UNIVERSITY of BRISTOL

Department of Physics

THE HISTORY OF THE DEPARTMENT OF PHYSICS IN BRISTOL: 1948 to1988

N. Thompson

H.H. Wills Physics Laboratory
Royal Fort
Tyndall Avenue
Bristol BS8 1TL

THE HISTORY OF THE DEPARTMENT OF PHYSICS IN BRISTOL: 1948 to1988

N. Thompson

October 1992

CONTENTS

CHAPTER 1

INTRODUCTION

In 1956 Professor A.M. Tyndall, who had retired as Head of the Physics Department in 1948, wrote a "History of the Department of Physics in Bristol, 1876 - 1948, with Personal Reminiscences." This was not published, but one or two typescript copies are still in existence, including one in the University library. His final sentence is, "But the full history of the department since 1948 is a subject for others to write at some future date." Since I was a member of the Department from 1933 to 1975, continuously except for four years during the war, I feel bound to admit the force of the suggestion, made by some of my friends, that I should contribute the next chapter.

The original idea was that my story should start where Tyndall left off. As it has turned out, a good deal of space has been devoted to earlier events. There are several reasons for this: (1) Some topics which seem to me to be important, or at least interesting, were not discussed at all by Tyndall. For example, he said nothing about either undergraduate or graduate teaching, and concerned himself mainly with research and the people who carried it out. (2) A recapitulation of some of the earlier material has been given here and there to provide an appropriate background for some of the later developments. (3) There are some matters which Tyndall may have thought inappropriate for inclusion when he wrote, but which have now passed into history, and can be set down. (4) This second instalment is different in character from the first. As indicated in his title, his was a personal account, with fascinating reminiscences; mine is more of a history (although here and there I have found it convenient to write in the first person singular). In view of our different relationships to the department, this is inevitable. The Physics Department was Tyndall's own creation, his life's work. I arrived as a very junior member of staff when it was already well established and saw things from a different point of view.

When I embarked on the project my intention was to follow Tyndall's example and stop at the date on which I retired. However, the process of writing has been spread over more than a dozen years, and in some places I have been unable to resist the temptation to bring the account more up to date. Unfortunately I have not been sufficiently diligent to make all sections terminate at the same point. As a second best, wherever the phrase "at the time of writing" occurs, the date is given. In particular, any section dealing with statistical data is usually as complete as I can make it, from the earliest times up to 1988, at which point I decided that enough was enough.

The result is that the final product is more a work of reference than a readable story. It includes too much detail to fit into the latter category, but I make no apology for this. It may be of interest to somebody, someday, and in any case can always be ignored. If not present it cannot properly be invented, and some of it is not to be found elsewhere. Since the whole account is a history, it contains numerous dates, but not being a proper historian I have not always taken the trouble to check that every one of these is absolutely accurate: the errors, if any, will not be large, and the spirit of the events has been preserved.

The Department has never kept any kind of official record of correspondence, or, until recently, any archives of any kind. Mrs Terry, when she was secretary to Professor Tyndall, kept a scrap book of newspaper cuttings and similar material; this is now in the main University library. It was not continued in any systematic way after she retired. Each successive Head of Department kept his own files of correspondence and, on giving up his appointment, dealt with them as he thought fit. Some would be passed on to his successor, some he would wish to keep for himself, and the rest would be destroyed. There are two exceptions. The personal files relating to the appointment and subsequent career of staff, both academic and technical, have been kept fairly continuously since the 1940's, although they are not always complete. These still exist. Secondly, the record cards of individual students have been maintained continuously. However, at one stage, pressure on the space available in the filing cabinet in use at the time led to the extraction of some of the earlier cards. These were placed in the departmental stores, and at a later date were found to have vanished. I suspect that the explanation lay in the enterprise and enthusiasm for tidiness of an assistant storekeeper in post at the time, but this has never been established with certainty. It is just conceivable, although very unlikely, that they were not destroyed, and may still turn up. During the last few years before my own retirement I made a practice of collecting any archival material which came to my notice. The result is a somewhat random collection of papers some of which refer to the time before 1948, and so to the period that is covered by Tyndall's History. Much of what follows is based on this material, supplemented by my own imperfect memory.

When, in the course of writing this account, it became clear that the records were so fragmentary, I did what I could to remedy what I considered to be a defect. Since 1981/82, our indefatigable departmental secretary, Felicity Hanley, and her successor Lilian Murphy, has collected and filed, each year, a number of notices, lists, time tables, exam papers and the like, sufficient to provide the bare bones of a continuing record. When I discovered that nowhere in the Department, or in the University, is there a complete set of past examination papers, I collected as many as I could, and put them in the library. The Examinations Office keeps copies for a few years and then destroys them. In the same way, the Registrar's Office from time to time goes through its accumulated records about individual members of staff and throws the old ones away. Fortunately, minute books of the more important committees are carefully preserved in the Registrar's department.

One consequence of this state of affairs is that this present account is somewhat unbalanced. Some periods, and some incidents which happen to be well documented, are recorded in detail, while others, possibly equally important, are hardly mentioned. It has become apparent, for example, that several annual lists of this and that, which are available for most years, are missing for a few years around 1955 to 1965. This happens to be the period for which Pryce was Head of Department, but while this might account for the absence of some correspondence files, I am at a loss to see why departmental circulars should also be missing.

One other curious gap concerns our library. There is a plate on its door saying that it is the Maria Mercer Library, but I never met anybody who knew who Maria Mercer was. In 1988, while writing this account, I was put onto the right track by our librarian, Lynne Burlingham. Eventually, I found that Maria Mercer was the last surviving daughter of John Mercer (1791-1866) a humble Lancashire weaver who taught himself sufficient chemistry to be elected to a Fellowship of the Royal Society in 1852. He was the inventor of the process of treating cotton known as mercerisation, and from that, and other commercial activities, he amassed a considerable fortune. When Maria died, in 1913, aged 93, she made bequests to various charities, and left the remainder of her estate, about £100,000, to be devoted to "such charitable purposes as her trustees thought fit." One thing they did was to offer "not less than £5,000" to the University of Bristol, towards the endowment of a Chair of Chemistry. Negotiations seem to have continued in a desultory fashion for some years. The trustees blamed the delay partly on "the depression in the cotton trade" [there was also a war in progress] and appear to have tried to withdraw their original offer. The correspondence, incomplete in our archives, and not always amicable, includes a letter asking the University "to accept £1,250 in full satisfaction of their gift." It ends with the receipt by the University, in 1923, of a cheque for £1,800. Council decided to use the money to establish an endowment for a library in the Department of Physics, "which shall be known as the Maria Mercer Library". There is much in this story which remains mysterious, not the least of the problems being "Why Bristol?", and "Why did Council switch the money from chemistry to physics?". But at least we now know who Maria Mercer was. There is a photograph of her, and also a more complete account of the whole business, in our library.

