing a university post, wanted to do similar work to his present job, but might have preferred to doiit with the Water Board or in forensic science.

14. Really would have liked to do pure research (rather than the development he was doing), but in the same sort of firm, i.e., pharmaceuticals.

15. Wanted to do work in his Ph.D. specialism, but not necessarily purer and certainly not in a university.

Here also listed are the preferences of the people who were very dissatisfied with the job they got.

1. Wanted university job, or failing that, research rather than data analysis.

2. Would have liked a job, e.g., with the National Environmental Research Council carrying on his Ph.D. topic more-or-less, or a job in medical physics.

3. Wanted a job more closely related to his Ph.D.

4. No clear ideas — wanted to do"something useful".

5. Wanted to make more use of his Ph.D.

The impression to be got from these lists is of people who, for the most part, were quite satisfied with the type of job they had got, but who would have liked to make fine adjustments — to work in a slightly different area, with a slightly different emphasis or in a different sort of firm.

The scientists' satisfaction with the fact that their jobs were in industry was picked up by their more long-term career orientations. These were almost entirely couched in industrial terms and are set out in TABLE 146.

TABLE 146

THE SCIENTISTS' LONG-TERM CAREER PLANS

1. Very management orientated — having both the goal of management in mind and a fairly clear sense of how they intended to reach it.	6
2. Fairly management orientated, but opportunities and specific routes towards it not yet investigated.	8
3. Probably aiming towards a commercial position and certainly having a clear idea of the route to it.	3
4. Thinking of moving into production within the next 2-5 years, then seeing how things go from there.	5
5. Wanting to stay and become more expert in some capacity in present non-research field.	4
6. See their future mostly in terms of carrying on in R & D, only perhaps (and hopefully) moving up the hierarchy.	10
7. Would either like to stay in present industrial research field or if that fails to turn out well, would try to get back to the academic world.	2
8. No ideas.	2
Total number of scientists	40

As can be seen from TABLE 146, the scientists long-term career plans were very unstereotyped. It is as if each scientist were surveying the scene from his own idiosyncratic vantage point and combining some notion of the possible opportunities lying ahead with some assessment of his own talents (or potential developable talents) and preferences. Having had their present jobs for only about a year on average, their knowledge of what possibilities there might be was as yet fairly limited, consisting in general outlines rather than many specific details. However, interestingly enough they did not respond, for the most part, to this lack of knowledge by pinioning on to the familiar: they remained characteristically open, their general orientation being set, while waiting for the knowledge, or in some cases the opportunities, to arise so that their outline plan could be activated in specific circumstances. In fact, the table just presented, because of its nature as a summary, tends to exaggerate the definiteness of their plans and understate the extent to which they were actively investigating various options.

As a group, these scientists tended to be fairly ambitious — 'the way ahead' being closely integrated into their perspectives. Twenty six of them emphasised career advancement explicitly in their priorities and most of the other fourteen had obviously given the matter careful thought, even if they were not as obviously ambitious. Most of their discussion of their long-term career hopes was a subtle blend of what they found interesting and what would be most advantageous for them in terms of "getting on". They weighed up job content against opportunities for responsibility and advancement, but often talked about being prepared to leave science if it would help them to advance. Indeed they very consciously manipulated their scientific training in their plans and saw it as a marketable commodity which could give them the edge over various other candidates for certain prized jobs. Although their desire to stay in scientifically or technically based industries was often expressed in terms of the intrinsic interest of the work, it was also reinforced by the fact that that was where their skills and qualifications lay and probably therefore their greatest opportunities. Often they expressed a willingness to move out of science in theory, but in practice their career ambitions and their involvement in technical matters were unlikely to be in competition and thus face them with such a decision.

It is a common view that 'pure science' has both more intrinsic interest and more status than 'applied science' or 'technology'. Interestingly, the views of these Ph.D. scientists showed no tendency to reflect such distinctions. When asked whether they would be prepared to move out of science, very often they seemed to have no clear sense of where a boundary between science and non-science might occur. The question was sometimes thrown back with a request to define "science" and what counted as "moving out of science". The question seemed irrelevant to their ways of thinking and indeed barely meaningful. This was the case both for those scientists who saw their careers largely in research terms and those with other plans. The point was not that they were unconcerned about their job content, but that commitment to pure science, as such, played no part in the way they thought about their work or careers. Insofar as a definition of science can be inferred from the way they talked, it would have to be very broad and include a great deal more than the research activities that are usually associated with university science departments or the small number of industrial laboratories where 'fundamental' research is carried out.

11. ORGANISATION OF WORK

The role of 'research scientist' as it is conveyed by the value conflict theorists is abstract and stereotyped. It fails to engage with the variety and complexity of situations in which such scientists work. As a corrective, the way the scientists in this sample worked will be looked at in some detail.

Although a large number of the jobs they did (N=29) fell into the category of research and development, this general designation covered a multitude of fields, tasks and ways of organising work. The major sources of variation in the way the scientists worked were:

(1) The way projects were initiated, i.e., whether the scientist himself decided what to work on; whether projects or fruitful lines of research were decided upon in consultation with others; or whether projects were just handed out by a superior, etc.

(2) How the scientists worked, i.e., alone; in a team etc.

(3) The way in which progress in work was evaluated, monitored and recognised.

The first two topics will be considered in more detail in the rest of this section. Recognition is a sufficiently large topic to be considered separately in a subsequent section.

(1) INITIATION OF PROJECTS

The major ways in which projects were initiated are listed in a simplified fashion below : —

1.	Told exactly what to do	1
2.	Was employed to work on a particular project or take over an area of work.	4
3.	Deals with customer complaints or other problems as they arise on a consultative basis.	4
4.	Projects originate (in e.g. sales department) with customer requests. Scientist may also think some up himself.	4
5.	Short-term problems (e.g. from production) dealt with as they arise. Other projects come through section leader.	4
6.	Given projects (by e.g. section leader), then left to get on by self.	7
7.	Work comes through boss, but can decide to some extent whether or not to follow something up and how far.	2
8.	Allocated to group or section working in certain area. Then work on own initiative within that area.	4
9.	Decided what to work on by choosing to work in a particular department.	1
10.	Working as a consultant — two-way selection of problems on the part of both himself and customer.	1
11.	Formulate projects or decide which lines to pursue in consultation with others (e.g. boss, colleagues etc.).	6
12.	Not applicable — don't do projects as such.	2
	TOTAL	40

The fact that this list is simplified needs to be emphasised: there were in fact almost as many ways of getting and running projects as there were scientists and they talked in considerable detail in their interviews about the exact mechanisms by which the content of their work was decided upon. Nevertheless there is sufficient information in the list to illustrate certain important points about the initiation of projects.

First, to talk in terms of doing 'projects' is not always appropriate in the context of some of these scientists' jobs. For example, the two production managers did not do projects of any description: they were responsible for managing a group of people working on particular plant on the factory floor. They might have to co-ordinate efforts to do a particular job, but doing projects, let alone having choice in them, simply did not arise. Similarly there were two people (in item 2 in the list) who were employed to work on a specific area of work where the doing of projects as such did not occur. One was analysing data from a power station in order to monitor its efficiency and maximise the chance of correcting design faults in subsequent models. The other had been employed to take over a sub-section of a company when the manager retired. He was reading in the area and generally trying to acquire the necessary knowledge and prepare himself for the post. So again, he was not involved in the doing of projects as such, although it was quite possible that he might be at some time in the future.

The other cases where it is not quite appropriate to talk in terms of 'doing projects' are for the scientists who were allocated to a group of people working in a certain area and then left to work on their own initiative (item 8 in the list), or who decided which lines of research to pursue in consultation with others (item 11). These people did not talk in terms of projects in describing their work. They worked by fitting themselves into an ongoing area of research and then pursuing what they judged to be worthwhile ideas. In these case the work was almost always closely related to their Ph.D. work and they could continue as if it were an extension of it. The term 'project' is both too grandiose and insufficiently 'organic' to describe this way of working — it suggests clearly specifiable boundaries to problems and the possibility of defining their beginnings, middles and ends.

The second point to be made is that it is not always appropriate to talk in terms of having or being denied 'choice'. The way some of the jobs were defined made the notion of 'choosing projects' absurd; for example, where scientists were employed to act wholly or partially as consultants or trouble-shooters who would solve clients' problems as they cropped up. Indeed when questioned about choice of projects, these scientists tended to be either scornfully dismissive or to repeat what their work involved — their assumption being that it was obvious that with that sort of job, you did not have choice over projects. If a customer had a problem, then it was your business to solve it and that was all there was to it.

In all the other cases, scientists did do projects, but having choice over what they did tended to be a non-issue. None had complete freedom to work on what he wanted, but many had some leeway and what they worked on would emerge from consultation and discussion with bosses, colleagues or some formally constituted group or steering committee. What emerged most forcibly from the way the scientists talked about the methods of initiating work was their eagerness to do work of value to their companies. For the most part, this concern was not seen as in conflict with doing work that was intrinsically interesting. Moreover it was never made subordinate to strictly scientific ends.

(2) WAYS OF WORKING

It is sometimes believed that scientists in industry are organised into fairly large teams, which rake exhaustively over a field, with each scientist having very little say either in the general direction of the project or in the way he should do his own little bit. Amongst these Ph.D. scientists, this was certainly not found to be the case. The majority were working very independently indeed and where they were working in teams, were usually in charge of them. The relevant numbers are set out in TABLE 154.

As can be seen from the table, the majority of these scientists worked very independently, although they rarely worked in isolation. Some of them worked in a set-up that could be described as federal, in that there would be a group of people of similar status and qualifications who would be working independently, but on related projects. The members of the federation would consult each other informally and discuss their work when necessary or interesting. They would sometimes also have more formal project meetings to discuss the overall progress of the research, interesting leads and possible future directions to go in.

TABLE 154

WAYS OF WORKING

	Scientists doing R & D	Other scientists
Mainly or completely alone.	3	1
Basically alone, but in consultation with others.	13	3
Working independently, but with technical help.	7	-
Working closely with boss (and technical help).	2	2
Working closely as a member of a team.	-	2
In charge of a small team - up to five people.	4	1
In charge of a large team (production managers).	-	2
TOTALS :	29	11

Alternatively, the nature of their work would demand consultation with people in various other parts of the firm. For instance, some of the scientists were working on projects which involved developing a product or process right through from the initial idea to a final manufacturing stage. In the case of pharmaceuticals, for example, this would involve a lot of consultation with other groups — clinicians, toxicologists, marketing men and ultimately people in the pilot plant or factory. Many of the scientists doing what could loosely be called research and development were involved in these extended networks of relationships with other departments and their activities were by no means restricted to their one corner of the laboratory. In addition, the jobs of some others specifically involved a service or negotiating aspect which involved them in very wide contacts either within or outside the firm itself.

The precise details of the way each scientist worked were discussed at length in the interviews and naturally they depended on the idiosyncratic organisation of the firms they were working for. On the whole they were satisfied with the way their work was organised. The complaints they did make were highly specific and very concrete — lack of communication with such-and-such a person, lack of technical help, lack of secretarial help, poor facilities, lack of desk calculator and so on. There was no question of matters of principle causing the trouble, only the routine snags that crop up in any job — each reflecting an individual scientist's idiosyncratic working environment.

111. RECOGNITION

According to the value conflict theorists, the desire for recognition is a major explanatory variable in understanding the workings of a scientist. It is this factor, Merton argues, that accounts for the frequency of priority disputes in science: recognition is the life-blood of a scientist and therefore his anxiety to establish his authorship of an idea or a discovery is no mere act of petty self-aggrandisement.[1] Developing this theme, Kornhauser argues that the very 'social system of science' is one which moves by the exchange of contributions to pure knowledge for rewards of recognition.[2] Hence the imperative to publish. On the level of motivation, it is assumed that a scientist will only work conscientiously if there is the promise of the reward of recognition. On the institutional level, it is assumed that science will only advance if contributions are made to it via published works. The latter point seems plausible, but has not in fact been systematically investigated. The importance of informal links in the communication of knowledge may well have been underestimated. This point is not going to be pursued here though. What is going to be questioned is the viewpoint that recognition has a crucial role as the major motivating force for a scientist.

For one thing, such a view leaves completely out of account the intrinsic fascination that work has for many scientists. The

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[1] "Priorities in Scientific Discovery".

[2] Scientists in Industry.

value conflict theorists tend to pay lip-service to this factor,[3] but in practice make no attempt to build it into their theories. Quite the contrary: by quite explicitly deciding to focus on the social organisation of science, they thereby deliberately ignore intrinsic motivators such as involvement in work. There is however another point which helps to account for the one-sidedness of their emphasis, and that is the crude behaviourist model of man employed by them. This meshes in with their functional perspective on society and sees man as learning and being motivated by rewards and punishments. They do not see him as an intelligent being who can acquire pure information about the world and then exploit it when it suits him as part of a planned strategy. Nor do they see man as someone who can get absorbed in things he does for their own sake — or at least, if they do, there is no official place for such propensities in their theories. The scientists of their imagination will be not only fixated on the reward of recognition, but recognition of a particular sort, that is, the recognition that accrues to the writer of a paper on pure science. More recently, commentators have recognised that industrial scientists may settle for recognition in a different idiom — namely that appropriate to industry — but nevertheless, they still lay heavy emphasis on the general point — that of the crucial role of recognition.

That a desire for recognition plays some part in a scientist's motivation will not be denied. What will be examined in this section though is how far and in what precise way

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[3]For example, Barney G. Glaser, Organizational Scientists, (New York, Bobbs-Merrill, 1964), p. xii, n. 3.

recognition was important for these scientists.

Like the previous discussions of the initiation and conduct of work, the most immediately striking fact about the category of 'recognition' is the diversity of processes that fall under it. These are summarised below.

TABLE 158

NUMBER OF TIMES VARIOUS FORMS OF RECOGNITION AND FEEDBACK WERE MENTIONED BY THE SCIENTISTS

1.	Formal assessment procedures.	26
2.	Internal reports (i.e. circulated or used in some way within the company).	23
3.	Technical discussion and consultation.	22
4.	Strong emphasis on relationship with boss as a source of feedback.	20
5.	Can tell from the work itself how well it is going.	16
6.	Merit increases in salary give some indication of whether you are progressing satisfactorily.	10
7.	External publications (i.e. scientific papers)	3

A detailed discussion of the variety of formal assessment procedures, promotion arrangements and salary scales will be left aside in favour of an account of what seemed to be the most important form of recognition to emerge in the interviews. This was what may be called the intrinsic feedback and recognition that was produced by the day to day conduct of the scientists' work.

The scientists did not depend on superiors or other significant people complimenting them or showing explicitly their recognition that a task had been well done: they relied for their sense of how well they were doing the job on all the technical preocesses and criteria by which the work itself would be evaluated (TABLE 158, items 2,3,4,5). Depending on the exact nature of the work, these could occur continuously while the work was in progress, or at the end. The important point is that the criteria by which work could be judged were, for the most part, quite accessible to the scientist himself and often more accessible to him than to anyone else. They were also fairly impersonal and objective. For example, if a piece of equipment had been designed and made, or a process developed, or a substance synthesised which met the required specifications, then a scientist did not need a manager to tell him that fact. If he chose to congratulate him, then that might be a very pleasurable bonus, but it would not be essential or even necessarily expected. In fact these scientists were remarkably autonomous with regard to recognition itself. They often required technical feedback as part of carrying out the job competently, but over and above that, they did not seem to require much explicit recognition. If the scientist knew he was doing the job alright, and this was not an area in which much doubt existed, then additional recognition was felt as a luxury and not usually one which they especially craved. It is of course possible that had praise, prizes and rewards been more abundant, then these scientists would have worked with considerably more eagerness and enthusiasm, but judging from the tones in which many spoke of their jobs, these qualities were not obviously lacking. Indeed the scientists seemed more-or-less self-regulating and self-motivating in their mode of operation.

