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Knowing everything about nothing

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Specialization and change in scientific careers

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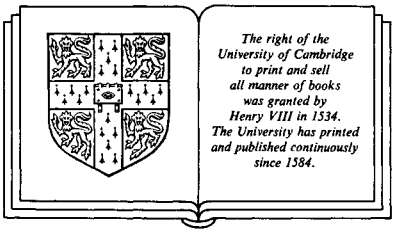

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'A philosopher *is a person who knows less and less about more and more, until he knows nothing about everything.*

A scientist *is a person who knows more and more about less and less, until he knows everything about nothing.'*

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Introduction

In the autumn of 1980, the late Professor Frank Bradbury, Coordinator for the Joint Committee of the Science Research Council and the Social Science Research Council, introduced me to Mr (now Professor) R.J.H.Beverton FRS. Ray Beverton had recently retired from 15 years as Secretary and Chief Executive of the Natural Environmental Research Council where he had had to grapple with the following managerial problem: For reasons of government policy, certain research establishments had had to be radically re-organized and re-oriented towards new problems. But a number of members of the permanent staff of these establishments strongly resisted any attempt to move them to different jobs within the Research Council. This was understandable where it involved moving house to a distant part of the country. But many of them were also resisting any substantial change in the subject of their research, even when this lay clearly within their capabilities.

This was puzzling. Scientists are supposed to be go-ahead people, who welcome novelty. Why should they object to doing research in a field which they had not worked in previously, but for which they seemed well qualified by their training and experience? What was it about their work, or their professional careers, or the way they were employed, that made them so rigidly specialized and resistant to change? Was there a stage in their careers when they became so unadaptable that it was scarcely worth the managerial effort to redeploy them on new problems when they were no longer needed in their previous work?

This issue intrigued me. I had always been interested in the personal aspects of the scientific life – the way in which scientists are educated, trained to do research, drawn into the scientific community, and eventually win recognition for their achievements. As far back as 1960 I had written a piece on the emergence of research as a profession (Ziman 1981), and in later years had even made two separate but abortive attempts to write a general book on science as a career. Ray was asking a practical question that touched upon this subject at every point. Other senior research managers were also very concerned about this matter, and in May 1981 (aided by Mr Harold Palmer, who had taken over as Coordinator for the Joint

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Committee) we were delighted to get a one-year grant from the SRC to start work on it together.

In a sense, it was a very familiar problem – yet surprisingly unexplored. The high degree of specialization in modern science is notorious. Sociologists of science have made extensive studies of research specialties – yet they have done almost nothing on scientists as specialists. Everybody knows that scientists become very attached to their subjects, and are often very reluctant to take up problems in other fields – yet there is very little in the literature on research management on how to deal with the personal problems that arise when they get into a rut, or their work gets out of date, or their services are needed for new research programmes.

After a brief library search, we came to the conclusion that this was not one of those practical issues on which there already existed a coherent body of applicable theory. In the past five years, I have had the pleasure and profit of discussing this issue with Lotte Bailyn (MIT), Barry Barnes (Edinburgh), Stewart Blume (Amsterdam), Daryl Chubin (Georgia Tech.), Thomas Gieryn (Indiana), Gerard Lemaine (Paris), Dorothy Griffiths (London), Tom Kitwood (Bradford), Nigel Nicholson (Sheffield), Terry Shinn (Paris) and other social scientists whose published research had contributed significantly to my understanding of various aspects of the central problem, and they all confirmed that it had not previously been studied systematically.

Since social theory was not very helpful, we turned to social practice. Through the summer of 1981, we discussed the whole matter informally and confidentially with a number of heads of research councils, departmental chief scientists, directors of research laboratories, *et al*. These interviews were taped for our own convenience of retrieval, but were not transcribed, and are not quoted verbatim in this book. Since then, I have had similar talks on the same topics with very senior scientists in the United States, France and Holland. I will not list our informants by name, for they might not now hold anything like the same opinions, nor wish to be associated with my present conclusions. Nevertheless, I am extremely grateful for the time and thoughtful attention they gave us.

These discussions with 'the great and the good' certainly opened up the subject for us very effectively. From them, we could sketch out and interrelate its salient features, and determine the lines along which our further enquiries might proceed. We began to appreciate some of the issues that really needed to be clarified, such as the difference between 'versatility' and 'adaptability', the influence of promotion procedures on specialization, the role of the 'scientific generalist' and so on.

Nevertheless, for all their wisdom and experience, these eminent and influential scientists were no longer in a position to see the problems of career change from

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the viewpoint of a person of more modest talent or standing. In many cases, they owed their present authority precisely to having made such changes successfully several times in their own careers, and found it difficult to appreciate the unease and fear of failure of someone faced with this possibility for the first time.

To find out what ordinary working scientists thought about the whole subject, we arranged a series of small informal meetings at various research establishments. At each of these meetings, we put a tape recorder on the table, and started off a discussion ranging over all the issues that we had identified. This book is based primarily on these group discussions. Every phrase, sentence, or paragraph (whether or not 'displayed' in the text) that appears in quotation marks without specific attribution comes from this source.