Tyndall's History was, for the most part, a continuous story in chronological order. I have preferred to divide the subject matter into several separate chapters, each one of which is roughly chronological. This has two disadvantages: some topics and incidents are to be met with in more than one place, since the categorisation cannot be watertight; and some of the earlier chapters will occasionally refer to matters not properly discussed until later. In spite of these drawbacks it seemed to me that the result is more coherent and comprehensible than would be given by any other arrangement. The immediately following chapter is titled "Recapitulation" because it goes over some of the material to be found in the later parts of Tyndall's history. The remainder covers essentially new ground.

CHAPTER 2

RECAPITULATION

When the H.H. Wills Physics Laboratory was formally opened in October 1927, Professor Tyndall found himself in charge of a Physics Department which was the largest in the country outside Cambridge and Oxford. He himself has told how this came about, and while it is clear that his own enterprise, enthusiasm and diplomatic skills had led to the building and endowment of the Department of physics, it is not clear to what extent he was responsible for its size. We know that he devoted a great deal of time and energy to the details of the interior design: the exceptional acoustics of the original Large Lecture Theatre (now the Tyndall Theatre) provide one example of the results. But of his influence on the absolute size, we know nothing. The whole scheme had started from very small beginnings, as is recorded in his History. We may guess that Harry Wills, having embarked on the project, would be in favour of a large and imposing structure. And, modest as he was about his own achievements, Tyndall was not the man to refuse. About 1933, when he was trying to get more money from the Rockefeller Foundation, the representative of that body described him as "an ambitious administrator". He was well aware that the building was disproportionately large in a University whose total numbers were less than a thousand. He deliberately left the third floor free of laboratory fittings, so as not to prejudice its future use - but arranged for it to be occupied by the Mathematics department, in order to forestall others who might look longingly at the empty space from their own more crowded premises, and who would have constituted less congenial company. Even so, when I arrived in 1933, there were still one or two laboratories standing empty, and several more were under-used.

Tyndall, with characteristic prudence, had been concerned that when his new building was completed it should be adequately endowed. He had even suggested to Harry Wills that the size of the proposed building should be reduced considerably, in order to provide funds for such an endowment. In response, he had received a verbal half-promise that additional funds for endowment would probably be forthcoming. However, Wills died shortly afterwards, leaving a considerable legacy to the University as a whole, "to be devoted to buildings and endowments". Tyndall then persuaded Council to agree to a proposal that a sum not exceeding £4,000 per year should be set aside from this fund, earmarked for physics, in addition to the normal departmental grant that would have been available in any case. In addition, interest which had been accumulating on the unspent balance of the original bequest of £200,000 in 1920 had, by 1927, built up into a sum of about £95,000. Thus Tyndall was in the happy position of having not only plenty of space, but plenty of money also. If this were not enough, he had an almost free hand in spending money and making appointments - within reason - since the Vice-Chancellor "seemed to see in the finances of the Physics Department a complicated financial problem which, in my view, never existed". This happy state of affairs continued up to 1939, but after the war the situation was totally changed.

In 1924/25 the academic staff of the Physics Department numbered five, rising to ten by 1928/29. There were about a hundred undergraduate students in all (excluding the pre-medical class), and about five post-graduates. Tyndall remarks "We all had a pretty heavy teaching load..." but most found some time for research. He adds, "it was quite clear that, to become prominent in British physics, the laboratory must include on its staff some men whose main function was research", and also that it was most important that they should include a theorist of sufficient standing to attract a small group "preferably working on problems related more or less directly to the experimental work in progress". The quotations are from a paper written by Tyndall in 1956, but there is no reason to doubt that they truly reflect the opinions that he held in the 1920's. Accordingly, he arranged for the appointment, in 1925, of J.E. Lennard-Jones as a Reader in Mathematical Physics. In 1927 this was followed by the creation of two Research Fellowships, which were awarded to L.C. Jackson, from Nottingham, and H.W.B. Skinner, from Cambridge, both experimentalists. In 1927 also, Lennard-Jones' title was changed to Professor of Theoretical Physics - the first such Chair to be created in England. It may be noted that L-J's original appointment was made before the building of the new laboratory had been completed. These appointments were the first steps - and very shrewd ones - in the process of building up the reputation of the new laboratory.

A broad general policy in relation to research was established at this time, which has been maintained in essence ever since, and which has been very successful. Tyndall's account is that, following the practice of other departments with a good reputation in physics, he and L-J. looked for a topic around which the main research effort could be centred. They considered expanding the work of Lennard-Jones on the frontiers of physics and chemistry, which was already attracting wide attention. First attempts came to nothing (Tyndall's phrase, and I am not clear what it means) so "we let matters take their course by collecting some young men to assist existing members of staff" and left in abeyance any question of future specialisation. This started the Department along a path, which it has followed ever since, of maintaining a wide diversity of research interests. Apart from the dictates of necessity, incidental advantages were seen for this state of affairs. It provided an opportunity for cross-fertilization of ideas from different sections, it provided a wide background for the training of young research workers, and it provided wider publicity for the growing Department.

Although Tyndall's resources at this time were large, they were finite, and he was also unable to make staff appointments that would involve long-term commitments of University funds. His policy therefore was to make a number of short-term appointments of promising young men. Their short-term character reduced their attractiveness to British graduates starting on an academic career, but a research grant of £300 pa would, it was thought, attract young men from the continent who "would jump at the chance of even a year's experience in England". (The salary of a professor at that time was about £900 pa.) This resulted in a series of appointments of people who tended to help with the work on problems already under investigation, or else to do something which had no lasting influence on the programme of the Department after they left. Among these we must count Gerhard Herzberg and Max Delbruck, who were invited to Bristol by L-J while he was on a visit to Bonn in 1929, and also Clarence Zener, who was here from 1932 to 1934, and whose presence in Bristol is unexplained by the surviving papers.

When, as a result of this policy, the supply of money showed signs of running out, Tyndall took steps to obtain a grant from the Rockefeller Foundation. They eventually (January 1931) provided £50,000, on condition that £25,000 was raised locally. After a good deal of trouble this was obtained from another member of the Wills family - Melville. This episode is mentioned briefly in Tyndall's "History" but, in fact, a great deal of effort and pertinacity lay behind the simple statement of the outcome. A fairly detailed account of the affair has been published by S.T. Keith in "Annals of Science" vol 41,(1984), p335. A brief summary is given as an appendix to this chapter. The representatives of the Rockefeller Foundation with whom Tyndall negotiated were "clearly" not interested in the possibility of a general grant to support unspecified fields of research. So, he says, "we (i.e. Tyndall and L-J) plunged for the subject of molecular structure and the chemical borderland of physics". Thus, in effect, it was Lennard-Jones' presence and activities which were used to appeal to the Foundation. The offer from the Foundation to the University was formally accepted in February 1931. It is rather ironic that, shortly afterwards, Lennard-Jones was offered, and accepted, the newly created chair of Theoretical Chemistry at Cambridge. The appointment was to date from the 1932/33 session but he spent the latter part of 1932 dividing his time between Bristol and Cambridge. However. as Tyndall notes with satisfaction, "no obligation of any kind (as to the field of research to be supported) was imposed by the Foundation in their gift" and he therefore felt quite free to try to get the best man available as a successor to L-J, irrespective of his topic of research.