However, recognition can be very subtle and the fact that, so far, the terms 'recognition' and 'feedback' have tended to be used interchangeably is no accident. Separating the two except in a formal grammatical sense is not at all easy with these scientists. Nor perhaps is there much to be gained from it. Certainly the scientists themselves showed no discernible tendency to separate the two. From the way they talked about their working relationships, it seems plausible to suggest that recognition could have lain implicitly for these scientists in any feedback they got. Every encounter in which the technicalities of their work were discussed, or even merely touched upon, in ways which implied that they were regarded as competent people who were to be taken seriously would confirm their sense of their own adequacy as scientists. As Dr. Ravelston said:

> . . . no one ever comes up to you and says, "you're doing a great job", or anything, but I know if my job is running on time, and the mere fact that I've never been to see the divisional manager or project manager about anything . . . you know, it's recognition enough to keep out of the way really. You can tell when things are alright. I'm sure it's only if things are going badly that you need some recognition of how valiantly you're struggling against almost insuperable odds.

Recognition then could reside implicitly in any encounter or event (or absence of them) which confirmed the scientists' sense of their own competence. These could vary from the everyday way in which they got along with bosses or colleagues, through to more organizational factors like their having considerable responsibility or autonomy. Many were gratified and clearly revelled in the amount of responsibility they had been given or the amount of independence they had to plan their own work, and organise and manage what liaising with customers, clients or anybody else they had to do. At the opposite pole, they were flattered by, for instance, people in the department looking upon them as "the physics expert" and bringing physics problems along to them. Many things then, both trivial and more substantial, could act in this way and to describe them at all adequately would demand a cataloguing of the type of work the scientists did in conjunction with a detailed review of the way in which they worked. Some schematic examples will now be given to give an indication of the sources of technical feedback and recognition.

Sources of technical feedback and recognition: Some schematic examples

Dr. Moray: doing scientific instrument research — has almost complete autonomy. Does all his own liaising with customers whose specifications his instruments have to meet. They provide the feedback. Otherwise can get plenty of advice within the company if he wants it.

Dr. Wemyss: analytical chemist working almost entirely alone. Never sees his boss unless he goes to see him himself, but he (the boss) always listens to and takes seriously any suggestions he might make and that makes him feel appreciated.

Dr. Hart: working as a development chemist in the textile industry. He's directly answerable to a section leader; otherwise he's directly associated with only one other colleague — they work separately on associated problems. Would welcome more discussion like at university. They have meetings with the section leader every two months when they cover all aspects of the work (not only his). Is monitored by his section leader mainly by informal discussion. Either he will come and ask how things are going or Dr. Hart will go himself and tell him if anything especially interesting arises. He usually writes bi-monthly progress reports plus additional ones if anything interesting emerges, and then a big report at the end of the project. Reckons that after six years training you can tell for yourself whether the work is going alright.

Dr. Clarendon: working as a technical assistant in a paper mill. Writes up the evidence from what he has done in a report which gets circulated, and then tries to persuade relevant people to implement his proposals. People are very friendly and "obviously you know whether you've satisfied people", though the section head or technical manager do encourage or remark on the work. Management is pretty responsive to suggestions and is good at keeping them in the picture. It's easy, he says, to lose sight of how your work is helping the company, but if you want to know anything, you have only to go and ask.

Dr. Ainslie: research chemist in the dairy industry. He is answerable to the section manager. Sees quite a lot of him because they work very closely together. Projects are written up in reports, these are considered by relevant departments, possible modifications suggested — usually by marketing people — and then it just goes ahead up to the stage of production of nothing stops it.

Dr. Grange: consultant instrumentation engineer. Works to a specification supplied by the customer, so there are no doubts about when he has achieved them. Also a lot of people come and ask him questions on the pure physics side which he finds very flattering. Boss is very friendly, but has a policy of leaving them pretty well to their own devices. Kept up to the mark by the client rather than the boss because he has project mettings with them. Gets indirect feedback by people being recommended by various people, including clients, to go to him. The hardware at the end of the job is indisputable feedback — as he says, "very nice and cut and dried".

Dr. Montgomery: working as a computer consultant. Gets his main feedback in the opportunities and responsibilities he is given. The real feedback comes in what the next piece of work is.

Dr. Annandale: research chemist. Works very closely with his section head, though not really answerable to anybody. Goes to head of department if he wants advice. Have project meetings when four of them fight out what they are going to do — there is no one with enough experience to give them a clear line to take. Writes progress reports every four months and has quarterly meetings (which are quite an ordeal) when each one gets up and says what he has been doing and the work is discussed.

Dr. Andrew: research and development physicist. Boss adopts "lead in from behind" approach, which means he asks for and acts on their advice. If you have problems you go to him; otherwise it is assumed you do not have problems. His results are reported for internal circulation. Feedback is very immediate — "the thing works and money starts rolling in". It's an American company and visiting Americans come for whom they give seminars, so you get feedback from higher levels of management, which is very helpful. Immediate feedback from boss who would tell you if you were not doing too well. Also you would never be asked to give a seminar unless your work was up to a certain standard.

Dr. Gilmerton: systems analyst. Liaises with people wanting the results, i.e. an internal customer. Also works in fairly close contact with his boss who has overall say, and there is fairly close feedback there. Has to keep costs etc. to estimate and he is closely monitored in that. Lots of feedback from customers in early stages and later when project is running, there is contact if problems arise.

Dr. Babberton: power source investigator. Just two chemists in the firm — him and his boss— and they work very closely together and get on very well. Same applies to his boss' boss, who is head of R & D. Writes reports on what he is doing, then discusses it and boss makes suggestions. He also goes and tells him if anything interesting crops up. The managing director also comes in and chats which is sometimes a nuisance, but good because it means he is in touch. Very good happy firm to work for.

These examples show how recognition was not seen by the scientists as a discrete process or happening, but was built into the role of a highly qualified scientist — to play the role competently in an appropriate setting was to be recognised. Viewed in another way, the processes of feedback and recognition operated to sustain and reinforce the scientists' identities as highly qualified scientists. These identities were by no means perilous as was indicated by the confidence the scientists had in their own abilities and performances. Thus lack of 'extrinsic recognition' did not undermine their sense of their own competence — it simply constituted a grievance for them. Similarly, adverse criticism by superiors was not felt to be especially threatening, but could be absorbed without undue damage to their self-esteem.[4] The scientists' ability to work confidently without explicit recognition was based very securely on their Ph.D. trainings. These provided them with the technical resources to do their jobs competently and the criteria by which they could make valid judgements of their own performances.

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[4]How resilient their confidence might be to attacks of various sorts is difficult to assess with the data to hand. However, an example may help to show that their confidence could be very resilient indeed. Dr. Bernard came into conflict with his firm on various counts. First he was very bitter about the inadequate recognition he felt he was getting in a very dismal salary. Second, he was adamant that management did not recognise the capabilities and potential of their Ph.D. staff sufficiently. This led them to underrate their capacity for independent work. Nevertheless, under this assault of potentially undermining experiences, Dr. Bernard's confidence was not in the least dented: he merely became more critical of the way the firm failed to recognise their Ph.D. staff.

lV. SOME ASPECTS OF A PH.D. TRAINING

The suggestion that these scientists' Ph.D. trainings might have played a positive role in helping them to perform their industrial jobs is perhaps surprising. It contrasts markedly with the popular belief that a Ph.D. training is narrowly specialised and therefore likely to leave a scientist with neither the capacity nor the inclination to adjust to the more worldly demands of an industrial job.[5] It also contrasts with the sociologists' view of the academically trained scientist as someone who is likely to have acquired a stable commitment to the pursuit of knowledge for its own sake. It is therefore worth looking in more detail at the link between the scientists' Ph.D. trainings and their work.

On the whole, the scientists were glad they had done a Ph.D. They had found the experience itself satisfying and they valued it as a training. It is however interesting that what they valued about the training was not any specific skills or techniques, but the more general characteristics of a scientific training — the ability to approach problems, plan and carry out research, think systematically and generally to have the confidence to persevere independently at a problem without having to rely on anyone else. The factors that they talked about valuing are summarised in TABLE 166.

Although they did have criticisms of their trainings, these were, for the most part, expressed fairly mildly and usually served

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[5] A view which is captured in both the Swann Report and the parliamentary paper on Postgraduate Education.

merely to qualify the overall positive emphasis in their views. The scientists' regrets or reservations about having done a Ph.D. are set out in TABLE 167.

TABLE 166

NUMBER OF TIMES VARIOUS VALUED ASPECTS OF PH.D. TRAININGS WERE MENTIONED

1.	Enjoyed doing the Ph.D. for itself — e.g. very interested in the research topic, whole experience very satisfying etc.	27
2.	Enjoyed university life, i.e. the freedom, atmosphere, frequent discussions etc.	14
3.	Valued the scientific training in a general sense, i.e. how to approach problems, plan research, do practical work, interpret it, draw it all together, persevere despite setbacks, etc.	26
4.	Stressed the independence and self reliance acquired through it.	10
5.	Valued confidence gained.	9
6.	Valued specific skills or techniques learned.	9
7.	Valued the status.	9
8.	It has, or may well help in getting a better job or promotion.	5

TABLE 167

NUMBER OF TIMES THE SCIENTISTS MENTIONED VARIOUS REGRETS OR RESERVATIONS ABOUT HAVING DONE A PH.D.

1.	May have been a drawback in career terms.	5
2.	Financially a loss — either in terms of an investment or in lost salary.	5
3.	Thinks in retrospect some alternative course (e.g. M.Sc. in applied science) might have been better.	3
4.	Had a rough time during the Ph.D. — bad supervisor, ill-defined problem, etc.	2

All the other reservations were mentioned by one person only : —

5. Hated the inefficient way in which university research was organised.

6. His doing a Ph.D. was a waste of money for the country.

7. Finds it difficult to justify spending the time on it.

8. Was oversupervised and did not learn how to do research independently.

9. Actual research was not very valuable scientifically.

10. Did not find the research topic very interesting and hated life (both scientific and social) in Cambridge.

11. University was a bit narrow and naive.

12. Totally disillusioned with research and academic life.

The emphasis the scientists placed on the value of their trainings in a general sense is perhaps rather surprising. It might be imagined that people with such highly specialised training would be likely to value its more specialised elements, such as particular skills and knowledge. Could they have been stressing their general skills as problem solvers because the specific content of their Ph.D. was not much use to them in their jobs ? In fact this was not the case. Many of the scientists worked in fields closely related to their Ph.Ds. The way they described the link between their Ph.Ds and their work is summarised in TABLE 169.

From TABLE 169 it can be seen that there were twenty scientists (half the sample) whose work was fairly similar to their Ph.D. research. Sixteen of these reported making use of similar skills and techniques. There were also a further three who made use of computing skills acquired while doing their Ph.Ds and a further four who thought that their Ph.D. content might be directly relevant to their work in the future. Nevertheless, only nine people (TABLE 166) reported actually valuing the skills they had learned. Why should so few of them seem to value the skills that had in fact proved useful to them ?

In typically stressing their general problem solving abilities, the scientists were stressing what they saw as their most distinctive resource. As some of them pointed out, given a general scientific background, such as a Ph.D. or even just a B.Sc., then there is nothing especially demanding about most of the techniques they would have occasion to use in their jobs. Having a reasonable

TABLE 169

THE LINKS BETWEEN THE SCIENTISTS' PH.Ds AND THEIR WORK

Work very similar to Ph.D. — similar topic and techniques.	8
Work fairly similar — different topic, but similar techniques.	8
Work is related, but uses different techniques or instrumentation.	4
Work only relates in the sense of using general research methods — nothing specific.	9
Ph.D. is not directly relevant now, but may well be in the future.	4
Work makes use of computing techniques learned during Ph.D.	3
Scientific approach useful — no connections otherwise.	2
No relationship at all.	2
TOTAL :	40

repertoire of skills and techniques was certainly useful and many of them took a craftsman's pride in the exercise of them. They felt they could do practical work with more finesse or "feel" than they conceivably could have done after their B.Scs. Nevertheless, these particular skills were largely a matter of practice and could often be carried out by a B.Sc. or less qualified technician. What such people would not have, they suggested, was the overall grasp and confidence to work independently that they had acquired through their Ph.Ds. Although such self-sufficiency might be acquired through other means, they argued that a Ph.D. training, with its stress on tackling problems independently, was singularly well-adapted to foster it.

V. THE PRIORITY OF PROBLEMS

It might be expected that because the scientists had learned their problem solving abilities in a specific context, this would mean that what interested them would be confined to that context. This was not the case however: they seemed capable of becoming involved in and getting satisfaction from a wide range of problems. The scientists' flexibility was not only a matter of capability, but also of the attitude they took to their work and the characteristic style of their involvement.

Regardless of the sort of work they were doing they tended to describe the source of their satisfaction in terms of problem solving.[6] Probably each scientist would have his own limits on what would constitute a challenging problem and his own preferred focus of interest. These individual variations were certainly set

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[6] Almost half the sample used the expression "problem solving" explicitly, but the replies of those who did not convey much the same idea.

however within sufficiently broad limits to cover the majority of tasks the scientists were required to do in industry. The majority found in their jobs quite sufficient to engage their interest and satisfy their intellect.

The types of problem which attracted the scientists were very varied in their nature and located in widely differing circumstances from those, on the one hand, which demanded theoretical solutions, concerned with scientific understanding, to those, on the other hand, which required intensely practical solutions, sometimes in the form of immediate action. Taking the sample as a whole, it was difficult to escape the impression that almost any problem could generate interest for them, providing the objectives were fairly well-defined and the route to them constituted a challenge. The challenge could be intellectual or practical and was often both. The notion that a problem should be difficult, demanding skill, ingenuity and sheer brain-power to solve it was a principal component in the very idea of a problem. It could however in a few circumstances be willingly forfeited. For example, constraints like pressure, or a high premium on the speed with which solutions were reached could act as a 'handicap' and provide compensation for lack of intrinsic intellectual challenge. Problems were seen by the scientists as having other dimensions, such as depth or breadth and long or short term natures, and these were often weighed up against each other. Sometimes a variety of different sorts of problem were appreciated, since this facilitated satisfaction in a variety of directions.

Within limits, the more difficult and challenging a problem, the more satisfying getting the eventual solution was felt to be. Problems did not have to be so formidable though that the solutions were impossible either in general or with the reasonable means at the scientist's disposal. Lacking appropriate equipment could act as another 'handicap' and tax ingenuity and inventiveness with the most gratifying results for the scientist concerned. Normally though, if a problem was too difficult or demanding, interest in it would wane, because the frustrations would outweigh the satisfactions.

Some examples from the interviews will now be given to illustrate these points.

1. <u>Dr. Wemyss:</u> <u>shows theoretical solutions being contrasted with practical ones</u>

— Can you say how the work relates to your Ph.D. ? You mentioned a bit at the beginning that it's slightly different.

. . . it's not so different really. The outcome is vastly different. At the bench level there's not so much difference at all — I'm still manipulating chemicals, thinking about what I'm doing at the bench level, writing reports, going to lectures at The Chemical Society of Manchester. I don't see there's much difference at my level, as I see it. For my Ph.D. I did three years on one subject in physical organic chemistry which is what I studied. After the first year it's just turning a handle and putting slightly different modified chemicals into the system and getting slightly different modified chemicals out and that's how it went on. So that's pretty repetitive. I probably did one year's research and two years' routine work at [naming the university]. Here the work is much more varied. I was surprised at the depth of some of the problems we have to go into — at a chemical level. I mean you don't just slide over the surface.

— When you compare what you're doing now with the sort of work you did for your Ph.D., can you say what it is that you find more satisfying about the work you're doing now and what you found more satisfying about the Ph.D. — if anything ?

Ph.D. — I found the chemistry very interesting; the bench chemistry wasn't. It was very interesting the results I managed to get out at the end — what they actually meant — but the work itself was a bit routine. Here it tends to be the other way round I suppose. The techniques and manipulations and the care needed can be rewarding, and the results you get at the end, but the overall picture of results you get at the end tend to be very bitty by the nature of the work. At the end of the year, you look at what you've done and you've analysed 32 [name of product] and 47 [name of different product], although there again, pictures do emerge. You find out that most manufacturers sail very close to the wind in quality [he gave examples]. So there's a picture emerging in that. Basically I found the work at [name of university] more and more boring. I will never regret moving out of academic life at all.

2. Dr. Dundonald: a scientist for whom almost any problem could generate interest providing it was intellectually challenging

— Would you prefer the work you're doing here to be closer to the work you were doing for your Ph.D. in terms of being more fundamental ?