The context of each discussion was very straightforward. From September 1981 to April 1982, we visited 15 research establishments, of which seven were run by research councils, five were in the public sector, two were in private industry, and one was part of an academic institution. Each visit was arranged for us by the director of the establishment, who was carefully advised of the purpose of the meeting. A group (in three cases, two separate groups) of about eight members of the permanent research staff of the establishment would be introduced to us, with the clear understanding that this was not a managerial investigation, and that nothing they said would be reported back or could ever be attributed to them in the final report of the research. Almost all the discussions were, in fact, very free and easy, and apparently uninhibited, with frequent references to the failures and follies of their superiors. This was corroborated by the noticeable change in tone on the only occasion when a senior manager of the establishment took part in the discussions – and then withdrew.

We had no detailed control of the choice of participants. Sometimes they were all members of a single research group, and knew each other well; sometimes they came from different research groups within a large establishment and only knew each other by sight. Although we felt that they were reasonably typical of the communities from which they were drawn, they could not be supposed to be statistically representative of that population. Since participation was voluntary, it is possible that these were amongst the more successful, more articulate and less narrowly specialized members of the staff of the establishment. They ranged in age from the late 20s to the middle 50s, but the median age was 39, and more than one third of them were between 36 and 40. In the terminology of the scientific civil service, they varied in rank from HSO to SPSO (see §3.3), but about half of them were in the 'career grade' of PSO. Most of the participants could thus be described quite fairly as being 'in mid-career': they were already solidly established professionally, but could still see themselves as working hard at their jobs for many more years.

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The discussions were as open-ended and unforced as we could make them. Each participant had been given a two-page account of the purpose of the project, which was explained again, informally, at the beginning of the session. Then, to start things off, we would ask one of the participants to tell us about his or her past career – what courses they did at university, what jobs they had then taken, how they had come into the establishment, what projects they had worked on, and so on. This would soon bring up one of the topics in which we were interested, which could then be turned over to the group as a whole for general discussion. As a matter of courtesy, we would ensure that each member of the group had had an opportunity to say something about their own career: in fact, since they often did not know each other well, this was of interest to everybody else!

By the time we had gone round the whole group in this way, we would have been offered opinions on most of the topics on our private check list without having had to pose them as explicit questions. Sometimes, of course, the conversation would flag, and we would have to prime it with an outright question, or probe for a positive opinion. Generally speaking, however, they did not need much prompting, and a couple of hours would pass in quite lively discussion, approximating in tone to what such a group might say amongst themselves over coffee or lunch. Because both Ray and I were known to be professional scientists – albeit older and of higher status – we could join in this conversation in a natural way. At the end, when we thanked them for their co-operation, they would often say how interesting it had been to talk about these matters, and how much they had enjoyed it.

The record of these discussions occupies some 50 hours of tape. It constitutes a raw sample of characteristic public discourse on a matter of personal concern to each of the participants. One may assume that it contains references to most of the considerations that members of the group deem relevant, conceptualized and formulated in the manner that they deem intelligible and acceptable to other members. In other words, it samples the common experiences and commonly held notions of people of that kind, in so far as they are willing to reveal them to their acquaintances.

These tapes have obvious limitations as a basis for a systematic analysis of the problem. Because the participants in the discussions were not a statistically representative sample, no quantitative indicators can be derived from them. Because the questions that we had in mind were not directly posed as such to the participants, the answers that we got are often contradictory, or inconclusive, and cannot be structured into logically coherent sets. And because each discussant spoke in the presence of colleagues or acquaintances, he or she must have concealed many private hopes or fears which might have been confessed in confidence to a sympathetic interviewer.

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It is doubtful, however, whether a different research methodology would have produced more useful data at this stage of the investigation. It would not have been possible to tap a statistically representative sample of the relevant population except through some impersonal written communication which could be dealt with quickly by relatively uninterested informants. A thorough survey by questionnaire would have given quantitative answers to specific factual questions, such as the prevalence of career changes of various types, but only at the expense of imposing a preconceived categorical framework on more subtle aspects of the matter. Individual interviews with selected informants would have probed deeper, but would have sampled much less widely in the time available, and we doubted whether we had the professional skill to carry out such interviews without projecting our own viewpoint on our informants. These and many other considerations are, of course, familiar methodological issues in the social sciences.

Our original plan, in fact, was to use the group discussions as a means of exploring in detail a very complex landscape which had never been mapped from this point of view. We hoped at first to compare the attitudes and experiences of British scientists with their contemporaries in other countries, and I did have the opportunity, whilst visiting the United States on other business, to record similar group discussions in two major industrial research laboratories. We then intended to use other methods, such as questionnaires and interviews, to study further any critical points that might emerge. It soon became obvious, for example, that the procedures for Individual Merit Promotion (§7.6) play a very important part in career patterns in the Scientific Civil Service and the research councils, and ought to be looked at much more closely. We even promised some of our discussion groups that we would come back to them and try out our preliminary conclusions on them, to correct our impressions and obtain further insights.