(a) Appointment of Mott as Professor.

Those "available" in those days, would probably have been restricted to people already working in a university, and probably in Cambridge. As to who was the best, Tyndall relied on his own judgement guided by information percolating along the academic grape-vine, - a wide spreading plant which seems to have had important roots in the Athenaeum Club, in London. Tyndall discussed the choice of a successor with L-J himself, and also consulted Fowler and Rutherford. He decided that N.F. Mott, then working at Cambridge, was "the best man available" and in the summer of 1932 appears to have offered him the job. The detailed story of the appointment is not given by Tyndall, and is worth recording. Mott was very reluctant to leave Cambridge and initially declined the offer (August 17th.) After further consideration he changed his mind, and decided to accept, provided that the appointment could be dated from October 1933. On Saturday,(!) October 8th. 1932, Tyndall wrote to Mott (a typed letter, so we have a copy) saying how glad he was, but that an interview by a Joint Committee of Senate and Council would be necessary in order to satisfy Regulations about the appointment of professors. There was to be a meeting of such a committee on the following Friday (14th) to transact other business (actually to appoint a Professor of Zoology) and if Mott could manage to be present he could be interviewed when the other business had been completed.

Presumably Mott agreed, because on Tuesday, (11th) Tyndall wrote to the Vice-Chancellor, recommending that Mott be invited to fill the vacancy. The letter also said that "Lord Rutherford, Prof R.H. Fowler, and Prof O.W. Richardson have expressed the view that we could not make a better choice". On the same day (11th.) the Registrar sent out letters to members of the Joint Committee, suggesting that the interview should take place after the completion of their other business. Again on the same day, Tyndall wrote to Rutherford and Fowler saying "it would help me if you would kindly send me a letter approving of our action". Their replies, in appropriately glowing terms, were sent by Fowler (12th.) and by Rutherford (13th.) and seem to have arrived in time for the meeting on the afternoon of the 14th. One must presume that Tyndall knew that they would, indeed, write in the way that he wanted. In addition to the speed of the postal service, and the absence of any mention of telephone communication, the interesting thing about the incident to those who have known the University in recent years is the informality of the whole process, and the authority of the Head of Department. Tyndall never had the slightest doubt that his recommendation would be accepted: indeed, he described the whole business as "a formality". He was able to act in this way partly because he had personal control of considerable funds, and partly because he was a senior figure in University circles, greatly respected by all his colleagues. But it also reflects an attitude more common in earlier years, in which The Professor really was Head of his Department, and more or less ran it as he thought fit.

Just before Lennard-Jones was offered the post at Cambridge, i.e. in the Spring of 1932, he had received an offer from D.S.I.R. to provide him with a research assistant, on condition that he would interest himself in the theory of metals and alloys. It appears that this move was initiated by Lindemann, who was in a position to influence the actions of two relevant D.S.I.R. committees. He was concerned that the British metallurgical industry was not receiving much support from the physics community, whose interests were strongly biased towards atomic and nuclear physics. Tyndall's History remarks "This was clearly an offer with potentialities for the future which it was important to accept". Again, although this was written in 1956, with the benefit of hindsight, the words probably give a true impression of his feelings at the time. The offer was accepted, and resulted in the appointment of Harry Jones, a young theoretician from Cambridge, as a Research Assistant paid from D.S.I.R. funds. He became a lecturer in 1933/34. He had arrived only a few months before Lennard-Jones left, but did not move to Cambridge with his nominal supervisor. By the time Mott arrived, about a year later, he was fully engrossed in the electron theory of metals. He was rather worried by the turn that events had taken, and was considering looking for a job elsewhere. He was therefore greatly relieved when Mott became interested in his work, and joined him with enthusiasm. Mott's previous work at Cambridge had been on the application of quantum mechanics to collision phenomena involving isolated atoms, and he said that he was profoundly ignorant of solid state physics. Nevertheless, he decided to switch his field of interest - a decision that was to have the most important consequences for the fortunes of the Bristol Physics Department. It says a lot about the versatility of theoreticians in general, and the high ability of Mott in particular that he was publishing work in his new field within six months, and that the famous book, by Mott and Jones appeared within three years.

When Mott arrived, he found five other lines of research in progress, in addition to that of Jones:

(1) Tyndall's own researches into the mobility of gaseous ions, for which he had as an assistant C.F. Powell, coming to the end of a research grant from D.S.I.R.
(2) W. Sucksmith and H. Potter, doing experiments on magnetism. These had started when Chattock had been persuaded by Tyndall to return from an early retirement in 1919, and had been continued by L.F. Bates(1920-22). Sucksmith had arrived in 1921 and Potter in 1924.
(3) S.H. Piper, using X-rays to investigate the crystal structure of long-chain paraffins and related organic compounds.
(4) L.C. Jackson, working on paramagnetic susceptibilities at low temperatures, following a period at Leyden which had generated a life-long interest in low-temperature work.
(5) H.W.B. Skinner, whose interests had moved from the spectroscopy of the radiation emitted when free atoms were subjected to electron bombardment ("excitation potentials"), to the radiation emitted by solids when similarly bombarded. Low atomic weight elements were used, for simplicity, and the observations were made in the region of soft X-rays.

With the exception of Powell, all of these people continued in the same or closely related fields of enquiry up to the outbreak of war in 1939. The magnetic work of Sucksmith and Potter received an added stimulus from close contact with the theoreticians interested in their results. This was even more marked in the case of

Skinner, whose experimental work on the emission and absorption of soft X-rays became very closely linked with the theoretical work of Mott and Jones on the electronic band structure of metals. It was, indeed, a case of mutual catalysis. Both investigations were to give results of major importance, and it is likely that neither would have progressed so far or so fast had it not been for the frequent interchange of ideas between theoreticians and experimentalists working in the same building.

(b) The Beginnings of Cosmic Ray Research.