Not at all. As long as it was interesting and not boring, it wouldn't worry me. If you could make out an interesting case for digging holes in the road to me — interesting — I'd go and dig holes in the road.

— Can you say what characterises an "interesting" thing for you ? What is it that you find interesting ?

Anything which presents an intellectual challenge — think out a better way of doing this or a different way of doing that — that's interesting. It doesn't matter what it is. I'm very interested in salmon fishing. Now this sounds totally unrelated to my work. I like salmon fishing because it's a challenge — trying to work out why salmon, who don't eat in fresh water, will take a lure if I present it to them in a certain way at a certain time. That's a challenge. O.K. it's nice standing beside the river there, but I don't fish for salmon because I like standing beside the river as much as I like catching salmon. And I don't like fishing for mackerel because you just chuck a hook in and pull them out. There isn't the challenge of getting round . . . straining oneself a bit to find what the answer is.

3. Dr. Moray: shows again how almost any technical problem could be interesting, providing it was not too difficult

— You said you enjoy the work very much at [naming the firm]. Can you say what it is about the work that you enjoy ?

I enjoy finding practical solutions to problems.

— The emphasis being on the practical ? . . . or not ?

Well all the problems I get at [naming firm] require a practical solution in some form or other. It's really an extension of what I like doing at home. [He elaborated on his do-it-yourself propensities.] . . . it is just feeling the way out of a problem, making the effort required to sort out a good product from a bad product. It's just a technical challenge and I enjoy it.

— When you talk about a problem, would any problem be capable of generating as much interest for you, or is it more specifically scientific ones or related things ?

You mean perhaps a problem involving employment or management or something like that ?

— Well as wide out as that if you were thinking like that. I suppose I was thinking about degrees of technicality.

Oh I don't think I'm really fussy in that line. I can get engaged in any sort of question that rquires an answer really. I mean, that's what the grey matter is for.

— There isn't any particular compulsion in, say, pure science problems as opposed to . . . ?

No, I get just as much out of sorting out how I get a little bit of cable through an inaccessible place if I'm rewiring the house. It's just that it's a technical problem. Well it's not even technical really; it's just a matter of athletics of how you're going to do it. But it doesn't really matter, no. The exact form of the problem is not perhaps that important.

[a little later in the interview]

. . . Sometimes the problem is just unsolvable by me, which is frustrating at the time, but I'm prepared to put up with that, because in other cases, in other areas, if I can have a good think, I can provide a solution that does work and I get a real kick out of seeing that solution working in practice. If you get too mind-stretching problems that you can't solve, it's not very pleasant, I would agree, but you can't . . . the problems that you can't solve . . . it's not the fault of the problem; it's the fault of you and you've just got to put up with the fact that you can't do everything. In a lot of cases there's someone else in [naming the firm] who can help you anyway, but it's the satisfaction of completing the job, finding the answer to the problem that I like and that applies whether I'm just putting a shelf up somewhere and managing to get it the right length, or I've got [tape inaudible for a few words]. It's finding the answer to a problem that I like doing and there's plenty of scope for that in the job. There's never a shortage of problems.

4. Dr. Marchmont: shows how intellectual difficulty was a principal component of a problem

— You mentioned earlier that you get a great deal of satisfaction out of the work you've done. Can you say exactly what it is that you find satisfying ?

I suppose you're going into the unknown and you start off with a project and you can define certain areas that require investigation and various parameters that are not at that time related to each other, that it would be useful if they could be related to each other; you design an experiment to do it; make up the apparatus and so on; go through lots of frustration — you know lots of bits of things that don't work. You then do the experiments and lots of people I know would work really quite hard at that stage and really be knackered most of the time. Then at the end of it — it depends on the project — sometimes the results just filter through; sometimes it's a mad rush at the end when lots of results rush out — and it's a satisfying sequence of events. At one stage, you're not really sure whether it's going to work and that's really frustrating. Lots of people, and probably I'm one of them, wonder is it worth going on even, and so, you know, certain frustrations build up and then you reach a climax hopefully and see something useful coming out and often the results that come out — one or two anyway — are really quite startling . . . [gave a long and dramatic example] . . . If you find something which solves a problem, then you know, I think I'd get satisfaction from it and the bigger the problem and the more dramatic the result, the more dramatic the effect really.

5. Dr. Inglis: shows how a premium on speed could act as a 'handicap' and compensate for lack of intrinsic challenge

— How satisfied would you say you were with your job ?

At the moment, most of the time, very satisfied. There are . . . as I say, I've been mostly concerned with the early development when the pressures are on time and the fact that you've got to get results quickly isn't dissatisfying — you know, if anything it's satisfying.

— Why?

Well, you know, I mean you've got to get the answers that much quicker — to perform well and efficiently and at the end of it, knowing you have done it is satisfying. Whereas in future, when the demand is just to keep costs down, the sort of changes you will be making will tend to be more trivial and also the programme will be more protracted — it could be over the whole of the rest of the development life, so you could be going over the same ground over and over again with minor modifications and the whole ground could become rather . . . you could become very jaded. So obviously not everything is rosey. Again I've been lucky here and know that things will not be exactly the same in the future. You know, you've just got to take the rough with the smooth. You've got to know that there are going to be times when it'll be highly satisfying — you'll be getting results, you'll be seeing answers to the problems; and other times you'll just be beating your head up against a brick wall. You know, it was like that when you were doing a Ph.D. When I look at my Ph.D. thesis, there was a whole year when, although I worked steadily and hard, there's none of that in my Ph.D. thesis. That's just the way it works out in the end.

6. Dr. Clarendon: shows comparison between the satisfactions of long and short term problems

. . . Perhaps if you have too much time on fundamental research, you can begin to have to have a slightly guilty feeling that you're doing it just to satisfy your own ego and you're not perhaps making . . . you know, it's difficult to get the satisfaction out of the job if you're all the time thinking in terms of fundamental work and research that's devoted to long-term ends — you don't get the immediate satisfaction of day-to-day jobs being done and finished and saying, "oh I've done that; that's finished". So, you know, there is a balance. It's one of the things that got me rather about doing a Ph.D. — it's all directed to a goal about three years away; it's a three-year project and you can sometimes work months and months without getting results and that can sometimes get a bit discouraging. So, in a way, it's rather nice to have some fairly short-term projects — almost routine, though not quite routine — but fairly short projects which give you results and get satisfaction regularly.

7. Dr. King: again shows contrast between pleasures of long and short term problems

. . . let's talk about the type of job, because this is what it all boils down to — it's very satisfying. You get a combination of short-term jobs and long-term jobs. The short-term will bring you short-term rewards — in other words, if you crack a problem fairly quickly which a customer wants you to crack because it's holding his production up — you know, so much money everyday is going down . . . if you can think of something that'll help him out or, you know, you admit that your [mentioning the name of the product his firm makes] is ropey and get some more sent in. Whatever you do. . . whichever way you do it, there's a short-term reward in having solved something fairly quickly. On the other hand, you've got the long-term development type job which brings in its rewards a lot more slowly and is a lot more tiresome as a project, because obviously the more something goes wrong when you're trying to solve it, the more tired you get of it; but once you've got the thing somewhere, then there's a tremendous amount of satisfaction involved that you've achieved something. I mean that's the thing with the Ph.D.: it's a long-term thing and you get fed up in the middle, but you keep going and once it's over, it's really great; you've really done something. I think it's the same with a fairly long project — an internal type project in this sort of department. Certainly the greatest satisfaction comes because you are genuinely helping customers out and, you know, without them, you wouldn't exist and you really feel you've got a bit of genuine competition. It's not like a research problem which is one of millions and which might or might not be accepted if it does get somewhwere. It's something where there's a definite problem and if you can solve it, then you've helped someone both inside the firm and the customer. It's a very satisfying job.

8. Dr. Babberton: shows the pleasures to be derived from the 'handicap' of not having the right equipment

. . . What tends to happen as well is that when you've only got a limited amount of stuff, you've got to really start thinking about what you're going to do. What can happen is that you get so used [i.e. at university] to using all this expensive stuff, that you don't really think about your experiments, but when you've got very little, [i.e. as often in industry] you've got to start improvising, you've got to start really looking at the thing and thinking, well I'd like to do it that way, but I can't really afford it, so I'll have to do it some other way. Then you start trying to get in from the back. That is quite often a good way of thinking. Often the best answer isn't from the most expensive way, it's from the cheapest way and, as I say, it tends to sharpen your wits a bit when you're looking at a problem. You tend to look at a problem as a whole, instead of . . . you tend to go in there thinking what can I do with the things I've got.

Generally, with most scientific problems, there's a lot of ways of looking at them and all these systems eventually give the same answer — you know, it depends which way you go and you still eventually reach the same answer through all these different pathways, which is one of the beauties about science — if you follow the logical thought processes, you always arrive at the right answer. What tends to happen is if you've got access to a lot of equipment, you tend to think there's only one way, but if you haven't got access to the equipment, you think, well if I had the equipment, I could do it that way, but I haven't got the equipment, so I'll have to think what other way to do it. Then what happens is you develop a far better understanding of the problem, because . . . alright you reach the same answer, but you've had to go about it in a different way . . .

Vl. KNOWLEDGE AND SKILLS

The aim of this chapter has been to provide material which offsets the view of industrial scientists provided by the value conflict theorists. First of all the process of job choice was examined and it was re-affirmed that academic orientations were few and far between. This further evidence against the academic socialisation picture was again reinforced by looking at the scientists' long-term career orientations. Their ambitions were, for the most part, formulated very much in terms of the type of career offered by industry. Next the organisation of work was examined. This brought out the variety of the scientists' work situations and should combat any stereotyped images of the role of industrial scientist. Next the process usually called 'recognition' was examined. This is a crucial term for the value conflict theorists because of its association with the exchange model of science, itself a standard part of their theory. It was argued that these Ph.D. scientists were able to operate independently of explicit recognition. Such capacity for independent work was attributed to their Ph.D. trainings, which it was finally argued had endowed the scientists with generalisable problem solving skills. Some of the implications of this data will now be drawn out explicitly.

A typical conception of how scientists work is that knowledge is exchanged for the reward of recognition. Knowledge and rewards are treated as two qualitatively different commodities. In some respects therefore, this view is a close relative of the simple psychological models proposed by behaviourists — a reward of food or drink will cause a rat to remember part of a maze that it has just run along. Learning on this view needs 'reinforcement'. Although for the scientists in this sample there was undoubtedly an exchange process involving financial 'reinforcement' for scientific work, the exchange view completely misses the most prominent feature to emerge from these interviews. This was the way that simply doing the job — its competent performance and smooth operation — was itself a reward. There are not two qualitatively different parts to this 'exchange': knowledge is not traded-in for something else. Going back to the comparison with the rat in the maze: on this modified model, the rat simply learns the maze without food or drink — it is a natural learner. No reinforcement is necessary; or, to put the point in another way, learning is intrinsically rewarding. An account of this kind appears to be necessary to capture the flavour of the everyday routines of these industrial scientists. The identification of information with reward chimes in with the fact that recognition and feedback were not separable in their accounts.

This finding contrasts with those of other studies like Ellis' or Barnes'. The scientists in their samples were much more concerned with displaying their competence and having it explicitly recognised. Few of Ellis' sample and none of Barnes' however had Ph.Ds and this difference in the composition of the samples seems the likely source of the divergence. A lower level of technical competence, with a correspondingly lower level of confidence, could alter the balance between autonomy and dependence on external recognition. This point is not as trite as it may seem. Its implication is that knowledge affects social relationships. The 'social system of science' cannot therefore be looked at in isolation from the content of the expertise and knowledge with which it endows people.

The emphasis on knowledge and information rather than either abstract values or rewards and reinforcements is a theme that is worth elaborating. It provides the beginning of a systematic view which both helps to make sense of the findings which have emerged so far and will help to sharpen the issues to be reported in the next chapter.

Returning again to the behaviourists' rat: what are the implications of dropping the idea that a rat needs rewards to learn, and saying instead that he learns naturally ? The idea is that rats simply learn about their environment and the resources it contains. They notice where the food and water is and then go to the appropriate place when hungry or thirsty. They learn the information without needing rewards to make the information stick. One suggestive idea that has been used by psychologists is to say that rats build up 'cognitive maps' of their environment and act on them according to their wants.[7] This idea can be usefully adopted by sociologists.

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[7]The main exponent of this view was a psychologist called E.C. Tolman. See e.g. his "Cognitive Maps in Rats and Men", Psychological Review, July 1948. Also reprinted in Edward Chace Tolman, Collected Papers in Psychology (Berkeley and Los Angeles, University of California Press, 1951). For a brief account of Tolman's 'purposive behaviourism' and its differences with the crude behaviourism that underlies the value-conditioning theory, see D.E. Broadbent, Behaviour (London, Methuen, 1964).

The idea is that scientists, like anyone else, have a 'map' of society in their heads. They know that different things go on in universities and in industry — these places have their respective positions on the map. Thus one would not expect a single conception of what science is — one monolithic value system — but a sense of science appropriate to different contexts. Correspondingly, there would not be anything but a taken-for-granted expectation that some things are appropriate and sensible in universities whilst other things are in industry. If this sort of information is central to scientists' behaviour, then value conflicts would not on the whole be expected — though of course preferred positions and destinations on the 'map' would be expected.

The metaphor of 'cognitive maps' taken over from the more sophisticated behaviourists is a psychological model of man which fits very well the picture of the scientist that has emerged from the results of this study so far. This is because of its stress on the learning of information, rather than the taking over of value orientations, as is the typical picture presented by Merton and the value conflict theorists. As a learner of information for later use, man is seen as more active and flexible.

There are then two important elements in this emerging cognitive view. The first is the smooth self-regulating way in which the scientists exercised their skills. The second is the knowledge they had of their environment and the options open to them — summarised via the metaphor of the cognitive map. So far, the idea of cognitive maps is speculative, but it has the virtue of having been suggested by the data. Some of the implications of the idea will be tested in the next chapter which deals with some of the changes in the scientists' attitudes between stage one and stage two of the study. The first element calls for some immediate comment.

The independent self-confident way in which the scientists worked bears directly on the question of whether it is Becker or Kuhn who offers the greater insight into the industrial scientist. Undoubtedly it is Kuhn. Kuhn's account of scientific training suggests that it is exactly the sort of process which would equip and encourage scientists to be orientated towards problem solving in the way discussed in sections 111, 1V and V of this chapter. Although Kuhn's argument was concerned with the way academic scientists learn to do pure research, the proposed extension to it to cover industrial scientists (pp. 50-2 above) appears thoroughly justified by these results. The way these scientists talked about their work and the way it related to their Ph.D. trainings suggests that the process of learning they had gone through during their Ph.Ds equipped them perfectly adequately to cope with the problems they encountered in their industrial jobs. Kuhn's characterisation of science as "puzzle-solving" and his description of the types of puzzle which most attract and engage scientists is easily recognisable in the accounts these industrial scientists gave of a challenging problem.

In contrast, the processes emphasised in Becker's theory were not at all prominent. In this sample there was no conspicuous exhibition of competence or assertion of entitlement to recognition.

Unlike Barnes' B.Sc. sample, these scientists did not seek to display their expertise in order to 'make out'. They did not need to. They could take their position for granted and simply get on with the job.

In order to consolidate the claim that Kuhn's theory and especially his account of normal science applies to industry, it may be useful to consider an objection. It might be objected that Kuhn's account suggests that scientists are initiated into particular puzzle-solving traditions, whereas my argument has been that the scientists had acquired a general capability from their academic trainings. Such an objection would be trading on a misleading opposition however. The point is that people do not necessarily need general trainings to acquire capabilities that can be generalised. Transferable skills can be learned in quite particular circumstances. Thus these industrial scientists, by generalising from the particular experience of their Ph.D. trainings, could apply their problem solving ability to the wide range of problems they encountered in industry. Indeed it could be argued that people will only learn general problem solving skills by coming to grips with real problems in particular circumstances. This idea is itself at the very basis of Kuhn's epistemology, based as it is on the notion of paradigms. His account of the internal workings of the institution of science thus provides a ready-made explanation of how such knowledge can find application outside the particular academic context in which it originated.