This promise could not be kept. Perhaps our plan was too ambitious. In any case, circumstances prevented us from carrying it out. At the end of April 1982, Ray Beverton took up an important international assignment which prevented his further regular participation in the project, and I decided not to apply for a continuation of the SRC grant, at least until I had made a systematic analysis of the existing raw data. As the date of this publication indicates, this took much longer than I expected. Perhaps a year of this delay was due to the unanticipated demands of other work, but the task I had set myself was surprisingly laborious and the present text is substantially longer than I originally thought likely.

The first step was to have the tapes transcribed for detailed scrutiny. For this I am extremely grateful to Lilian Murphy and Felicity Hanley, of the H. H. Wills Physics Laboratory of the University of Bristol, where I then held an appointment. It was a lengthy job, which had to be fitted in amongst their other duties, and they often had to cope with extraneous noises, poor diction, and people talking

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simultaneously. Nevertheless, within six months they had produced a complete text of all the discussions, amounting to the equivalent of about 750 pages of single-spaced typescript.

The next step was to go through the transcripts, marking relevant passages and indexing them according to a preliminary scheme based upon the interim report that we had already prepared for the Science Research Council. This index was then analysed and restructured until it began to look like a tentative synopsis for the present book. Finally, as I came to each section, I retrieved the relevant passages from the file of transcripts, selected parts of them for quotation, and linked them together into a coherent text.

This book consists in large part, therefore, of the actual words of working scientists about their careers. But I have not followed current sociological fashion by treating the verbatim text as if it were sacrosanct. For brevity, the quotations have been pruned of minor irrelevancies, repetitions, and vacuous phrases such as 'I think', 'I mean', etc. To preserve the anonymity of our informants, all personal names have been excised, and words that might identify a particular establishment or scientific specialty have been generalized. For clarity, the grammatical hiccups of impromptu speech have been tidied up in accordance with the obvious intentions of the speaker, or gaps filled with appropriate link words. All such changes from the transcribed text are indicated by ellipses . . . or by brackets []. In my opinion, the information thus lost is insignificant by comparison with the unavoidable effects of background noise and ambiguities of transcription, let alone the elimination of information about the speaker and the context of the conversation from which the particular quotation had been selected. But when in doubt whether or not to quote a particular passage, I would usually decide to include it, if only to indicate how diverse and contradictory people's attitudes can be.

Perhaps the most serious defect of this material is that it is now five years out of date. The project was triggered off by the general feeling that British science was going through a grave crisis of confidence. As it turns out, what seemed like a sorry state then has got worse. Many threatened cuts and closures have proved even more severe than was then feared, some establishments have been completely reorganized, and most research programmes have been radically reorientated. As a consequence, the public morale of the British scientific community is much lower now than it was even five years ago. But that does not mean that scientists as individuals have not been able to cope with these rapid changes in their situation. As this study shows, they are usually more versatile than they tend to believe, and adversity may even have strengthened them professionally by forcing them to become more flexible and adaptable. Nevertheless, it would be immensely instructive to go back again to these establishments, as we half promised, and look

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for changes in attitudes towards change itself.

The overall structure of the book is as follows: Chapter one is about scientists as subject specialists. Chapter two is a schematic account of the organizations that employ scientists to do research. Chapter three deals broadly with the careers of scientists in organizations that are now undergoing rapid change. In Chapters four and five I discuss the practicalities of a personal change of research specialty and the motives that a scientist might have in resisting or welcoming such a change. Chapter six is concerned with career changes *out* of research, into management, administration, etc. Chapter seven then goes through various organizational policies and practices which seem to have some effect on the versatility and adaptability of scientists, and Chapter eight suggests a number of practical steps that can be taken to help each individual through periods of career change. In the final chapter I have tried to set the earlier conclusions into a wider setting of national science policy, of international comparisons, and of the changing role of the 'scientist' in modern society. I suppose it all adds up, in the end, to asserting that scientists are really much more versatile and adaptable than they or other people tend to think, and it is to everybody's advantage to give them the time, the opportunity, and the sympathetic leadership to face the challenges of radical change. But there can be no better way of convincing you of this than by inviting you to read at length the evidence presented on the following pages.

In thus trying to account for the genesis and structure of this book, I have already mentioned by name a number of people to whom I am particularly indebted for advice and assistance. To these should be added the 100 or so working scientists who took part so enthusiastically in our discussions, and the directors and administrative staffs of some 15 research establishments who arranged these meetings for us. I am grateful to Nicola Kingsley for the preparation of this index. And one of the real benefits of the whole project was spending many days in the company of Ray Beverton, driving up and down the country, meeting people together, and discussing every aspect of the scientific life to which both of us had been so long committed.

Imperial College
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