About this time there was a growing interest among physicists everywhere in the problems of nuclear physics. Chadwick's discovery of the neutron in 1932, and Anderson's discovery of the positron in the same year, were confirmed in 1933 by the work of Blackett and Occhialini. Tyndall recalls "informal discussions" among the senior staff as to whether the Department ought to become involved in this kind of work. This must have been the first of the very rare occasions on which something approaching a collective decision was taken by staff already in post about the possibility of embarking on a new line of research. The normal process was for new work to be a development of work already in progress, stimulated by the initiative of the research worker involved. He dates it as "about 1935" but in fact it began before this. In February of 1933 he had written to Mott (who had been appointed but who had not yet arrived in Bristol) a letter in which he said, inter alia," Powell and Webster are dead keen on starting on a Blackett cosmic ray experiment." Powell had previously had experience of cloud-chamber work in Cambridge with C.T.R. Wilson, and Webster knew about counter techniques. Mott replied that he would be "overjoyed if you started some cosmic ray work", and later, in May, said that he was "reading up cosmic rays". At that time Skinner was in America on a Rockefeller Fellowship, working with O'Bryan in MIT on soft X-rays. As a result we have a very illuminating series of letters that he wrote to Tyndall - but not, unfortunately, the replies. (Typed letters were still far from common for ordinary correspondence, and the labour of making a manuscript copy was too much, except for very important communications. Tyndall did use carbon paper sometimes.) On February 26th. 1933, Skinner wrote, very excited about the recent discovery of the positive electron. The letter predicts the existence of positron annihilation, and of pair-production, and adds, "the day of atomic physics is as dead as the day of classical physics was in 1900" a typical piece of Skinner hyperbole!. More to our present purpose, he says that he is "pleased to hear that the lab. is showing signs of changing over to meet the new crisis". He goes on to discuss in some detail the possible sources of high-energy particles. Radio-activity is dismissed out of hand. Cosmic rays are also dismissed because such work would involve the use of expansion chambers and big magnets, like Blackett, and this expenditure would not be justified. He therefore advocates an accelerator to produce fast protons and, in particular, a Van de Graaff machine working in vacuo, the design of which is discussed in some detail. (Van de Graaff was also working at MIT at the time.) A second letter, to Powell, dated March 1st. goes into even greater detail. On April 18th. he discusses plans for his own work on returning to Bristol: "I am itching to get at the nucleus." On May 16th., he is "pleased to hear that Tyndall is "willing to gamble on nuclear physics", and is talking about sending drawings of the Van de Graaff vacuum tank, so that it could be copied at Bristol. However, by June 18th., his enthusiasm is cooling. His work on soft X-rays was producing interesting results, and was occupying all his time. Indeed, on his return to Bristol in the Autumn - at the same time as Mott's arrival - he pressed on with the X-ray work with great success, and continued in the same field until interrupted by the war in 1939.

But the interest in high voltage generators did not lapse altogether. Harper tried to build what I think was a Van de Graaff type, operating at high pressure. A massive bronze casting, about the shape and size of a rather small lavatory pan, occupied the attention of some of the workshop staff for quite a long time, but I do not think that it ever worked. (My arrival in Bristol coincided with Skinner's return, and I can remember seeing this and wondering what it was.) Powell, on the other hand was successful in building a Cockcroft type generator to give 700kV. In 1933/34 he was still working on the emission of positive ions from the surface of clean and oxidised tungsten. This was an offshoot from his previous work with Tyndall, in which a similar arrangement had been used to produce the ions for the mobility measurements. But by June 1937, the H.T. set was sufficiently nearly operational to give rise to correspondence on requirements for safety precautions. This had been done in spite of the fact that Powell had spent four months in 1936 in Montserrat, with a Royal Society expedition investigating earthquakes, and six months on his return on working out the results. So far as I am aware, no written account of the H.T. installation exists, and the only thing of which I can be sure from memory is that the big vacuum chambers consisted of large glazed porcelain insulators, about a meter tall and 25 cm. in diameter, as used in the transformer stations of the Electricity Board. There is also in existence one photograph of some of the people involved in its construction, in which part of the installation can be seen in the background. It was erected on the fourth floor of the laboratory, this being the only room with sufficient head-clearance. (This, of course, was before the single large room, invariably known as the chapel on account of its mock-gothic windows, was sub-divided by the extra floor, and all the partitions.) The usual troubles with faulty water-cooling systems to the (glass) vacuum pumps caused a great deal of trouble in the library immediately below. Experiments using the energetic protons which it produced were to be done using an automatic cloud-chamber, of which Powell had had some earlier experience. This was not completely built by October 1937, when a letter appeared in Nature from Blau and Wambacher giving the results of observations on tracks caused in a photographic plate by - possibly - the interaction of a cosmic ray particle with an atom in the emulsion. Powell immediately tried the technique with the fast protons from his H.T. set, and was so impressed by the results that, as Tyndall says, the cloud chamber was never completed. A great deal of work remained to be done to develop the photographic method into a reliable quantitative technique, and perhaps even more work to convince the sceptics that this had been done. Powell published half a dozen letters in Nature giving preliminary accounts of observations with the photographic emulsion technique, using both cosmic radiation and particle accelerators as the source of the energetic particles, but a full account of the work, delayed by the war years, did not appear (in P.R.S.) until 1942-44. The subsequent history of these experiments is given later.

(c) Other pre-war Research.

The period 1933-39 also saw developments in other fields of research in the Department. The influx of German scientists, refugees from the Hitler regime, had a considerable impact. Walter Heitler, Herbert Fröhlich and Heinz London were the most distinguished names, and the more junior people included Kurt Hoselitz. They were financed in a variety of ways at various times. Heitler, already with an international reputation, held a Fellowship from the Academic Assistance Council. After a little time, Fröhlich was given an appointment on the University staff. Hoselitz was paid out of a grant from an industrial organisation. All three of them were sent to internment camps in June 1940 as "enemy aliens" in spite of vigorous protests from Tyndall. They were released later in the same year, piecemeal, in what appeared to be inverse order of seniority, but were never allowed to make any contribution to the preliminary work which led to the development of atomic energy. However, there exists the typescript of an unpublished paper by Heitler and Fröhlich with the title "Chain Reactions in Uranium". It bears no date, but can be dated approximately on internal evidence. In June 1939 Peierls' paper discussing the critical mass needed to give rise to a divergent chain reaction in uranium with neutrons had been published. Heitler and Fröhlich refer to this. In a letter dated 24th. June 1940 Tyndall mentions the Heitler-Fröhlich paper. It must therefore have been written between June 1939 and June 1940. The paper assumes the existence of a sufficiently large mass of uranium and attempts to calculate the minimum concentration of the 235 isotope necessary to give a divergent reaction. The absence of numerical data prevented any definite conclusions from being drawn. Fröhlich later told me (in 1987) that they gave up this type of research when the secrecy restrictions were introduced (1940).

Gurney arrived in Bristol from Manchester in 1933. Tyndall hoped that he might do some experimental work (which he never did), and possibly also felt that his interest in atomic ions might give some support to his (Tyndall's) work on gaseous ions. This, too, never happened, but his interest in ions, both in solution and in crystals, had a seminal influence on the work in progress on solid state physics, and served to broaden it from the consideration of metals to include also ionic crystals and semi-conductors. The book "Electronic Processes in Ionic Crystals", written in collaboration with Mott, appeared in 1940. Gurney was abroad, in Sweden, at the outbreak of the war, and never returned to Bristol. He did not express any wish to do so, and since his war record might have proved embarrassing, no attempt was made to persuade him. He died in New York, at an early age, in 1953.

Burch arrived from Imperial College in 1936, having been invited by Tyndall to join the Department, for reasons that are not entirely clear. Although he had an active group of people working with him from time to time, the work never interacted much with that of the rest of the Department. However it was of sufficient interest and importance, and happens to be so well documented, that I have thought it worth while to treat it at length. (see "Applied Optics" in the chapter on Research.)