CHAPTER Vl

SCIENTISTS AND COGNITIVE MAPS:
A COMPARISON OF EVENTUAL ACADEMIC AND INDUSTRIAL SCIENTISTS

The possibly unexpected findings of the last two chapters suggest two sets of questions : —

(1) To what extent were the industrial scientists typical of the rest of the sample ? That is, were they especially flexible and industrially orientated compared to those who did not go into industry ? Also, were they much less attached to academic values than the others ?

(2) Had the values and attitudes of the industrial scientists altered at all since their university days; and if so, in what ways.

In this chapter, an attempt will be made to answer these questions by an examination of material from the first stage of the study. Since what is at issue is the contents of two allegedly conflicting value systems — those of academic science and industry — the analysis will be confined to two groups of scientists within the sample: those who eventually took academic and industrial jobs respectively.

The questionnaires which provide most of the information to be used in this section were sent to the scientists in the second week of March in their final year as Ph.D. students. The reasons for wanting access to their thoughts at this particular time were two-fold. On the one hand, getting a job would be a sufficiently pressing issue for them to make it likely that they would give serious thought to questions on the matter. On the other hand, most of them would not yet have obtained a job, so if they had any reasons for preferring particular types of job, these would be more likely to emerge unencumbered by any realignements of their views that might occur after they had secured a definite job.

The different theories being examined yield different expectations about what values and attitudes scientists at this stage in their training might have. These can be summarised in the form of a number of hypotheses. In the material to follow, each of these hypotheses will be presented along with a commentary on its implications for the theories, relevant results from the questionnaires and a brief summary.

1. THE ETHOS OF SCIENCE

HYPOTHESIS ONE There will be no difference between the tendencies of the eventual academic scientists and the eventual industrial scientists to profess attachment to the norms of science

Implications

1. (a) If no significant difference is found between the two groups, then this will represent support for the cognitive mapping theory — that is, that the scientists share a cognitive map, having similar ideas about what rights academic and industrial scientists should have. They might be expected to have different ideas about which of these freedoms they personally want, but that is another matter and will be examined under a subsequent hypothesis.

   (b) If this finding was combined with a high absolute level of support for the norms and little tendency to discriminate between their appropriateness in different institutional contexts, then it would be support for the value conflict theorists and the idea that scientists are indeed socialised during their training into the ethos of science.

2. If there are significant differences between the two groups, then two major possibilities are suggested : —

   (a) Scientists are differentially socialised into the norms and values of science and this affects their subsequent job choice. Scientists attached to the norms of science will try to stay in the academic world and those without such attachment will be more prone to opt for industry. This is the thesis of Cotgrove and Box.

(b) Scientists get orientated towards specific jobs (for whatever reasons) and are then inclined to espouse the appropriate values.[1A]

Questions designed to provide evidence for this hypothesis were in section three of the questionnaire and were confined to the scientists' views on autonomy and communality. Asking questions on the other norms did not seem very practical, nor were they in fact matters about which there was likely to be a conflict in industry. The questions on autonomy and communality were arranged so that the scientists could discriminate between university and industry in saying what freedoms they thought scientists should have. The likelihood of such discrimination was suggested by Barnes' findings which showed that scientists believed different norms and values to be appropriate in different institutional settings.

The results are summarised in TABLE 191. As can be seen there were no significant differences between the eventual academics and eventual industrialists in their views on these matters — that is, both groups had similar distributions of opinions on the autonomy and communality that should be the right of industrial and academic research scientists. What is more, their answers show a very clear discrimination between what they thought appropriate in the different institutional areas. The rights to publish, decide which projects to work on and carry out research projects in the way they think best

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[1A] A possibility also raised by Box and Cotgrove. As they point out, the direction of causation is problematic.

were all thought to be more relevant for academic scientists than for industrialists.

TABLE 191

ATTACHMENT TO THE NORMS OF COMMUNALITY AND AUTONOMY

Scientists answering 'YES' to the following questions :—	Eventual Academics N=99	Eventual Industrialists N=62	Probability under Ho for df=1 that :-
a. Academic scientists should have the right to publish their research work.	92	57	$X^2 \geqq \cdot 09$ is $\cdot 8 > p > \cdot 7$
b. Industrial scientists should have the right to publish their research work.	50	23	$X^2 \geqq 2 \cdot 3$ is $\cdot 2 > p > \cdot 1$
c. Academic scientists should have the right to decide which projects they will work on.	60	42	$X^2 \geqq 1 \cdot 06$ is $\cdot 5 > p > \cdot 3$
d. Industrial scientists should have the right to decide which projects they will work on.	13	7	(regrouped figures below)[1]
e. Academic scientists should have the right to carry out research projects in the way they think best.	81	57	$X^2 \geqq 3 \cdot 59$ is $\cdot 1 > p > \cdot 05$
f. Industrial scientists should have the right to carry out projects in the way they think best.	62	38	$X^2 \geqq \cdot 13$ is $\cdot 8 > p > \cdot 7$

[1]In this and some of the tables to follow, some of the categories were too small for the X^2 test. These cases are analysed separately after each table to which this applies. The methods used will be indicated in each case. In this instance, the test can be carried out by separating those who answered 'NO' and 'DEPENDS' to produce a 2 x 3 rather than a 2 x 2 test.

TABLE 191 (continued)

Regrouped figures for (d)

<u>Industrial</u> scientists should have the right to decide which projects they will work on.		Eventual Academics	Eventual Industrialists	Probability under Ho that : -
Scientists answering : -	YES	13	7	
	DEPENDS	64	35	$X^2 \geq 2{\cdot}55$ for df=2 is
	NO	22	20	$\cdot 3 > p > \cdot 2$

11. IMAGES OF INDUSTRY

<u>HYPOTHESIS TWO</u> <u>There will be no difference between the images of industry that eventual industrial and eventual academic scientists have</u>

<u>Implications</u>

These are very similar to the implications of HYPOTHESIS ONE. If there is no evidence of significant differences between the two groups, then this will be further support for the cognitive mapping theory. As members of the same society, they share a cognitive map of that society. Evidence of significant differences, on the other hand, might arise either as a cause or an effect of differential socialisation.

The questionnaire items on this topic concerned the amount of freedom industrial scientists were imagined to have and the perceived scientific interest of their work.

TABLE 193

IMAGES OF INDUSTRY

Scientists answering 'YES' to the following questions : - Industrial research scientists in general do : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
1. have the right to publish their research work.	12	5	(regrouped figures below)
2. have the right to decide which research projects they will work on.	3	1	(regrouped figures below)
3. have the right to carry out research projects in the way they think best.	24	19	$X^2 \geq \cdot 54$ is $\cdot 5 > p > \cdot 3$
Also number of scientists in each group indicating that they 'agree completely' or 'agree with reservations' with the statement : -			
4. There are plenty of opportunities in industry for scientists to do research that is of significant scientific interest.	40	20	$X^2 \geq 1\cdot 5$ is $\cdot 5 > p > \cdot 3$

Regrouped figures for (1) and (2)

Answering 'YES' and 'DEPENDS' (rather than just 'YES' as above)

1. have the right to publish their research work.	65	43	$X^2 \geq \cdot 15$ is $\cdot 7 > p > \cdot 5$
2. have the right to decide which research projects they will work on.	34	20	$X^2 \geq 1\cdot 07$ is $\cdot 3 > p > \cdot 2$

The evidence in TABLE 193, showing as it does that there were no significant differences between the images of industry held by the two groups, again provides support for the cognitive mapping theory. In addition to having similar values (as shown in TABLE 191), the scientists also have similar ideas about what actually occurs in industry. Comparing the two tables, it is apparent that there is a difference in their views of what rights they think scientists should have in industry and what rights they actually do have. The combined results from the two tables seem to confirm the traditional characterisation of the scientist in industry as someone whose rights are unduly restricted. For ease of comparison, these figures are set out together in TABLE 195 below. Since there were no significant differences between the eventual academics and eventual industrialists, they are combined.

From these results alone, it might be expected that the sample would contain people who, if they went into industry, would be discontented. However, as has already been shown, this was simply not the case. There were certainly a small number of discontented scientists amongst the industrialists interviewed, but they were not discontented because of any feeling that rights such as these were being violated. The question is thus raised of whether industry is 'better' than most scientists imagine it to be, or whether, on the other hand, scientists modify their views when later they come into an actual industrial situation. In fact the scientists' industrial jobs often turned out better with respect to these freedoms than they had indicated in their questionnaires that they expected.

The number of times this happened was as follows : —

Industrial situation turned out better than expected with regard to : -	Number of times
a. freedom to publish	16
b. freedom to decide which projects to work on	21
c. freedom to work in own way	20

TABLE 195

A COMPARISON BETWEEN THE RIGHTS AN INDUSTRIAL SCIENTIST SHOULD HAVE AND THOSE HE DOES HAVE

Industrial research scientists : -	Both Groups N=161	as percentage of 161
1. should have the right to publish their research work.	73	45%
do in general have the right to publish their research work.	17	11%
2. should have the right to decide which projects they will work on.	20	12%
do in general have the right to decide which projects they will work on.	4	2%
3. should have the right to carry out research projects in the way they think best.	100	62%
do in general have the right to carry out research projects in the way they think best.	43	27%

111. WHAT THE SCIENTISTS LOOKED FOR IN CAREERS

<u>HYPOTHESIS THREE</u> <u>Eventual academic scientists will look for greater provision in their jobs for being able to behave in conformity with the norms of science than eventual industrial scientists</u>

<u>Implications</u>

1. If the hypothesis were confirmed, then it would provide further support for the cognitive mapping theory. In the light of the previous results, the scientists could be seen as having similar ideas of what various career options involve and what values are appropriate to them, but different ideas about what they personally want out of this agreed upon scheme of things — that is, about where on the 'map' they want to be located. This leaves to be explained why different scientists want different options. This question will be explored via later hypotheses.

2. If there were no significant differences between eventual academics and eventual industrialists, then the process of job choice would appear decidedly mysterious. Insofar as scientists 'choose' jobs, it would have to be for reasons other than these.

The questionnaire items relating to this hypothesis were on page ten of the questionnaire. Scientists were asked to indicate how important the factors specified in TABLE 197 would be to them in looking for a job.

TABLE 197[2]

ACADEMIC JOB REQUIREMENTS

Scientists ringing a code for 'necessary' or 'very important' against the following factors that might be looked for in a job : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=l that : -
*a. Freedom to publish your research work.	49	12	$X^2 \doteqdot 13{\cdot}65$ is $p < {\cdot}001$
*b. Opportunities to consult and contact other members of your profession.	58	24	$X^2 \doteqdot 6{\cdot}7$ is ${\cdot}01 > p > {\cdot}001$
*c. Freedom to decide which projects you will work on.	46	15	$X^2 \doteqdot 7{\cdot}26$ is ${\cdot}01 > p > {\cdot}001$
*d. Freedom to do the work in the way you think best.	82	40	$X^2 \doteqdot 6{\cdot}9$ is ${\cdot}01 > p > {\cdot}001$
*e. Interesting scientific work.	90 (91%)	43 (69%)	$X^2 \doteqdot 11{\cdot}47$ is $p < {\cdot}001$
*f. Chances of making a contribution to science.	55	16	$X^2 \doteqdot 12{\cdot}96$ is $p < {\cdot}001$
g. Opportunity to make a good career without moving out of science.	41	15	$X^2 \doteqdot 5{\cdot}7$ is ${\cdot}02 > p > {\cdot}01$

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[2] All significant differences in this and subsequent tables are marked for clarity with a *. Differences will be counted as significant only at $p < {\cdot}01$.

As is shown in TABLE 197, the differences between the two groups were significant with respect to all the items except (g). Eventual academics were more prone to emphasise the importance of factors traditionally associated with the pursuit of pure science than the eventual industrialists were. It is clear however that this emphasis was not overwhelming even amongst eventual academics. 'Interesting scientific work' had priority for them, with all the other factors except 'freedom to do the work in the way you think best' trailing some way behind. Further comment on these results will be postponed until after some other data has been presented which helps to show them in perspective.

HYPOTHESIS FOUR Eventual industrialists will emphasise worldly factors associated with 'good careers' more than eventual academic scientists in saying what they look for in a job.

Implications

1. If significant differences are found, then the implications would be the same as for the last hypothesis.

2. If no significant differences are found and this is combined with the evidence for the cognitive mapping theory which has already emerged, then eventual academics could be seen as as career orientated as eventual industrialists. The Mertonian features they say they require of a job could be seen as the prerequisites for making a good career in the academic world.

The questionnaire items for this hypothesis concerned the importance to the scientists of having : —

a. good promotion prospects

b. opportunities to take responsibility

c. good salary

TABLE 199

EMPHASIS ON 'CAREER' CONSIDERATIONS IN JOB REQUIREMENTS

Scientists ringing a code for 'necessary' or 'very important' against factors they would look for in a job : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
*a. good promotion prospects	64	53	$X^2 \geq 8 \cdot 6$ is $\cdot 01 > p > \cdot 001$
b. opportunities to take responsibility	59	49	$X^2 \geq 5 \cdot 95$ is $\cdot 02 > p > \cdot 01$
*c. good salary	57	50	$X^2 \geq 9 \cdot 6$ is $\cdot 01 > p > \cdot 001$

As can be seen from TABLE 199, the differences between the two groups in their emphasis on factors associated with having a 'good career' do tend to be significant. Apart from (b) where the difference just failed to reach significance, these factors were more often viewed as important by eventual industrialists than by eventual academics.

There is a further aspect of the scientists' career orientation which will be examined before moving on to comment on this material.

HYPOTHESIS FIVE There will be no difference between the general problem orientation of eventual academics and eventual industrialists

This hypothesis checks on whether the findings which emerged in the last chapter concerning the priority of problems are also applicable to the eventual academics. Three items on the questionnaire are relevant. The scientists were asked to indicate the importance to them in a career of having : —

1. Work which involves challenging problems irrespective of their relevance to pure science.
2. Work which fully uses the skills and knowledge you have acquired doing a Ph.D.
3. Work useful to the rest of society.

TABLE 200

GENERAL ORIENTATION TO PROBLEMS

Scientists ringing a code for 'necessary' or 'very important' against the following factors they would look for in a job :-	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
1. Work which involves challenging problems irrespective of their relevance to pure science.	68	50	$X^2 \geqq 3{\cdot}4$ is $\cdot1 > p > \cdot05$
2. Work which fully uses the skills and knowledge you have acquired doing a Ph.D.	35	21	$X^2 \geqq 0{\cdot}27$ is $\cdot1 > p > \cdot05$
3. Work useful to the rest of society.	54	37	$X^2 \geqq 0{\cdot}46$ is $\cdot7 > p > \cdot5$

The picture that is emrging of the scientists is one of the two groups having similar conceptions of the options before them and the values appropriate to them. They tended to differ however on precisely where they wanted to go in this agreed upon scheme of things. The eventual academics tended to stress their desire to do interesting scientific work and to have the associated opportunities and the evnetual industrial scientists tended to place more stress on good career opportunities. Such tendencies can be seen as linked to their job preferences. However, having said that the groups are differentiated according to their career orientations, there are some striking facts about the absolute numbers of scientists stressing the different factors. Looking at TABLE 197, for instance, only half the eventual academics actually thought freedom to publish would be very important to them. Slightly fewer than half wanted freedom to decide which projects they would work on. If these figures are compared with those in TABLE 199, then it is apparent that in absolute terms career considerations were important to more of them than those two freedoms. Here the similarities between the two groups of scientists were as interesting as their differences.

Such a view was reinforced by the results presented in TABLE 200. This shows that neither group put an especially high premium on getting work which fully used the particular knowledge and skills acquired doing their Ph.Ds, but both wanted work which would involve challenging problems. These findings confirm that the view of the industrial scientist as a man with a general interest in and capacity for solving a wide variety of problems irrespective of their relevance to pure science may also apply to his more academic counterpart.

Similarly, the lack of significant difference between the two groups on the extent to which they want to do work useful to the rest of society, suggests that the entry of practical utility into the vocabularies of scientists is not confined to industrial scientists, but is a more general phenomenon.