The same period saw the beginnings of a development that was to be of very great importance. I have quoted above a statement by Tyndall emphasising the importance of having the closest interaction between the work of the theoreticians and the experimentalists in the Department. Mott held the same views, and I have also mentioned how this attitude bore fruit in the case of soft X-rays (Skinner) and magnetic phenomena (Potter and Sucksmith).An extension of the policy was to seek to collaborate with industrial laboratories. This, too, was no new idea. It had been advocated in the past by Tyndall, and also by Skinner. Mott's work with Gurney on the details of the photographic process led to discussions with the research staff at Kodak on the nature of the latent image. Fröhlich spent a good deal of his time working on dielectrics, and in particular on dielectric breakdown. This was done in collaboration with the Electrical and Allied Trades Research Association, part of his salary being paid by that body. The same fund also provided support for two other refugees, Gross, working on dielectrics, and Hoselitz on magnetic materials. Further extension of the practice of obtaining funds from industry to support research in the Department had to wait until after the war, but a start had been made.

The list of research papers originating in the Department in the late 1930's shows that they numbered, on average, about thirty per year. The biggest single contributor to the total was Mott. Some of his papers were written in collaboration with Gurney or Fröhlich, but in most cases Mott was the sole author. His output was some half dozen papers per year, almost entirely on various problems relating to metals, semi-conductors or ionic crystals, but with a few on liquids. We may note that he made no attempt to build up a coherent research group of junior people working under his direction, as became the standard practice in later years. In addition to Gurney and Fröhlich, people working on research of direct interest to Mott were Harry Jones and Frank Nabarro and, for a couple of years Klaus Fuchs. The names of not more than three other associated research students appear in the publication list for the whole period.

The next most prolific author was Heitler, again usually as sole author, but sometimes with Fröhlich or some other person. Most of the titles refer to cosmic rays, but there are some others on a variety of topics. These two people account for about half of the total output. For the rest, each year usually saw the publication of at least one paper from:- students working under Tyndall on the mobility of gaseous ions; Piper on the X-ray structure of long-chain organic molecules; Potter and/or Sucksmith on problems in magnetism; Jackson on low temperature susceptibilities; Skinner on soft X-ray emission or absorption; Appleyard or one of his students on the properties of thin evaporated metallic films; and Burch, mainly on optics. It is clear that the original policy of maintaining a wide diversity of research interests had been continued.

During the war, with several members of staff away on government service, the teaching and administrative load on those that remained was greatly increased, and little research was done. For part of the time, the staff and students from King's College, evacuated from London, shared the building with the Bristolians. Some of the classes were combined, and the teaching duties were shared by the remnants of the two staffs. I myself was away on war service for the whole of the period concerned, and so have no personal knowledge of events. I have been unable to find among the papers in the Department the slightest mention, of any kind whatsoever, of the arrangements that were made, or how they were made, or even of the dates of arrival and departure of the visitors, and Tyndall's History makes only the briefest mention of the matter. However, it is clear that Powell was able to continue to develop his photographic emulsion technique for particle detection. Much of his effort was in connection with the early experiments on atomic energy, and was not published. Burch, with no teaching commitments, spent a good deal of time collaborating with Government departments on, for example, improving lenses for aerial photography. Fröhlich and Heitler continued to work with E.R.A.. Mott had a succession of official appointments away from Bristol, first with Anti-aircraft Command, dealing with problems of radar and searchlights, and later as Superintendent of Mathematical Research in Armaments, under Lennard-Jones at Fort Halstead, in Kent. This was the R. and D. department evacuated from Woolwich Arsenal. Even while working away from the University, Mott continued to arrange for an "extra-mural group", consisting principally of Fröhlich and Sack, to help him from time to time with various theoretical and computational problems. This arrangement provided them with both work and money, but was not without its difficulties, since both government and industrial people were, occasionally, very sticky about giving Fröhlich access to information that was classed as "confidential", while anything "secret" would have been quite out of the question.

(d) Post-war Planning.

In the latter stages of the war there was a good deal of activity about "post-war planning". Blackett and Fowler had written a letter to the Secretary of the Royal Society in October 1943, as a result of which a committee was set up in the following month, to consider the problems in relation to physics. Similar committees to deal with chemistry, biology, geophysics, and geology/geography followed at monthly intervals. Tyndall was a member of the physics committee, and a combined report covering all the deliberations was issued in January 1945 "for private circulation". It appears that Tyndall had written to Mott, Powell and Skinner in mid 1943 asking for their suggestions in relation to Bristol, since the files contain their replies. Powell begins with a general survey of current problems in nuclear physics, and of the equipment available in Great Britain for producing the necessary energetic particles. He discusses the possibility of re-building the Bristol H.T. generator, which had been dismantled to free the space for use by an Admiralty department. He makes a fairly convincing case for doing so as soon as possible, giving details of the staff and equipment that would be needed. He also considers the possibility of building a cyclotron in Bristol, but shows no great enthusiasm for the idea. Skinner is briefer, and less specific. He confines himself to advocating increased co-operation with industry, and pointing out that research in universities will be more expensive than in pre-war years, because of the higher salaries that staff had been receiving in government service, because of the greatly increased reliance on drawing office and workshop support to which they had become accustomed, and because of the increased elaboration of the apparatus that would be needed. In the event, he did not return to Bristol at all. Mott is quite specific, and detailed. "I hope to make the theoretical physics group at the H.H. Wills Physics Laboratory a centre for research on problems of interest to industrial physicists." In his autobiography, written many years later, he quotes a letter, dated May 1943, in which he refers to "an institute for Industrial Theoretical Physics" either at Bristol or elsewhere. In his reply to Tyndall, he goes on to enumerate in some detail the kind of problem that he would like to tackle, the staff and supporting experimental techniques that would be needed, and concludes with a section on the training (in Universities) of mathematical physicists to fit in with these ideas.

In the Autumn of 1944, when the end of hostilities was in sight, there was a continuous and frequent interchange of letters between Tyndall and Mott. These were concerned with the possible release from war service of existing staff, including Mott himself, with the recruitment of new staff to replace those who were not returning, and with the making of provision for any post-war expansion. Jones had left in 1938 for a chair at Imperial College, and Sucksmith had similarly gone to Sheffield in 1940. Heitler went to the Institute for Advanced Studies in Dublin in 1941, Potter was a sick man on part-time duty, Harper and Gurney were away on war service, and did not return. Other universities were also engaged on staff-recruiting exercises. Mott received informal enquiries from both Cambridge and Oxford about the possibility of his accepting a chair, and although he was clearly interested, particularly in the possibilities at Cambridge, he also found Bristol very attractive. It would appear that Tyndall had suggested that Mott might succeed him as Director of the laboratory when he himself retired in 1947, because, in a letter dated June 12th. 1944, Mott replies "... as regards being director of the lab ..." (he doesn't want to make a decision at present.) Later, in a letter dated December 3rd., there is a passing reference to "the agreement about the directorship". The appointment might have given rise to a delicate situation if Skinner had continued in his intention (of June 1943) of returning to Bristol, since he and Mott were of comparable distinction and seniority. Mott was well aware of this. In the same letter written on June 12th. he says, "It is really the difficulty with Skinner, isn't it ? He is too good and too distinguished for it to be easy to appoint a youngish man over his head, and yet no-one thinks that he is personally the man to be director of a laboratory." In the event, the problem disappeared. In 1943, Skinner had gone to Berkeley, where he stayed until January 1946, returning to England to help set up the Harwell establishment. In March 1944, Mott wrote to Tyndall, "Skinner has almost decided to stay in his present line of business ... From your point of view this makes it less essential that I stay in Bristol, as being the only man available that could be put over Skinner ..." In July, Mott asked Tyndall for help in finding a house in Bristol - presumably to buy. He still owned a house in Cambridge, and before the war had been renting a flat in Clifton. As a result, Tyndall arranged that Mott should be offered the tenancy of Stuart House for a modest sum. It is not clear whether this was done to provide an additional bait to retain Mott. It would have been in character that it should have been, and it seems to have had that effect. By December 1944, discussing the tenancy agreement, Mott wrote "... from your point of view, once my family is in that house, Cambridge will be completely out of the question."