The only suggestion so far that the two groups might differ in their notions of what constituted interesting work was the overwhelming tendency of the eventual academics to want to do 'interesting scientific work' (TABLE 197). As has been indicated, they did not equate this with doing work which followed on closely from their Ph.D. research. Both groups had similarly low propensities to indicate continuity with their Ph.D. research as a requirement of a future job. However, it seems plausible to suggest that, regardless of their lack of desire to continue in the fields of their Ph.Ds or to make use of the specific skills and knowledge they had learned, nevertheless, their differential requirements for interesting scientific work might be, in some way, a reflection of the patterns of satisfaction and frustration they had experienced while doing a Ph.D.

1V. PATTERNS OF INTEREST IN WORK

Two items were included in the questionnaire to get evidence on any possible differences in patterns of interest in work between the two groups. These were on pages four and five of the questionnaire and concerned the different facets of scientific work that might be a source of either satisfaction or frustration; and the overall balance of their interest in their research over the three years of their studentships. Hence : —

HYPOTHESIS SIX Eventual academics will have been more interested, on balance, in their Ph.D. research than eventual industrialists

Implications

If there are significant differences, then this will suggest : —

a. that the job choice of eventual academics is a reflection of their interest in academic work;

b. that their greater interest in work is a symptom of a prior orientation to academic pursuits;

or c. and more likely — some mutually reinforcing combination of the two.

If there are no significant differences, then the source and nature of the differential orientation of the two groups continues to remain mysterious.

TABLE 204

BALANCE OF INTEREST IN PH.D. WORK

Scientists indicating that, on balance, they were 'very interested' in their Ph.D. research during their : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
First year of research	40	25	$X^2 \geq \cdot 18$ is $\cdot 7 > p > \cdot 5$
Second year	42	21	$X^2 \geq 1\cdot 02$ is $\cdot 5 > p > \cdot 3$
*Third year	70	31	$X^2 \geq 7\cdot 14$ is $\cdot 01 > p > \cdot 001$

Discussion of these rather intiguing results will be left until further facets of the scientists interest in their work have been presented.

HYPOTHESIS SEVEN While eventual academics and industrialists will have similar propensities to find satisfaction in problem solving, there will be different emphases in the rest of their interests

In particular it seems plausible to suggest that different sorts of problem might typically arouse their interests. Academics might be expected to get more satisfaction out of : —

1. mastering scientific theory
2. doing original scientific work
3. getting research work published
4. discussion with students and staff

5. exchanging information and ideas

Industrialists might be expected to get more satisfaction out of : —

1. designing and constructing pieces of apparatus
2. using sophisticated scientific equipment

What is being suggested is that the likely patterns of interest of the two groups might have a tendency to split along theoretical and practical lines.

As is shown in TABLE 206, there were in fact few significant differences between the different facets of the interests of the two groups. The major exception was that the eventual academics were more likely to indicate that 'mastering scientific theory' was a major source of satisfaction. The only other item on which the two groups could be discriminated was (g), and the eventual academics had a greater tendency to indicate that 'exchanging information and ideas . . . at conferences' was a source of satisfaction. Since the absolute level of the figures was so low and so few people in either group found it a major source of satisfaction, little can be made of these results. It would seem that discussion and exchanging ideas (items e,f and g) did not in general contribute much to the scientists' satisfaction. The actual work itself (items a, b and c) was much more important to them. From these results, neither group seemed especially moved by the idea of the more practical aspects of the work (items h and i). However, with the benefit of hindsight and the experience of talking to many of these scientists at length about their satisfaction in work, this result may be partly a consequence of the questionnaire

TABLE 206

PATTERNS OF INTEREST

Scientists finding the following activities a 'major source of satisfaction' : —	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
a. The general activity of solving and overcoming problems	61	41	$X^2 \geq \cdot 49$ is $\cdot 5 > p > \cdot 3$
*b. Mastering scientific theory	62 (63%)	26 (42%)	$X^2 \geq 6\cdot 8$ is $\cdot 01 > p > \cdot 001$
c. Doing original scientific work	65	39	$X^2 \geq \cdot 136$ is $\cdot 8 > p > \cdot 7$
d. Getting research work published	33	16	$X^2 \geq 1\cdot 17$ is $\cdot 3 > p > \cdot 2$
e. Discussing research problems with other research students	16	12	$X^2 \geq \cdot 202$ is $\cdot 7 > p > \cdot 5$
f. Discussing research problems with your supervisor or other staff	26	11	$X^2 \geq 1\cdot 44$ is $\cdot 3 > p > \cdot 2$
g. Exchanging information and ideas with scientists doing research in different fields at seminars or conferences	16	5	(regrouped figures below)
h. Designing and constructing pieces of apparatus	28	25	$X^2 \geq 3\cdot 16$ is $\cdot 1 > p > \cdot 05$
i. Using sophisticated scientific equipment	15	14	$X^2 \geq 1\cdot 57$ is $\cdot 3 > p > \cdot 2$

Regrouped figures for (g)

*Scientists finding 'exchanging information and ideas . . . at seminars or conferences' a 'major' or a 'minor source of satisfaction' (rather than just a 'major source of satisfaction' as above)	64	27	$X^2 \geq 6\cdot 8$ is $\cdot 01 > p > \cdot 001$

items having been somewhat off target. The scientists at both stages of the interviewing put a lot of emphasis on the joys of practical work and this does seem inadequately reflected in these questionnaire responses. If the questionnaire items had been more on target though, this would not necessarily have affected the relative differences between the two groups.

With regard to the time course of the scientists' interest in their Ph.D. research, as TABLE 204 shows, there were no significant differences between the two groups until the third year. At that point, the interest of the eventual academics shot up and that of the eventual industrialists increased only slightly. It is tempting to link such a finding with the tendency of the eventual academics to stress the satisfaction of mastering scientific theory. The opportunities for this satisfaction are perhaps likely to be at their greatest towards the end of a research project when the overall significance of the findings will be most apparent. If a scientist had less interest in the theory, as such, but was more of an experimentalist, then it seems more likely that his interest and involvement in his work would be steadily maintained throughout.

There is some further evidence which assists in the interpretation of the questionnaire results. This comes from the interviews that were carried out with a subsample of scientists during the summer before they graduated. A short diversion will now be made to consider the bearing of this interview material on the evidence concerning the norms of science.

V. THE COMPARABILITY OF THE FIRST STAGE INTERVIEWS AND QUESTIONNAIRES

The replies the scientists gave to the pre-coded items on the questionnaire were, on the whole, remarkably consistent with their attitudes and opinions when they were given the opportunity to talk more freely in their subsequent interviews. One of the most important and conspicuous points of divergence however concerned their opinions on the norms of science. There was a systematic tendency for the scientists to appear more attached to the norms in their questionnaire responses than they did in their interviews. This divergence cropped up with particular frequency in relation to publishing. Whenever it occurred, it was always in the same direction — that is, that when talking in greater detail, the scientists were more prone to accept restrictions on scientists' freedom, for example in industry, as legitimate. In twelve of the twenty five cases of scientists interviewed this divergence over the norms occurred, and in nine of these cases, the divergence focused, at least partially, around their attitudes to publishing. Some examples may help to show the complexities that entered the scientists' views and made them look less in accordance with the ethos of science than they appeared in their questionnaires.

Example One

The main source of complexity in this man's views was idealism. This does not emerge very strongly in his specific statements about publishing (illustrated below), but these have to be seen in the context of a viewpoint which emphasised the importance of science in terms of its potential social usefulness, rather than as an activity geared to producing knowledge for knowledge's sake.

In his questionnaire, he indicated that industrial research scientists should have the right to publish their research work, but that in general, they do not have that right. Thus a fairly straightforward divergence was indicated between the right to publish an industrial scientist should have and the rights he does have. In his interview, by contrast, he said the following : —

> I've not got any strong feelings on it [i.e. publishing]. If you go into industry, most of the research you do you could publish — they'd have to vet it of course to make sure . . . well . . . if you were doing research for Unilever, to make sure that Proctor and Gamble couldn't benefit from it. Most of the stuff you could publish. A certain amount of it, if you happened to be in the development department for instance, there'd obviously be restrictions. You'd have to do your work secretly and not tell anyone else about it. If you go into that sort of job, then . . .

— You think you just have to accept it ?

> Mm. I've got a much wider grudge against publishing generally inasmuch as I wonder how much the Earth can take — you know, the volume of books. There's an awful lot of trivial stuff published. Who's going to read it all ? You can dig into little bits of course, but as well as the population explosion, there's a publication explosion. Libraries are filling up with great books that contain information. Future generations will use one per cent of this — that's exaggerated probably.

— But publishing wouldn't be important to you personally, or would it ?

I suppose it would be important to a scientific career — having an impressive output to get a job.

— So it's career reasons that would make you want to publish ?

I don't see why else people would want to publish — most of it is pretty mundane stuff.

Idealism was in fact one of the most common sources of divergence. The form it often took was the argument that, yes in an ideal world scientists should have freedom to publish, but that in practice, given the present state of society, such freedom neither could nor should be expected. Scientists should be thinking more of the contributions they could make to society than the special privileges and rights to which they might reckon they were entitled.

The other common source of divergence between the views expressed in the interviews and indicated in the questionnaires did not concern the answers given in the questionnaires, as such, but the interpretation to be put on them. For example, one scientist indicated in his questionnaire that industrial scientists should have the right to publish, but that they did not always have that right in practice. This could be taken as evidence for a traditional Mertonian viewpoint, but such an inference would be misleading. In his interview, he spoke as follows : —

Example Two

. . . like the question in your questionnaire — you must have freedom. I mean the driving force for a scientist, or one of the driving forces, when you want to progress is that you must have publications — certainly in academic spheres. And if you want to move about industry and get on the board and this sort of thing — this to me must be accepted if you go into industry — that you set your sights at an executive post. You don't want to work in the lab all the time, otherwise you will work in the lab all the time. You must set your sights. And to do that, you must communicate with other scientists in the same branch, the same discipline, i.e. you must publish. It's much more restricted in industry than it is in university — I know from my experience. Academically it's bad enough. You can find it quite difficult to publish things. I've got something I want to publish now and the prof — I work with the prof — isn't very keen on publishing it at all. My other co-supervisor is quite keen on publishing it, but because the prof is the god, that's it. In industry you get the same thing applying, but at a much greater level. People say, "oh you don't want to publish this so that the others know what we're interested in. If we're going to publish it at all, we'll publish it as a patent". And as you don't put much information in — well you put a lot of information in, but a lot of it is not relevant, so it's as bad.

What concerns this scientist is that scientists should have freedom to publish not for the sake of science, but for instrumental reasons, because it affects career chances. In one sense, his views could be said to conform to the norm of communality in that he does emphasise the importance of publishing. As Merton himself stressed, special altruistic motives do not have to be attributed to scientists. As long as they conform to the norm, their motives are immaterial.

However, while motivation may be immaterial in an academic setting, it could be crucial in an industrial setting where the game is a very different one and the same instrumental motives could lead to a very different outcome. For example, given this scientist's strong ambition, it seems plausible to suggest that if he found himself in an industrial post where the way upwards did not depend on publications, but on some other form of achievement, then he would probably switch his attention to that. As he says, he takes it for granted that in industry an executive post is what you set your sights on. In fact he had set his sights on an academic career and clearly publications would be important to him there.

The general point that emerges from the comparison between the first stage interviews and questionnaires is that scientists may say different, even apparently contradictory things, according to the level at which a question is pitched. If it is pitched rather generally and abstractly, as in the questionnaire, then their response is likely to be much more clearcut than if the question is part of a more detailed account of their views where they are also being asked more situationally located questions, like, "in what circumstances ?", "does that principle apply always ?", "are there any occasions when some other course of action might be legitimate ?". Thus asking a general question may trigger familiar platitudes which in fact have very little connection with what somebody would actually do in a real situation.

Such a point is similar to that made by Frank Parkin in his discussion of the contrast between the answers people sometimes give to abstract questions and their behaviour in real situations.[3] Abstract questions merely tap the "dominant value system". If Parkin's argument is applied directly to these results, then his "dominant value system" could be equated with the expression the scientists gave to the norms of science; and his "subordinate value system" with their more situationally contingent responses. The correct inference from this equation however is not that the Mertonian values are really more important. The value system about which there is consensus is not likely to be the most powerful. Indeed the reverse is more probable. Values upon which there is general agreement could be compared to everyday commonplaces. These may be precisely the values that are most impotent when it comes to deciding how to act in a particular situation.

The evidence here takes the argument back to Barnes and Dolby and the idea that scientists will resort to statements of the norms in situations of celebration, justification or conflict, but will not actually rely on them as a guide to action.[4] In other words, the norms of science are available in the culture of scientists and can be used as resources when convenient. Like slogans about the government or trade unions, property speculators or stock-brokers, they can be used when appropriate as a weapon, or defense, or even just as a reassuring common denominator when more specific thoughts fail to come to mind.

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[3]Frank Parkin, Class Inequality and Political Order (London, Paladin, 1972), pp. 92-5. For a similar argument see also: Hyman Rodman, "The Lower-Class Value Stretch", Social Forces, 42, 1963-64, pp. 205-15.

[4]"The Scientific Ethos: A deviant viewpoint".

The conclusion to be drawn from these points is that what limited evidence was found in the questionnaires for attachment to the norms of science should be interpreted cautiously. As TABLE 195 shows, the number of scientists indicating attachment to such views reached sixty per cent at most. As a guide to how scientists might act in practice or what conflicts they might experience, these results are likely to be an overestimate — and that is precisely what was found in the second stage.

V1. CAREER ORIENTATIONS

The picture of the eventual academics and eventual industrialists that is emerging from the questionnaires, reinforced by the first stage interview material, is of two groups alike in their values and in their perceptions of the career options open to them. They also seemed similar in their general orientations. That is, for the most part, they were not committed to pursuing research in the particular area of their Ph.D. research, and both groups had similarly high propensities to define interesting work in terms of its potential for throwing up challenging problems. Where the two groups did differ was in the general direction they tended to want to go in their careers. The eventual academics did have a significantly greater tendency to want to be involved with interesting scientific work, even though this tendency was not overwhelming (91 per cent of eventual academics rated it as 'necessary' or 'very important', as opposed to 69 per cent of eventual industrialists — see TABLE 197). And of course what was meant by 'interesting scientific work' is open to some doubt. Both groups emphasised 'career considerations', though the eventual industrialists did significantly more so. Neither group showed any strong preferences for jobs which permitted behaviour associated with the norms of science, although the eventual academics did significantly more so.

Thus although the two groups were significantly differentiated in some respects, there was a considerable amount of overlap between them. Rather than see any sharp divergences between the scientists who eventually became academics and industrialists respectively, it is probably more accurate to see many people in the two groups as very similar with some more single-minded people at the margins accounting for what differences in overall tendency there were.

All these points are crystallised if their profiles of ideal jobs are inspected. In the questionnaire, the scientists were asked: "Suppose that the job market were ideal and that you got your Ph.D.: what occupation would you most like to have ?" The results are set out in TABLE 216.

As is clear from TABLE 216, there were differences in the ideal jobs of the two groups. The eventual academics were, as might be expected, inclined to favour university jobs and the eventual industrialists, industrial jobs. However, there was also a considerable measure of overlap, largely because many of the eventual academics had similar preferences to the eventual industrialists.

TABLE 216

THE SCIENTISTS' IDEAL JOBS

Ideal job : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62
Industry	15	33
Research (location unspecified or various)	15	10
University	45	6
Teaching	3	2
Vague and various	7	2
Specific other	10	8
No information	4	1

Indeed these results suggest that, far from industrial scientists frequently being frustrated academics, if anything the converse is true — academics are more likely to be frustrated industrialists.

Such a view was reinforced by the evidence from the first stage interviews. Comparison between these and the questionnaires shows a consistent tendency for scientists to emerge as more favourably disposed towards industry when interviewed than they did in their questionnaires. For example, four out of the ten eventual academics interviewed who specified in their questionnaires that they wanted academic jobs, in their interviews talked of finding the prospect of an industrial job quite appealing, and none of them ruled it out. If this was a consistent tendency over the sample as a whole, then the alleged tendency of Ph.D. students to want academic jobs (for example, as asserted in the parliamentary paper on Postgraduate Education), begins to look very ill-founded. The discrepancy between the questionnaires and the interviews over this topic is perhaps another case of the process whereby different answers are given according to the level of generality and abstractness at which a question is pitched.