Be the inner history what it may, the outcome was that Mott did not go to Cambridge - then. He moved, with his family, into Stuart House, and remained there for the rest of his time in Bristol. Tyndall did not retire until July 1948, but during the preceding three years a great deal of his time and energy were occupied with University affairs. The Vice-Chancellor, Loveday, had been staying on past retiring age to help the University in a difficult period, but in September 1944 he did (nominally) leave to accept an invitation to take up a government appointment. From then until the new Vice-Chancellor, Philip Morris, arrived in February 1946, Tyndall was either Joint Acting Vice-Chancellor with Loveday (up to April 1945) or else Acting Vice-Chancellor (from May 1945 to February 1946), or Pro-Vice-Chancellor (from March 1946 until his retirement). In these circumstances it was inevitable that much of the responsibility for running the Physics Department should fall on the shoulders of others, particularly Piper and Mott. When Tyndall's retirement was imminent, Mott was duly elected to the H.O. Wills chair which Tyndall would vacate, and to the Directorship of the laboratory. This time, it was all done very properly, with the setting up of a Joint Committee of Senate and Council, and the appointment of two External Assessors, so as to make it respectable.

Tyndall was awarded a C.B.E. in 1950, but it was not until 1958 that the University thought fit to give him the honorary degree of LlD. There have been few, if any, who deserved such recognition more. Not only was he responsible for the erection of the laboratory building but, even more important, also for the selection of staff, the general policy and the establishment of the congenial atmosphere which characterised the whole Department. "In the University, he had occupied every position in department, faculty, senate and council from undergraduate, assistant lecturer to head of a department and Acting Vice-Chancellor." (Royal Society Biographical Memoir) The sum of his contributions to British science, both in Bristol and nationally, could well have merited a knighthood. I have been told that, in his later years, an attempt was made to secure this honour for him; the reason for the lack of success was not known to my informant.

APPENDIX to CHAPTER 2.

Dr Keith kindly lent me photocopies of documents from the Rockefeller archives, and the following summary is based both on these and on his own published account. It appears that, already in 1927, with the new laboratory just opened, Tyndall had been in touch with the Paris office of the Foundation, trying to obtain funds to pay additional research fellows - without success. However, representatives of the Foundation visited Bristol in October 1928, and again in April 1929, to discuss proposals. Tyndall again suggested the provision of money to pay 3-4 young scientists, 26-30 years old, to work in the Physics Department. The representatives comment in their confidential report that, "Tyndall is ambitious to make a showing for physics at Bristol comparable to the grandeur of his laboratory" and that "it looks as if he has already strained the finances of the University." They conclude that the scheme for financing 3 - 4 fellows is entirely out of the question. Nevertheless Tyndall, never one to be easily discouraged, went ahead with a formal appeal, supported by the Vice-Chancellor and the Chairman of Council, again with no result. Another visit from representatives of the Foundation took place in October 1929, and they then stated that the board "would not even consider " any proposal which placed the entire financial burden on them: they normally expected a contribution of at least 50% from the Institution. However, the visit had made a favourable impression on them, the Vice-Chancellor (Loveday) being described as "a very agreeable gentleman ... inspiring confidence at once." The result was a more modest proposal by Tyndall - cut by half, in fact - but even this was described as "quite unsatisfactory in many details." We may guess with some confidence that Tyndall regarded this as a step forward, since the rejection was less categorical. The main difficulty seems to have been that the University was not willing to commit itself to the provision of "new money" to match the suggested Rockefeller grant, and was trying to camouflage this fact by financial manipulation of existing resources. Tyndall also perceived that the Foundation would look more favourably on requests for more specific and well-defined purposes, and so shifted his ground to a request for the financing of an endowed Chair in "the chemical borderland of physics." This took advantage of the involvement and prestige of Lennard-Jones, who had also made a favourable impression.

The results of these visits and informal discussions was the submission in April 1930 of a formal appeal for support. It runs to 25 pages of typescript, and gives, in addition to the proposals, the historical background of the laboratory, together with what can only be described as advertising material. It asks for £50,000 from the Foundation, and says "Efforts must be made on our part to secure the additional £25,000 which is required." A month later, in May 1930, the Foundation said that they were prepared to proceed on the basis of the papers submitted. This looked like success, but there were still problems ahead. In the October of the same year, the University Treasurer reported that they were having difficulty in raising the money locally. The only possibility seemed to be the Wills family again. The elder brothers were dead, and Melville did not have the same enthusiasm for benefactions. The case proved hopeless without disclosing to him the situation in relation to the Rockefeller Foundation. Out of respect for the wishes of that body, the University was reluctant to do this - but eventually did so. The result was the offer of £5,000 - a quite inadequate sum: but after further pressure, this was raised to the necessary £25,000. It is reported that Melville Wills thought that he had been blackmailed, and Tyndall mentions that Monica Wills, the widow of Harry, had had a hand in the affair.

At this, the head of the Paris office of the Foundation wrote to the New York headquarters recommending that the request be approved. The letter was quite enthusiastic. The writer said that he had been able to obtain opinions from men interested in Physics and Chemistry in many places, and "without exception, Bristol was spoken of as a place in which research in physics ... stood very high. Opinions vary, but the indication seems to be that Bristol is placed higher in this respect than the other universities of England, excepting perhaps Oxford and Cambridge. (Note the "perhaps"!) Its chief advantage lies in the fact that two men, Tyndall and Lennard-Jones, are the guiding personalities." All this was wasted, however, since the papers did not arrive in time for the meeting in New York. Nevertheless, the grant was approved, and the money was received on February 17th. 1931. It seems probable that some of it was used to create a second endowed Chair in Physics, since the Melville Wills Chair of Theoretical Physics also dates from 1931.

CHAPTER 3

ADMINISTRATION.

(a) Organisation and Committees.

In 1935, the Physics Department had a staff of 10, was concerned with the teaching of 80 undergraduates (including 20 taking courses at Intermediate level), employed about 8 technical staff (at a guess) and one secretary. Forty years later there were 80 staff, 340 undergraduates and 50 graduate students, 55 technical staff and 10 secretaries. This increase in size was accompanied by a greatly increased complexity of organisation. The Department had grown up under Tyndall's guidance and he was, without any doubt, The Director. This is very clearly illustrated by the story of Mott's appointment, on p.8. In his History, he says "I took men of seniority into my confidence - a policy that I never hesitated to adopt when appropriate." - a statement that makes it clear that he, and he alone, was the judge of when it was appropriate. Piper was his right-hand-man, who did a large part of the routine work of running the Department. Now, we have a Committee of Professors, and regular Staff Meetings; there are Working Parties on various subjects, and Standing Committees on this and that. As long ago as 1969 a memorandum appeared which listed 24 "administrative responsibilities" that had been delegated to members of staff. In this chapter we will be concerned with the history of these changes.

To be fair to Tyndall, he had discussed with Lennard-Jones in 1926 the general policy on research that should be adopted in the infant Department. And in 1945 he had an extensive correspondence with Mott, still away on war service, about the recruitment of new staff. He clearly thought it "appropriate", and that the two people were "men of seniority". He had already decided that Mott was to succeed him as Director, and he had no doubt that this would happen. But in a memorandum dated November 1947, - just before he retired - he says "It must be admitted that, through the 20-year period of marked growth, the administrative machine in my hands has not materially altered, except by adaptation from time to time to meet increased demands. Probably, as a result, it will show its defects badly when I am not there to coax it at the critical moment; it may well be that the time has come to scrap it and devise another." Four months earlier, Powell and Fröhlich had jointly sent a letter to Mott on the subject. In it, they suggest, with some diffidence, an organisation that would consist of:- (a) a Director with final authority, who was not heavily engaged in administration, in University business, or in details of teaching organisation: (b) heads of sub-departments who direct scientific work in practice and are themselves engaged in research: (c) a head of the organisation of teaching: (d) an administrator responsible for general day-to-day laboratory business: (e) a policy committee that would have regular meetings for the discussion of new problems, the making of major decisions and the resolution of points of difficulty.

It is rather remarkable how much of this came to pass. The "sub-departments" are now known as research groups. When I grew into a position of some authority in the Department, I made it my business to pay special attention to the teaching. We eventually appointed an Administrative Officer (M.G. Smith) to look after much of the routine administration. His title was officially "Lecturer/Administrative Officer": This was suggested by Sir Philip Morris, who said that with this title "he would then be one of you; otherwise he won't." When Powell succeeded Pryce as Head of Department he promptly set up a Steering Committee of professors. The only other point in the list is the first one. In my view, at least, it has never been found possible to free the Director from administration sufficiently to allow him to concentrate on broad issues of policy, both in education and research.

The memorandum by Tyndall which is quoted above is something of a mystery. It runs to ten closely-typed foolscap pages, and was cyclostyled, so that multiple copies must have been envisaged. Why it was written, and for whose information, is not clear. It includes a brief survey of the history and current position of the Department, and thumb-nail sketches of the staff. It also contains several shrewd remarks about organisation. He comments, for example, on the problem of the dedicated research worker, justifiably made a professor, but still quite unsuitable for the control of a Department that has teaching responsibilities. (This was in the days when there was usually only one professor in a department.) He points out two particular difficulties which are likely to arise on his own retirement. The first relates to financial control. He "suspects" that the leaders of research groups (he mentions Powell in particular) would like to have complete control over their finances, and expresses himself with some firmness against the idea, and in favour of the central control which he himself had exercised. "I have yet to be convinced that either economy or increased efficiency would result from a change". The system which has evolved, of almost complete autonomy within the limits of agreed group budgets, would doubtless have surprised him by its success. The second problem that he mentions is what he calls the control

of essential services, for example the allocation of priorities to jobs in the workshop. The man to whom the task is given must remember that "he may be dealing with a man of high international repute, who may have the zeal of a fanatic, the impatience of an artist, and a profound belief that his own research is far more important than that of anybody else." There, clearly, speaks the voice of experience.
Tyndall retired in 1948, and Mott, taking over as Director, continued to rely heavily on Piper for much of the administration. When Piper retired in 1953, it seemed natural that I should take on many of the jobs that he had been doing. I had been helping him, with examination marks for example, for some time. I was never appointed to any such role; I just drifted into it, though doubtless with Mott's approval. There were no sudden changes with the advent of the new Director, although we may guess that Powell had considerably increased autonomy over the spending of his funds. This was the beginning of the period when he was relying heavily on D.S.I.R. for support, and any other arrangement would not have been reasonable. The numerous grants which Mott had coaxed out of various industrial organisations were mainly for the salaries of research workers whose activities were under his own direct guidance. It is therefore again reasonable that he should retain personal control of the spending, which he did. When others began to obtain research grants from D.S.I.R., the original application was approved by, and often made in the name of, the Head of Department; but once the grant was given, the spending of it was under the control of the group leader. In 1947, - actually before Tyndall's retirement - Salter had been appointed as storekeeper. (Interestingly, this was not as the result of a job advertisement, but following an enquiry from him about the possibility of employment, made originally to the Chemistry department. He had been a war-time evacuee to Sidcot School, with one of the London Polytechnics, liked living in the West country, married, and decided to settle down.) He was soon given the task of acting as book-keeper for all spending other than salaries, and of keeping a watchful eye on the rate of spending generally, under the supervision of, first, myself, then John Davies and later John Alcock. By 1964, there was a well established routine of collecting estimates of next year's expenses from all research groups, teaching labs, lecture theatres and workshops. For some obscure historical reason these were known by the University as "urgent needs". The requests were collated and sent to the Finance Officer/Vice-Chancellor for approval. When approval was received - usually for a smaller sum than requested - the available funds were apportioned between claimants by the Director or his nominee. For many years this was one of my jobs, and in the later stages the result was approved by the Steering Committee before being put into operation. I do not know when this drill was established, but in 1969 the suggestion was made at a staff meeting that the allocation should become the collective responsibility of that body itself. The suggestion was not approved, and the existing system continued.

Staff meetings have a long history. A notice giving the Agenda for one has survived from as long ago as March 1949, the first year of Mott's Directorship. At that time they were held at irregular intervals, and the only record of their deliberations consists of some untidy manuscript notes that I made at the time, from 1950 onwards. They were concerned mainly with details of arrangements for undergraduate teaching, such as syllabi, time-tables, examinations etc., and more rarely with things like workshop costing and the deficiencies of the library. Over the years there was a slow change in their character. The habit of setting up "working parties" and "standing committees" to look after the details of particular topics began to grow, and matters of more substance began to appear on the agenda. The early 1960's saw the planning and building of the extension to the Department, with which Pryce was much concerned, and in November 1963 he convened a special staff meeting to discuss the whole question of the proposed growth of the Department. In the following year there was a Science Faculty working party on Departmental Organisation, and, again, its findings were discussed at length at a staff meeting.