Vll. POSSIBLE CAUSES OF DIFFERENTIAL CAREER ORIENTATIONS

The fact that the eventual academics and eventual industrialists had a great deal in common is not surprising. The whole sample was, after all, a highly selected group. Nevertheless, despite their overall similarities, there were, as has been shown, some differences between the two groups. They tended to have different emphases in their conceptions of satisfying work and also tended to be orientated towards the employment area to which they were eventually destined. Given their overall similarities, what inclined one group towards a university career and the other towards industry ? Did any systematic factors lie behind these different preferences ?

Several possibilities were examined : —

1. Class. This was to test the implications of Box and Ford's theory, that is, the implication that working class students are more likely to become academic scientists. (See Science, Industry and Society, p. 62 and p. 73.)

2. Differences in academic performance.

3. The influence of supervisors.

4. Biases in the content of Ph.D. work.

Perhaps surprisingly, no significant differences were found between eventual academics and eventual industrialists in any of these respects. The following presentation of hypotheses and results will therefore be kept brief. Nevertheless, the discussion will show that some points of very considerable interest do emerge from this data.

HYPOTHESIS EIGHT Working class students will be more likely to become academic than industrial scientists

The items upon which the evidence for this hypothesis were based concerned the occupations and educations of the scientists' fathers. They were on page eleven of the questionnaire. Social class origins were attributed according to the Registrar General's Classification.[5]

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[5]Office of Population Censuses and Surveys, Classification of Occupations 1970, HMSO: SBN 11 690104 7.

TABLE 219

SOCIAL CLASS ORIGINS

Registrar General's Classification Class : —	Eventual Academic Scientists N=99		Eventual Industrial Scientists N=62	
1	14		10	
11	39	64	26	43
Non manual 111	11		7	
Manual 111	24		16	
1V	5	31	1	17
V	2		0	
Armed forces	1		0	
No information	3		2	

Probability under Ho that $X^2 \geq 0{\cdot}26$ for df=1 is $\cdot 7 > p > \cdot 5$

The lack of significant differences between the class origins of the eventual academics and eventual industrialists means that Box and Ford's hypothesis has to be rejected for this sample of scientists.

HYPOTHESIS NINE Eventual academics will have given what are conventionally considered better academic performances than eventual industrialists

The aim of this hypothesis was to test the common idea that universities, being the favoured career option for scientists, usually keep the most able scientists, while industry has to make do with the academic leftovers.

The measures of performance that were used were : —

a. first degree results

b. the number of publications to date

c. the number of difficulties and setbacks experienced in the course of doing their Ph.Ds.

TABLE 220

CLASS OF FIRST DEGREE

B.Sc. degree class : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
Honours: class 1	48	20	$X^2 \geq 3{\cdot}9$ is $\cdot05 > p > \cdot02$
2.1	33	24	$X^2 \geq 0{\cdot}47$ is $\cdot5 > p > \cdot3$
undivided 2	6	9	
2.2	8	6	
Other	4	3	

Grouping the '2.2' and 'Other' degrees together yields:
Probability under Ho that $X^2 \geq 5{\cdot}3$ for df=3 is $\cdot2 > p > \cdot1$

TABLE 221

NUMBER OF PUBLICATIONS

Number of papers already published at time of questionnaire : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62
4 or more	7	1
2 or 3	25	8
1	24	16
none	38	35
no information	5	2

Collapsing bracketed categories together for purposes of the test yields probability under Ho that $X^2 \geq 7{\cdot}02$ for df=2 is $\cdot 05 > p > \cdot 02$

Difficulties and setbacks

The questionnaire items for these are on page three. As can be seen they are quite complex and the answers were of course correspondingly complex. The scientists reported experiencing a considerable number of difficulties and setbacks. They tended to be at their most acute in the first year of research and then to fall off gradually in the subsequent years. The results were assembled and some X^2 tests performed on those most likely to yield significant differences between the two groups. No significant differences were found at all and so no further tests were carried out. In general the two groups appear to have been alike in their experience of difficulties and setbacks.

<u>HYPOTHESIS TEN</u> <u>The eventual industrialists will have tended to have supervisors who are more industrially orientated and the eventual academics supervisors who are more academically orientated</u>

The scientists were asked to indicate how far various statements applied to their supervisors (questionnaire page five). On the hypothesis, the following results would be expected : —

<u>Eventual industrialists</u> would be more likely:
to affirm that their supervisor : —

a. has worked in industry.

b. has contacts with industry.

and to deny that he : —

c. regards most industrial research as having little <u>scientific</u> significance.

d. thinks that the freedom of industrial research scientists is sometimes unduly restricted.

<u>Eventual academics</u> would be more likely:
to affirm that their supervisor : —

e. is very concerned that the research he undertakes and supervises should be published.

f. has a wide scientific reputation.

g. has a good scientific reputation in the department.

<u>Both groups</u> would be equally likely:
to affirm that their supervisor : —

h. is deeply interested and involved in his work.

TABLE 223[6]

SOME CHARACTERISTICS OF SUPERVISORS

Scientists affirming (answering 'Yes' and 'Yes to some extent') that their supervisor : -	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
a. has worked in industry	23	14	$X^2 \geqq \cdot 037$ is $\cdot 95 > p > \cdot 9$
b. has contacts with industry (via consultancies etc.)	55	41	$X^2 \geqq 1\cdot 79$ is $\cdot 2 > p > \cdot 1$
Scientists denying (answering 'No' and 'Quite the opposite') that their supervisor : -			
c. regards most industrial research as having little <u>scientific</u> significance	51	30	$X^2 \geqq \cdot 13$ is $\cdot 8 > p > \cdot 7$
d. thinks that the freedom of industrial research scientists is sometimes unreasonably restricted	4	3	(regrouped figures below)
Scientists affirming (answering 'Yes') that their supervisor :-			
e. is very concerned that the research he undertakes and supervises should be published	49	37	$X^2 \geqq 1\cdot 72$ is $\cdot 2 > p > \cdot 1$

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[6] It was not possible to present the figures in this table so that they were entirely comparable because of the small numbers in some of the categories. With item (a), the 'Yes' and 'Yes to some extent' responses had to be combined; so this was also done for item (b) to make it comparable. With item (d) even the combined 'No' and 'Quite the opposite' responses failed to yield numbers big enough for the X^2 test. The affirmative answers were therefore used instead.

TABLE 223 (continued)

Scientists affirming ('answering 'Yes') that their supervisor : -	Eventual Academic Scientists	Eventual Industrial Scientists	Probability under Ho for df=1 that : -
f. has a wide scientific reputation	44	32	$X^2 \geqslant \cdot 97$ is $\cdot 5 > p > \cdot 3$
g. has a good scientific reputation in the department	61	38	$X^2 \geqslant \cdot 028$ is $\cdot 9 > p > \cdot 8$
h. is deeply interested and involved in his work	68	45	$X^2 \geqslant \cdot 16$ is $\cdot 7 > p > \cdot 5$
Regrouped figures for (d)			
Scientists answering 'Yes' or 'Yes to some extent' that their supervisor thinks that the freedom of industrial research scientists is sometimes unreasonably restricted	24	17	$X^2 \geqslant \cdot 16$ is $\cdot 7 > p > \cdot 5$

As can be seen from TABLE 223, there were no significant differences in the way the eventual academics and eventual industrialists described their supervisors.

Interestingly, emerging from these figures is an important confirmation of the idea that supervisor/student relationships are confined to technical matters and could not therefore act as a channel for the transmission of general precepts. A large number of both eventual academics and eventual industrialists indicated that they did not know what their supervisors thought about industrial scientists.[7] If Merton's stress on precept and example is correct, then it would be expected that the students would know what their supervisors thought about these matters. By contrast, the Kuhnian picture predicts that the students' knowledge of their supervisors' attitudes would be mainly confined to matters concerning work content — as indeed it seems to have been. However, both student and supervisor would have occasion to judge the significance of industrial research in the ordinary course of following the literature in their field (that the students' reading was not, on the whole, confined to papers written by academic scientists is shown in TABLE 226). In conformity with the Kuhnian picture, about two thirds of the students did indeed know their supervisors' attitudes on these technical matters. The topic of the freedom of industrial scientists is only likely to occur in more general discussion — hence the students' greater ignorance. Less than a third knew their supervisors' attitudes on this point.

Further confirmation of the scientists' ignorance of their supervisors more general attitudes is contained in their answers to questionnaire items on the Rothschild Report.[8] This came out and caused a great controversy in scientific circles a few months before my questionnaire was sent out. Eighty per cent indicated that their supervisors had not mentioned or shown any interest in the report. Fifty per cent indicated that they did not know what their supervisor's

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[7]Taking both groups together, 69 per cent did not know whether their supervisors thought that the freedom of industrial scientists is sometimes unduly restricted and 36 per cent did not know what their supervisors thought about the scientific significance of industrial research.

[8]"A Framework for Government Research and Development", (Cmnd. 4814, November 1971).

judgement of the report would be. Given the outrage the publication of this report provoked and the threat it was seen to pose, its lack of penetration into the everyday discourse of staff and research students is a truly remarkable confirmation of the cognitive and technical nature of that interaction. Kuhnian finger exercises rather than grand values are the currency of the socialisation of scientists.

HYPOTHESIS ELEVEN The Ph.D. research of eventual industrialists will tend to have been more industrially orientated than that of the eventual academics

The questionnaire items for this hypothesis concerned the sources of the scientific literature the scientists had had to read for the purposes of their research, and the relevance of their research to industry.

TABLE 226

SOURCES OF LITERATURE READ FOR PH.D. RESEARCH

Answers to the question: "Is the scientific literature you have had to read, for the purposes of your research, written by : —	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62
only university scientists	23	6
mainly university scientists	60	38
both industrial and university scientists	14	17

Probability under Ho that $X^2 \geq 7{\cdot}06$ for df=2 is $\cdot05 > p > \cdot02$

TABLE 227

INDUSTRIAL RELEVANCE OF PH.D. RESEARCH

Answers to the question: "Would you think the research you are doing . . .	Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62
is of interest only to university scientists.	46	18
might be of interest to industrial research scientists.	35	22
might have industrial applications.	11	12
does have industrial applications.	7	9
no information	-	1

Leaving out the individual for whom there was no information, probability under Ho that $X^2 \geq 6{\cdot}4$ for df=3 is $\cdot 1 > p > \cdot 05$

Given the lack of any significant differences between the eventual academics and eventual industrialists in the respects considered under hypotheses 8-11, the question remains: were there any systematic factors which lay behind the differing orientations of the two groups ?

Further exploration of the data revealed only one clue. There was some evidence that the differing orientations of the two groups dated from at least the time when they had decided to do a Ph.D. The retrospective reasons the scientists gave for wanting to do a Ph.D. picked up the differing career orientations of the two groups. Eventual academics cited academic ambitions more often than eventual industrialists and eventual industrialists cited career reasons more often.[9] The results were as follows.

TABLE 228

TWO REASONS FOR WANTING TO DO A PH.D.

Reasons		Eventual Academic Scientists N=99	Eventual Industrial Scientists N=62	Probability under Ho for df=1 that : -
*Hoped eventually to secure a university post	major	23 (23%)	6	
	minor	33	12	
	total	56	18	$X^2 \geqq 10 \cdot 59$ is $\cdot 01 > p > \cdot 001$
*Thought it would improve career prospects generally	major	30 (30%)	41 (66%)	$X^2 \geqq 22 \cdot 4$ is $\cdot 001 > p$
	minor	36	15	
	total	66	56	

The absolute level of the figures is as telling as any differences between the two groups. In particular, there is a marked asymmetry between the number of eventual academics indicating that they 'hoped eventually to secure a university post' and the number of eventual industrialists citing 'career prospects' as a

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[9]There were no significant differences between the two groups on any of the other reasons — see questionnaire, page one.

major reason for wanting to do a Ph.D. Indeed more of the eventual academics (thirty per cent) joined with the eventual industrialists in hoping to improve general career prospects. These results strengthen the suggestion made earlier that academics are more likely to be frustrated industrialists than vice versa. They also indicate that the relative weakness of any academic orientations was not a recent development, but had existed since at least the time when the scientists had decided to do a Ph.D.

CHAPTER V11

SUMMARY AND SUGGESTIONS FOR FURTHER RESEARCH

The aim of this thesis has been to close a gap in the growing evidence against an influential theory in the sociology of science. This theory stems from the ideas of Robert Merton. His conception of science is that of a social institution governed by a set of special norms — universalism, communism, disinterestedness and organized skepticism — known collectively as the ethos of science. A scientist will be socialised into these norms by precept and example during his academic education and they will then tend to become incorporated into his conscience as moral values.

A number of other writers, such as, Barber, Hagstrom, Kornhauser and Storer have drawn on this work in developing the theme that academically trained scientists who go into industry will suffer a conflict of values. The ethos of science and the ethos of industry are held to be different. Other writers like Avery, Abrahamson, Marcson and Box and Cotgrove have modified this 'value conflict thesis' and explained the frequently observed lack of conflict by postulating processes of accommodation. The most thorough of these studies has been Box and Cotgrove's. Lack of conflict for their sample was explained by the idea that scientists are differentially socialised into the norms of science. They argued that working class students would be more likely to become dedicated to science because this could provide a solution to an identity crisis engendered by marginality. Further work by Ellis and Barnes has likewise failed to find evidence of value conflicts amongst industrial scientists, but they have not sought to accommodate their results to Merton's theory. Barnes, in particular, has rejected the idea that scientists are socialised into the norms of science and, drawing on the 'situational adjustment' theory of Howard Becker, has developed a very different perspective. This emphasises personal change and the effects of the situation on the individual's structuring of his experience.

Although the drift of empirical findings away from value conflict has been steady, the empirical issue is by no means clear-cut. Apart from the fact that some of the results have been American and some British, most British findings (Box and Cotgrove and Ellis) have used mixed samples of B.Sc. and Ph.D. scientists. Barnes used only B.Scs. The possibility remains that value conflict would be observed if only Ph.D. scientists were studied because they would have had more prolonged exposure to the 'ethos of pure science'. The present study therefore looked at Ph.D. scientists who went into industry. 357 final year Ph.D. students in 34 university departments of physics and chemistry returned a questionnaire on their attitudes, values and beliefs about academic and industrial science. A subsample of 25 of these scientists were also interviewed. All the scientists were then followed into their jobs and forty of them who took industrial employment were interviewed when they had been at work for about one year.

In the interviews, the scientists' attitudes to two allegedly central academic norms were examined via three questions: (1) freedom to choose projects; (2) freedom to work in their own way; (3) freedom to publish. (1) and (3) were not a source of concern, nor indeed of very much interest to the scientists. 'Freedom to work in their own way' was of interest, but not because it was a value related to the nature of science. It was important to them for pragmatic and industrial reasons: they were highly qualified and competent people who had been employed to do responsible jobs. Therefore they felt that to be unduly supervised would reflect poorly on their competence.

At most two out of the forty scientists appeared to be attached to the norms of science, and even this attachment did not provoke value conflicts or dissatisfaction. Much more conspicuous was the almost total acceptance of the industrial ethos. Whatever the process of socialisation that is undergone during a scientific training, it is not such as to give rise to value conflicts in industry.

The interviews were designed to provide more material than was necessary for the purely negative task of criticising the value conflict theory and it was possible to build up from them a positive picture of the industrial scientists. They were in no way orientated towards academic life — indeed it was sometimes viewed with contempt. They had entered industry whole-heartedly to make a career and were eager to make a competent and useful contribution to it. It became clear that industrial science is a very diverse activity which has not been at all adequately captured by the rather stereotyped picture of the value conflict theorists. For example, the way work is initiated, carried out and monitored frequently makes it inappropriate to talk of 'projects' or to enquire into 'freedom to choose projects'. Again, the stress which some investigators have placed on 'recognition' was also inappropriate for this sample. These scientists were not preoccupied with recognition or rewards, let alone predominantly motivated by them. Instead, they seemed to operate as 'self-regulating mechanisms', depending for guidance mainly on the technical feedback that arose in the ordinary course of their work.

It seemed possible to attribute the scientists' confident independence in work to their Ph.D. trainings. These had equipped them with generalisable problem solving skills. However, it was not only that they were able to apply themselves to a wide diversity of problems; they also seemed genuinely interested in doing so. Their primary motivation appeared to be an interest in problem solving, as such, rather than just the problems of pure science.

The finding that the scientists who had gone into industry seemed so flexible and industrially orientated raised the question of whether they were typical Ph.D. science graduates or a highly selected and especially industrially orientated group. The first stage questionnaires permitted a comparison of these eventual industrialists and those of their fellow students in the sample who eventually became academic scientists. This showed that, while at university, there had been no significant differences in their replies to questions about the norms of science. Most of them indicated that, in some degree, the norms of autonomy and communality were appropriate in academic life, though to a considerably lesser extent in industrial life. Both groups also had very similar images of industry and the amount of freedom to be expected there. They in fact indicated that industrial scientists do not have as much freedom as they felt they should have. The main respect in which the two groups did differ significantly was in their overall career orientations. The eventual academics, not surprisingly, had a greater tendency to want the freedoms associated with academic life and the eventual industrialists to favour more worldly considerations of salary and promotion prospects. The two groups also has different biases in the patterns of their interest in work. These were tentatively equated with theoretical and experimental orientations.

At first sight, the value conflict theorists might have found some comfort in the questionnaire results on the norms of science, especially in the discrepancy between the freedom the scientists indicated industrial scientists should and actually do have. This would surely suggest that the sample contained scientists who, if they went into industry, would be discontented. From the second stage of the study however, this was found not to be so. One reason was that, in many cases the scientists' industrial jobs turned out better with respect to these freedoms than they expected. More importantly, the questionnaire responses turned out to be unreliable indicators of how scientists would feel and behave when faced with the exigencies of real situations. It was clear from the interviews at both stages that the motivation behind the few apparent expressions of Merton's norms was not in the spirit of the 'ethos of science'. The scientists' own career concerns were much more prominent. This explains the majority view that academic scientists should have freedom to publish, but that freedom to work in their own way and to choose projects are much less important.

These results are evidence against the idea that values acquired during scientific training function as conditioned determinants of behaviour. It appears, rather, that both groups had what was called the same 'cognitive map' of their social environment. They knew the different entries and locations on the map and the kind of behaviour and values appropriate to those locations. With this in common, their differences were simply in where they wished to travel on that map. This cognitive map theory stresses the role of information as a crucial variable. It allows for a conception of action which is more informed, intelligent and calculating than does Merton's model of socialisation. On the latter view, men acquire stable emotional orientations. On the former view, the possession of a cognitive map, or knowledge of society, will ensure that simple mismatches of the value conflict kind do not occur.

In an effort to locate the sources of the divergent aims of the industrial and academic groups, a number of possibilities were examined. These were degree class, research topic, supervisors' attitudes and social class. None of them were found to coincide with the desire to go into industry or stay in academic life. It is interesting, though, to see that: (1) there was no difference in the ability, as measured by degree class and number of academic papers published, between the eventual academics and industrialists; and (2) Box and Ford's class hypothesis was not confirmed for this sample.

Variation in social class was not related to academic orientation.

Further confirmation of the idea that university staff do not play a significant role in transmitting general values also emerged from this material. The scientists were found to be largely ignorant of their supervisors' attitudes on various general issues associated with the ethics of industrial and academic science, but to have more idea of their views on the contents of industrial research, as such. For example, only twenty per cent of the research students indicated that their supervisors had either mentioned or discussed the Rothschild Report. This had recently been published and its considerable implications for the organisation and funding of scientific research were being widely discussed in newspapers, radio and television.

One brief, but overstated, way of summarising the findings of this study is by reversing Merton's theory. Instead of thinking of the research students as being socialised into the norms of science and then having value conflicts when they went into industry, they can be seen as adhering to the values of society and having value conflicts associated with academic life. The requirement of practical usefulness would come into conflict with the aim of knowledge for knowledge's sake. Certainly the belief that a lot of the work that is carried out in universities is socially irrelevant was very widespread in the present sample. Consequently when these students eventually left university to take up industrial jobs, they could settle into industry with a feeling of relief and a sense of 'natural propriety'. This reversal of Merton's theory is useful, but does not fit the findings exactly. For example, it is probably an exaggeration to say that the research students who went into industry suffered value conflicts while at university. Some of them saw their scientific education as a training for non-academic careers or believed in the potential usefulness of much pure research. Such beliefs therefore fitted into a worldly set of values. The scientists were also prepared to suspend the ordinary values of life for a while. The university experience could be encapsulated and still have a legitimate place on the cognitive map.

Two main conclusions can be claimed for this study. The first conclusion is that the remaining gap in the evidence against Merton's theory can now be closed. The possibility that value conflict between the norms of pure science and those of industry would occur if the sample consisted only of highly trained Ph.D. scientists proved not to be the case. The study also contained clues as to why this theory has been so tenacious. The evidence in CHAPTER Vl showing that the more abstractly a question was pitched, the more stereotyped and idealistic was the response, indicates how the value conflict theorists could have got the answers they did about the operation of the norms of science. The abstract questions in their interview schedules and questionnaires (see, for example, Box and Cotgrove's questionnaire) are likely to have provoked answers of an equally abstract type. Undoubtedly, there is something in such answers: they indicate what verbal resources are available in the culture and at the scientists' disposal. The only mistake is to assume, as the value conflict theorists have done, that statements of the norms of science are the whole story and a reliable indicator of behaviour.

The second main conclusion of the study is that rather than having a set of scientific values, the industrial scientist appeared to be equipped with two types of information: his cognitive map and the skills of his trade. This picture could be said to straddle Merton's theory, dealing with factors which are either more general or more particular than at his level of analysis. As far as his cognitive map is concerned, the industrial scientist is probably best assimilated to other middle class professional or managerial groups for the purposes of study and comparison. As far as his scientific skills and problem solving orientations are concerned, the style of analysis derived from Kuhn's work captures the tone of his involvement admirably.

The approach derived from Becker with its emphasis on 'making out' undoubtedly has some application. Like others in the professional middle class, these scientists were ambitious. This could be said to be a feature of their cognitive map — it included a career route in it. However, Becker's approach does not provide the resources with which to capture the real involvement in problems and enthusiasm these scientists had for the contents of their work. If this were merely a matter of characterising the scientists' emotional orientation to their work, then sociologically it might not be very interesting. However, the discussion of Kuhn's work should have shown that the compulsion to solve problems goes right to the heart of science. The institution of science works by being a puzzle-solving tradition.

Ph.D. scientists are, however, an extremely select group. Do the findings of this study have any wider applicability ?

Comparing these highly trained and qualified scientists with Barnes' B.Sc. sample suggests some possibilities. Barnes' sample were far less able to depend on their qualifications to give them an ascribed status in industry. Instead, they had to display their competence conspicuously in a bid to achieve recognition and status. This suggests that Beckerian 'making out' processes are likely to be most prominent in situations where people's roles are ill-defined and their qualifications for performing them diffuse. The general sociological implications of Kuhn's theory, on the other hand, are likely to apply whenever people are carrying out jobs for which they have been trained into a tradition of workmanship which they and their employers can recognise and take-for-granted. Doctors, lawyers, architects, musicians and craftsmen of many kinds may all be revealingly compared with these Ph.D. scientists. The important point is that such groups of people will not be properly understood if their social relationships are analysed independently of the skills and knowledge which they possess. Separating the sociology of the practitioners of a body of knowledge (or art) from the sociology of the knowledge itself is likely to be self-defeating. In the sociology of work, such a point would be trite: one would not expect to understand coal-miners' social relationships in isolation from the contents of the work they do. In the sociology of science, such a standpoint has tended to dominate research.

On a general level, the idea of cognitive maps has very considerable applicability. One area for further study concerns the extent to which people share cognitive maps, but make different choices on the basis of them. This relates to one of the main outstanding issues of this research: what factors lay behind the eventual academics' and eventual industrialists' choices of employment ? As has been stressed, there was no question of their preferences correlating with more favourable images of the respective goals — they shared cognitive maps. The finding that future academics and industrialists seemed to be biased in favour of theoretical and experimental work may be worth pursuing. As yet, it is unclear whether these biases were superficial or represented more deeply rooted cognitive styles. To trace the nature and origin of such differences, however, might well throw light on processes that remained obscure in this study. By going backwards chronologically and looking at the patterns of influence on schoolboys, it might also be possible to investigate both the way in which cognitive maps are built up and how individual preferences within them are conceived.

In conclusion, two further topics of research, which emerge directly from this study will be outlined. The first topic concerns those scientists who were not followed up in the second stage of this study — that is, the research students who stayed in academic life. Their fate becomes interesting in the light of the idea that the very opposite of Merton's theory seemed to apply to the industrial sample. On the mirror-image view, the academics should be having their value conflicts in a university setting. It is certainly true that more of the eventual academics had wanted industrial jobs than vice versa.

These academics should be followed up to see how deeply rooted and persistent such preferences were. Did they cling on to the utilitarian and practical values of society and suffer 'value conflicts' and discomfort in their academic careers, or did they fairly easily become reconciled with them ?

The second topic of research emerges from the finding that the Ph.D. scientists who went into industry seemed to be so flexible and industrially orientated. If this is generally true, what is the origin of the stereotype that highly qualified scientists are narrowly specialised and fit only to inhabit ivory-towers ? It would be particularly interesting to investigate this idea in the minds of employers of Ph.D. scientists. Are their beliefs linked to any evidence ? And if so, what is it ? It seems plausible that their assertion of these views is comparable to the abstract values the scientists in this study occasionally expressed. That is, when questioned by sociologists, government report-writers or journalists, industrial managers may reiterate these stereotypes about highly qualified scientists. On the other hand, when faced with the practical exigencies of running their companies and recruiting employees, they may well be more inclined to recognise, appoint and reward Ph.D. scientists — people who, on the basis of this study, will be eminently suited to industrial employment.

APPENDIX

CORRESPONDENCE, QUESTIONNAIRE AND INTERVIEW SCHEDULE

Included in the appendix are copies of the following : —

1. Initial letter to heads of science departments.

2. First stage questionnaire and introductory letter.

3. Request for job information and reply form.

4. Second stage interview check list.

Other correspondence, like reminder letters and more individual arrangements for interviews, is not included.

1. Initial letter to heads of science departments

December, 1971

Dear

I am writing to ask whether you would be kind enough to help me with a study I am doing by sending me the names of the final year Ph.D. students in your department.

The study concerns the employment prospects and subsequent careers of scientists with Ph.Ds, and I am carrying it out with the support of a Social Science Research Council studentship. It is an attempt to develop the findings and conclusions of a similar study I was engaged upon (at the Science Studies Unit, University of Edinburgh) confined entirely to B.Sc. graduates. I have discussed the details of the project with Dr. Robinson, the Education Officer at The Royal Institute of Chemistry, and he is of the opinion that it is very worthwhile, particularly at the present time.

During the first stage of the study, I hope to cover a fairly large number of people by means of a postal survey, and it is here that your assistance would be invaluable. If you could send me the names of final year Ph.D. students in your department (if possible with their addresses if they cannot be contacted via the department) this would enable me to contact them directly with a questionnaire early next year. The questionnaire will be fairly short and should, in no way, interfere with their work.

Finally, may I assure you that any information on the names of your students you give me, will be used only for the purposes of this study. I should be very grateful indeed if you could help me, and look forward to hearing from you soon.

Yours sincerely,

Celia Merrick

2. First stage questionnaire and introductory letter

March, 1972

Dear

I am writing to you in the hope that you will be willing to help with a study I am doing by filling in the enclosed questionnaire.

Recently, there has been a great deal of concern over the employment prospects of highly qualified scientists and the way in which their skills and abilities are utilised. Despite this concern, very little is known about the way the scientists themselves view their training and science in general and their reasons for choosing particular jobs.

Hence this study, which is concerned with the training and subsequent careers of postgraduate scientists. It is an attempt to develop the findings and conclusions of similar studies I have been engaged upon, confined to B.Sc. graduates. The present study is in two stages. The first stage involves a survey addressed to a large number of final year Ph.D. science students all over Britain. At the second stage, it is hoped to follow some of you into the jobs you have taken and to see what experiences you have and what your problems and prospects are.

For the purposes of the study, the head of your department has been kind enough to send me your name, along with those of other final year Ph.D. students in your department. The questionnaire is not as long as it looks, because many of the questions offer you a number of possible answers — you are requested merely to ring a code for the answer that most closely approximates to your opinion or your situation. Occasionally written replies are requested. At times, you will undoubtedly feel that the answer you give by ringing a code does not express your opinion accurately. In these cases, please try to ring a code, but also add any comments or qualifications you wish. These, along with any other remarks you might want to make, will be very helpful.

Finally, may I assure you that your replies will be treated with the strictest confidence. Any information you give will not be attributable or traceable to you, or your department, in any material that may be published, and the data will be accessible only to the research team.

I should be very grateful indeed if you do have time to help and look forward to receiving your completed questionnaire soon.

Yours sincerely,
Celia Merrick.

SERIAL NUMBER

SECTION ONE

We should first like a few details about your first degree and your decision to do a Ph.D.

At which university did you do your first degree ? 8

Please indicate the type and class of your first degree:- 9

	Ordinary	0
Honours:	class 1	1
	Undivided 2	2
	2.1	3
	2.2	4
	3	5
	Other (specify)	6

(ring one)

Please consider the following factors and indicate, by ringing a code opposite each, whether, in your decision to do a Ph.D., each was : -

	a major reason	a minor reason	irrelevant	a disincentive	
Interest in and enthusiasm for science.	1	2	3	4	10
Interest in a particular scientific problem.	1	2	3	4	11
Hoped eventually to secure a university post.	1	2	3	4	12
Alternatives did not appeal.	1	2	3	4	13
Thought I had the ability.	1	2	3	4	14

(Questionnaire p. 1, continued)

Finals results made it possible.	1	2	3	4	15
Thought it was necessary for an interesting research career in science.	1	2	3	4	16
Thought it would improve career prospects generally.	1	2	3	4	17
Wanted to keep eventual job options open.	1	2	3	4	18
Encouragement of university staff.	1	2	3	4	19
'Canvassing' of university staff.	1	2	3	4	20
Liked university life.	1	2	3	4	21
Any other reasons ?					22

(Questionnaire p. 2)

SECTION TWO

This section concerns your present research

As a research student, are you in your : -	3rd year	1	23
	4th year	2	
	5th year or more	3	
		(ring one)	

Are you likely to complete your research before : -	Summer 1972	1	24
	December 1972	2	
	Later	3	
		(ring one)	

(Questionnaire p. 2, continued)

Have you been seconded by industry or the civil service ? (If yes, please give brief details)

	Yes	1	25
	No	2	
		(ring one)	

Are you sponsored by industry or the civil service ? (If yes, please give brief details)

	Yes	1	26
	No	2	
		(ring one)	

What is (are) your supervisor's name(s) ?

What is the subject of your research ? 27

Could you list, in order of importance, the pieces of equipment which you make central use of in the course of your research ? If one particular piece seems to you to be crucial or fundamental to your topic, please underline it. 28

(Questionnaire p. 3)

How much time have you had to spend building, adapting or learning to operate the special equipment described above ? Please indicate whether, in the following periods, you spent approximately : -

	90-100% of your time	75-90%	50-75%	25-50%	0-25%	
1st year of your research	1	2	3	4	5	29
2nd year research	1	2	3	4	5	30
3rd year research	1	2	3	4	5	31
4th and subsequent years (if applicable)	1	2	3	4	5	32

Is the scientific literature you have had to read, for the purposes of your research, written by : -

only university scientists	1	33
mainly university scientists	2	
both industrial and university scientists	3	
mainly industrial scientists	4	

(ring one)

Almost all research students experience difficulties and setbacks in the course of their research. Please indicate, by ticking the appropriate space (or by using a double-tick if the problem has affected you very acutely) if the following problems have beset your research during your : -

	1st year of research	2nd year	3rd year	4th and subsequent years (if applicable)	
A false start which led to a change of topic					34
Delays in getting apparatus or equipment					35
Poor or inadequate facilities					36

(Questionnaire p. 3, continued)

Practical difficulties in using equipment or getting it to work					37
Boredom or lack of enthusiasm for science					38
The feeling that you were isolated and had no one with whom to discuss research problems					39
Lack of results					40
Doubts about your own ability or competence					41
Lack of help from your supervisor(s) or other staff					42

Any other problems or difficulties : - 43

(Questionnaire p. 4)

Would you think the research you are doing : -

is of interest only to university scientists.	1	44
might be of interest to industrial research scientists.	2	
might have industrial applications.	3	
does have industrial applications.	4	

(ring one)

Have you already, or are you likely to publish any of your present research work ? Please indicate the number of papers you have : -

already published ________ 45

are likely to publish ________ 46

(Questionnaire p. 4, continued)

Different people find satisfaction (or frustration) in different aspects of scientific research. Please consider the following and indicate how you feel by ringing a code opposite each statement as follows : -

1 = a major source of satisfaction; 2 = a minor source of satisfaction; 3 = irrelevant or doesn't arise; 4= a minor source of frustration; 5 = a major source of frustration.

Designing and constructing pieces of apparatus.	1	2	3	4	5	47
Mastering scientific theory.	1	2	3	4	5	48
Doing original scientific work.	1	2	3	4	5	49
Getting research work published.	1	2	3	4	5	50
Using sophisticated scientific equipment.	1	2	3	4	5	51
The general activity of solving and overcoming problems.	1	2	3	4	5	52
Discussing research problems with other research students.	1	2	3	4	5	53
Discussing research problems with your supervisor or other staff.	1	2	3	4	5	54
Exchanging information and ideas with scientists doing research in different fields at seminars or conferences.	1	2	3	4	5	55
Any other satisfactions or frustrations : -						56 57

(Questionnaire p. 5)

In the previous question, you have indicated which aspects of your research you have found satisfying or frustrating. Can you now indicate how interested you have been, on balance, in your research:

	very interested	quite interested	slightly interested	not interested	actively disliked	
During your first year of research	1	2	3	4	5	58
Second year of research	1	2	3	4	5	59
Third year of research	1	2	3	4	5	60
Fourth and subsequent year (if applicable)	1	2	3	4	5	61

This question concerns a few characteristics of your supervisor(s). Please indicate how far each of the following statements applies to him by ringing a code opposite each (or if you have two supervisors, by underlining, in addition, the codes for the second supervisor), as follows : -

1 = Yes; 2 = Yes, to some extent; 3 = No; 4 = Quite the opposite; 5 = Don't know.

Has worked in industry.	1	2	3	4	5	62
Has contacts with industry (via consultancies etc.).	1	2	3	4	5	63
Is deeply interested and involved in his work.	1	2	3	4	5	64
Is very concerned that the research he undertakes and supervises should be published.	1	2	3	4	5	65
Has a wide scientific reputation.	1	2	3	4	5	66
Has a good scientific reputation in the department.	1	2	3	4	5	67
Regards most industrial research as having little <u>scientific</u> significance.	1	2	3	4	5	68
Thinks that the freedom of industrial research scientists is sometimes unreasonably restricted.	1	2	3	4	5	69

(Questionnaire p. 5, continued)

Are you, on the whole, glad you stayed on to do a Ph.D., or do you regret it ? Why ? 70

71

(Questionnaire p. 6)

SECTION THREE

It has been claimed that scientists lay great stress on the importance of having freedom in their work. In this section, we are interested in the sort of freedom you think scientists should have and the particular emphases that you think should be placed on scientific research carried out in different establishments.

This question concerns the sort of research that you think, (a) should be done, and (b) actually is done by academic and industrial scientists respectively; and the sort of rights you think they, (a) should have, and (b) actually do have. Please consider the statements below and indicate your attitude to each by ringing a number by YES, DEPENDS, NO, or DK (don't know) in each box. If you would like to elaborate any of your replies, particularly where you have made 'DEPENDS' entries, please do so on the back of the previous page.

	Academic scientists should ...	Academic scientists in general do ...	Industrial research scientists should ...	Industrial research scientists in general do ...	
have the right to publish their research work.	YES 1	YES 1	YES 1	YES 1	8
	DEPENDS 2	DEPENDS 2	DEPENDS 2	DEPENDS 2	9
	NO 3	NO 3	NO 3	NO 3	10
	DK 4	DK 4	DK 4	DK 4	11
have the right to decide which research projects they will work on.	YES 1	YES 1	YES 1	YES 1	12
	DEPENDS 2	DEPENDS 2	DEPENDS 2	DEPENDS 2	13
	NO 3	NO 3	NO 3	NO 3	14
	DK 4	DK 4	DK 4	DK 4	15

(Questionnaire p. 6, continued)

have the right to carry out research projects in the way they think best.	YES 1	YES 1	YES 1	YES 1	16
	DEPENDS 2	DEPENDS 2	DEPENDS 2	DEPENDS 2	17
	NO 3	NO 3	NO 3	NO 3	18
	DK 4	DK 4	DK 4	DK 4	19
rate the practical applicability of their research work more highly than its relevance to 'pure' science.	YES 1	YES 1	YES 1	YES 1	20
	DEPENDS 2	DEPENDS 2	DEPENDS 2	DEPENDS 2	21
	NO 3	NO 3	NO 3	NO 3	22
	DK 4	DK 4	DK 4	DK 4	23
develop any potentially useful aspects of their research projects as thoroughly as possible.	YES 1	YES 1	YES 1	YES 1	24
	DEPENDS 2	DEPENDS 2	DEPENDS 2	DEPENDS 2	25
	NO 3	NO 3	NO 3	NO 3	26
	DK 4	DK 4	DK 4	DK 4	27
work out any potentially theoretically significant aspects of their research projects as thoroughly as possible.	YES 1	YES 1	YES 1	YES 1	28
	DEPENDS 2	DEPENDS 2	DEPENDS 2	DEPENDS 2	29
	NO 3	NO 3	NO 3	NO 3	30
	DK 4	DK 4	DK 4	DK 4	31

(Questionnaire p. 7)

The next three questions are concerned with the constraints on the choice of academic research projects.

The financial influence of industry and government directly determines the nature of <u>academic</u> research projects in : -	the majority of cases	1	32
	a sizeable minority of cases	2	
	a small number of special cases	3	
	in no cases at all	4	

(ring one)

(Questionnaire p. 7, continued)

The present degree of influence that industry and government have over the choice of <u>academic</u> research projects is : -	very unfortunate	1	33
	unfortunate in some cases	2	
	irrelevant	3	
	beneficial in some cases	4	
	very beneficial	5	
		(ring one)	

Any attempts made by industry or government to extend their influence over the choice of <u>academic</u> research projects should be : -	resisted on principle	1	34
	resisted in some cases	2	
	viewed with indifference	3	
	encouraged in some cases	4	
	encouraged on principle	5	
		(ring one)	

Please indicate your opinion on the following matters by considering the statements and ringing a code opposite each as follows : -

1 = agree completely; 2 = agree with reservations; 3 = feel neutral or don't know; 4 = disagree with reservations; 5 = disagree completely.

It is a valuable attribute of a scientist to be able to write light, readable accounts of his scientific work for popular journals.	1	2	3	4	5	35
There are plenty of opportunities in industry for scientists to do research that is of significant <u>scientific</u> interest.	1	2	3	4	5	36
Many more of the problems of modern society could be solved than at present, if scientists were given more influence in government and administration.	1	2	3	4	5	37
The social consequences of scientific research are not the responsibility of the scientist.	1	2	3	4	5	38
However distinguished, intelligent and practical scientists may be, they cannot be so well qualified to decide what the needs of the nation are, and their priorities, as those responsible for ensuring that those needs are met.	1	2	3	4	5	39

(Questionnaire p. 8)

These questions concern the recent Rothschild Report.

With regard to the Rothschild Report (A Framework for Government Research and Development), have you : -	heard of it	1	40
	taken any interest in it	2	
	read it	3	
	(ring all that apply)		

Has your supervisor(s) mentioned or shown any interest in the Rothschild Report ?	Yes	1	41
	No	2	
	(ring one)		

Has the Rothschild Report been discussed in your department ?	a great deal	1	42
	to some extent	2	
	not at all	3	
	(ring one)		

If the recommendations of the Rothschild Report were implemented, do you think the following people would judge the consequences for science as likely to be : -

	very harmful	harmful in some cases	irrelevant	beneficial in some cases	very beneficial	don't know	
Yourself	1	2	3	4	5	6	43
Your supervisor	1	2	3	4	5	6	44
Your second supervisor (if applicable)	1	2	3	4	5	6	45
Most people in your department*	1	2	3	4	5	6	46
Scientists in general	1	2	3	4	5	6	47

*If you think opinion in your department is split on this issue, please tick here: - ______ 48

(Questionnaire p. 8, continued)

Any other comments on the Rothschild Report ? 49

(Questionnaire p. 9)

SECTION FOUR

This section is concerned with your career plans.

Do you yet know definitely what you will do when you have finished your Ph.D ? If so, please specify : - 50

51

If you have not yet arranged a specific job, what occupations are you considering ? 52

53

Suppose that the job market were ideal and that you got your Ph.D: what occupation would you most like to have ? 54

55

What do you find either appealing or off-putting about your chosen occupation, or what makes you hesitate between alternatives ? 56

57

(Questionnaire p. 10)

Please could you now indicate what general features you look for in a career by considering the factors below and ringing a code opposite each as follows : -

1 = necessary; 2 = very important; 3 = marginally important;
4 = irrelevant; 5 = a positive disincentive.

Good promotion prospects.	1	2	3	4	5	58
Interesting scientific work.	1	2	3	4	5	59
Freedom to decide which projects you will work on.	1	2	3	4	5	60
Work which involves challenging problems irrespective of their relevance to 'pure' science.	1	2	3	4	5	61
Opportunities to work with people.	1	2	3	4	5	62
Freedom to publish your research work.	1	2	3	4	5	63
Work involving interesting technical problems.	1	2	3	4	5	64
Good salary.	1	2	3	4	5	65
Work which fully uses the skills and knowledge you have acquired doing a Ph.D.	1	2	3	4	5	66
Opportunities to consult and contact other members of your profession.	1	2	3	4	5	67
Good long term security.	1	2	3	4	5	68
Work useful to the rest of society.	1	2	3	4	5	69
Freedom to do the work in the way you think best.	1	2	3	4	5	70
Opportunities to take responsibility.	1	2	3	4	5	71
Work suited to your particular talents and abilities.	1	2	3	4	5	72
Chances of making a contribution to science.	1	2	3	4	5	73
Opportunity to make a good career without moving out of science.	1	2	3	4	5	74
Any other factors ?						75

(Questionnaire p. 11)

SECTION FIVE

In this final section, it would be very useful if you could give us a few brief details about your background.

Are you : -	single	1	76
	married with 0	2	
	1	3	
	2	4	
	3	5	
	4 or more children	6	
		(ring one)	

What is (was) your father's occupation ? (Please give as full details as possible) 77

What sort of education did he have ? If you know, please specify: -

Type of secondary school he attended : - 78

Age he left school : -

Higher education (if any): -

Qualifications (if any): -

Finally, as mentioned in the introductory letter, we are hoping to follow you up later into the jobs you take. For these purposes, we should be most grateful if you could give us a permanent address where it would be possible to contact you next year (e.g. your parents' address if you don't know where you will be living).

3. Request for job information and reply form

May, 1973.

Dear

You may remember filling in a questionnaire last year concerning your attitudes to jobs and your Ph.D. training. Thank you very much indeed for filling it in and returning it to me. I hope you haven't taken the absence of any previous acknowledgement as a sign of my ingratitude. May I assure you that the trouble you took in answering my questions was very much appreciated: the analysis of the survey is now well under way.

As I mentioned in my original letter, I am hoping to follow some of you into the jobs you have taken to see what experiences you are having and what your problems and prospects are. For the purposes of this second stage of the study, I should be most grateful if you could let me know whether you have completed your Ph.D. and if so, what sort of job (if any) you are doing. I enclose a reply form and S.A.E. to make the task as little trouble as possible. Incidentally, please note that I have changed my address from Kent to Edinburgh University.

Finally, may I assure you that, as with the information you gave me in your questionnaire, your replies will be treated with the strictest confidence.

I should be very grateful indeed if you do have time to send me the few particulars detailed on the form and look forward to receiving your reply soon.

Yours sincerely,

Celia Merrick.

SURVEY OF PH.D. SCIENTISTS

1. Have you completed your Ph.D. ?

____

2. Have you taken a job ?

____

If YES, please give the following details : -

3. Job title or a brief description of your work:

____

4. Name of your employer or the type of firm or institution in which you work:

____

5. Date of taking up your present job:

____

6. Address to which any future correspondence should be sent (if different from the one I have).

____

4. Second stage interview check list

JOB formal title and brief description of what it involves.

JOB CHOICE how did you get this job ? How many jobs did you apply for ? What sort were they ? How many did you get offered ? Is this the sort of job you wanted ? If so, ideally, or merely the best in the circumstances ? If not, what sort of job would you have preferred ? What do you think stopped you getting that sort of job ?

PRESENT JOB have you had any formal or informal training ? Details of what job involves — i.e. how work is obtained; how it is done; how completed — is it published, patented or what ? If published, how many ? What feedback do you get ? or recognition in terms of formal or informal comment, salary increments etc. ? Are you satisfied with the amount and nature of the recognition ? Are you content with your salary ? Have you always worked like this ? — brief outline of job from beginning. Did you, for instance, begin at the beginning or middle of a project ? How long do projects last ? Is this satisfactory ? What would you prefer ?

SURROUNDINGS office ? lab ? Moving around departments ? Which ? Do you travel at all ? Where ?

PEOPLE whom do you work with ? What sort of people are they — ages, qualifications, experience ? Authority relations ? Where do they work ? Frequency of contact ? Do you feel isolated or oversupervised ? Satisfied/dissatisfied ? i.e. do you get on alright with them — accepted, marginal or rejected ? Are there any struggles for authority or influence ? Are there any difficulties in overcoming them ? Any group formation ? On what basis ? Do you see things in the same way as these other people, e.g. the nature of tasks, research and company objectives ? Details of similarities and differences. Are you aware of any group, department or company ideology or ethos ? What do you feel about it ?

EQUIPMENT What are you using,if any ? What methods ? How do these connect with the work you were doing for your Ph.D. — continuities and discontinuities ? Satisfactions/dissatisfactions ? Are you making use of your Ph.D. training ? Which aspects of it do you value most ? Are your skills being fully utilised ? Details of satisfactions and dissatisfactions.

SATISFACTION/DISSATISFACTION Insofar as dissatisfied, why ? What in your view would be important for satisfaction ? What strategies, if any, are you adopting to overcome these problems ?
Insofar as satisfied with work, can you say what it is exactly that you find satisfying ? e.g. pure science, problem solving etc. ?

Have you always felt like this, or have your feelings changed ? When ? How ? What do you relate this to ? Time course of interest in science, detailing exam performances, changes of mind about jobs etc. Consistency of academic success — fluctuations ? At various choice points, have you ever considered anything other than academics ? Why ? Why not ? Would you ever be prepeared to move out of science ? Details. Degree of satisfaction with job generally ? cf. other jobs in industry and outside industry.

FREEDOM Do you have enough freedom in your job ? For instance, do you want freedom : —

to choose what projects you will work on ?

to work in the way you think best ?

to publish or otherwise communicate your findings ?

In what spirit are these views held — moral vs. instrumental ? What do other scientists think about these matters ? — in your firm ? generally ? Do you have or anticipate any problems with these freedoms or with : scientifically uninteresting work; foreclosure of projects; pressure for results; unwillingness of management to allow problems to be pursued to a satisfactory scientific conclusion ? Are you satisfied with your company's policies in these respects ? If any problems, have you made any attempts to resolve them ? What ? Have you always felt like this ? If not, can you relate your changes of mind to anything ?

Has your job turned out as you expected ? If not, in what respects is it different ? Satisfactions/dissatisfactions ? What changes, if any, would improve it ?

What is your view (if any) of your likely future career ?

Are you conscious of any changes in your viewpoint on science/industry since taking this job ?

Anything else ?

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