When Powell succeeded Pryce as Head of Department, staff meetings continued much as before. Typed and duplicated Memoranda of the proceedings exist from June 1964, and I think that this date was in fact the beginning of the practice. The meeting itself did not wish these records to be called "minutes": they thought it sounded too formal. Around 1965 there was a major re-organisation of undergraduate teaching, in the Faculty of Science, with the introduction of the three-subject first year, and of joint honours schools. These questions were also discussed at length in staff meetings. Another matter considered was the desirability of changes in the syllabus content to give increased emphasis to "applicable physics" i.e. topics with possible technological applications. This was referred to a working party. In 1969 there was a discussion on departmental policy on possible expansion of both undergraduate and post-graduate numbers in the 1972-75 quinquennium. All these examples show that the agenda for staff meetings were no longer confined to matters of detail. The composition of the meeting itself had been extended in 1967/68 to include representatives of both undergraduate and post-graduate students, and also the technical and secretarial staff. Meetings were now held regularly, twice each term. A memorandum on Proposed Division of Administrative Duties, dated July 1969, includes the names of ten people, other than the Professoriate, who had been given jobs to do. Participatory democracy was the order of the day; but I well remember a conversation with Powell about such matters which ended with his prescription "but not too much bloody democracy".

The other departmental committee with a long history was known originally as the Steering Committee. Its establishment was proposed by Powell in 1964, when the Joint Committee of Senate and Council was considering the consequences of Pryce's resignation. Powell proposed, and the Committee approved, the setting up of an "advisory body" to "keep the main policy of the department under continuous review, and to help maintain a sense of unity among the various sections of the laboratory". It was to consist of the Professors of Physics, and one or more senior academic staff. One of Powell's first actions on succeeding Pryce as Head of Department was to set up such a Steering Committee, which included all the physics Professors. I attended regularly, wearing my "senior tutor" hat, and John Davies was often present when matters of finance were to be discussed. Others were invited to attend from time to time for particular reasons. In fact, in spite of the declared original intention, the minutes do not record anything that could be called a major policy decision. A great deal of time was spent reviewing the progress of graduate students, considering extensions of their periods of study, and sources of finance for them. The result was to achieve some degree of uniformity in the treatment of the students which could not easily have been obtained by any other mechanism. Possibly even more time was spent on staff promotions, salary increments and - particularly - appointments to staff vacancies. Although no single point could be called a major policy decision, collectively the proceedings formed a very effective means of directing the research effort of the Department. For example, a phrase that frequently occurs in the minutes is that "more support should be given to Dr X" - or should not be given, as the case may be. The support might take then form of a junior staff appointment, the services of an additional technician, or the attachment of a PhD candidate in the coming session, and so on. A similar, and more routine method of exercising the same kind of direction was the annual operation of agreeing the details of the departmental budget. The Committee had the last word about the proposals to be submitted to the Vice-Chancellor, and about the distribution of the money available. Occasionally rather more drastic decisions on research work had to be taken. For example, when Pryce left, experimental work on spectroscopy was stopped (this affected only one man) and proposals to promote astronomy were abandoned. When Perkins left, plans for the setting up of a bubble-chamber group were abandoned, and the resources diverted elsewhere. Shortly after Ziman took over as Director at the end of 1976, the name was changed from "Steering Committee" to "Committee of Professors".

I am told that, in 1988 it was agreed that a representative elected by and from the non-professorial, UGC- funded academic staff should join the committee, and its name was changed back again to Steering Committee. The first of these representatives was Rodney Hillier. The functions of the committee have remained about the same.

A word or two about some of the other committees that exist might not be out of place. Possibly the most important is the "Departmental Committee on Technical Staff". It was set up in February 1976. For the previous year or so, Chambers had been responsible in the Department for matters concerning laboratory staff, but when he became chairman of the University committee dealing with the same business, some change was clearly called for. Frank, as Director, decided to set up this permanent committee to advise him. It originally consisted of five academic staff, with Nye as chairman. Its birth followed a particularly disturbed period of staff problems. In 1972 there had been a national "re-structuring" exercise, carried out by the Department of Employment, with the objective of rationalising pay scales and conditions of employment over all universities, and making them "comparable" with similar employment elsewhere. The result was an elaborate, civil service kind of scheme, with lots of rules and procedures, and endless scope for argument about interpretation. The objective of greater uniformity may perhaps have been achieved, but the cost in terms of internal harmony was considerable. It marked a first step towards attitudes of confrontation between academic and technical staff, which are more usually associated with the relations between employer and employees in industry. The morale of the technician work-force reached an all-time low: in Chambers' words, the net effect in Bristol was "to turn a reasonably cheerful and co-operative group of technicians, spread reasonably over the range of grades, into a far more highly paid, but not very contented or co-operative group, with a nonsensical spread over grades".

One of the first tasks of the new committee was to sort out all the anomalies, and smooth out all the problems arising from this exercise. This gradually merged into the annual operation relating to promotions and salary increments. The time was also a period of increased trade union activity in the world of technicians, and the committee received a good deal of help from our local representatives. A little later it took the secretarial staff under its wing, as well as the technical staff. There then followed a difficult period of claims for general salary increases, to which the committee was usually sympathetic, and of financial stringency following actions by central government, with the committee playing piggy-in-the-middle. Nye proved to be an excellent chairman, with his combination of a calm, unhurried manner, an eminently reasonable approach to difficulties, and a sympathetic treatment of individual problems. Formally, the committee existed to advise the Head of Department, and in fact it saved him an enormous amount of time and worry. From time to time, it has concerned itself with wider issues, such as the age structure of our group of departmental technicians, questions of re-deployment to match the changing balance of work in the Department, and the problem of recruiting skilled staff, particularly in the field of electronics, where there is strong competition from commercial and industrial organisations, able and willing to pay higher salaries than the university scales. Full minutes, and much other relevant paper work, have been preserved, and it is clear that the committee has discharged an important and difficult task very successfully.

The laboratory Safety Committee was set up in 1973, the University having appointed its first Safety Officer in the previous year. This is not to imply that no attention was paid to such matters before that date. An item "Safety Precautions" appears on the agenda for a staff meeting in 1949. For many years the Laboratory Assistants Committee of the University had concerned itself with First Aid, and there had been an organisation dealing with Radiation Protection for a shorter period. At one stage I was acting as Radiation Protection Officer for the University, with the rather thankless task of persuading staff in other departments that used radioactive materials to behave sensibly when disposing of waste. I can find no record of the dates. The Physics Department had also arranged "Fire Drills" from time to time, and some time around 1970 I had written a lengthy and rather naive memo on "Safety precautions" for circulation within the Department. But I suspect that it was the Health and Safety at Work Act, which dates from 1974, which provided the stimulus for systematising these things. Our departmental Safety Committee has been meeting at regular intervals since 1973, and the minutes of its activities have been preserved. Its composition includes academic, technical and secretarial staff. Much of its business has been concerned with fairly trivial matters, with something of the character of a public relations exercise between academic and other staff - in itself quite a valuable function. But it also has some important duties, notably the responsibility for all the radiation protection activities within the Department, through the agency of a departmental radiation supervisor. This is quite an important part of its work, carried out under the watchful eye of the National Radiation Protection Board, which occasionally sends a representative to look around. Fortunately, we have not yet had a major accident, of any kind, so perhaps the Safety Committee has done its job well.

The Library Committee has existed for longer than either of the above. The first minutes in the file are dated February 1959, and it is clear that this was not the first meeting. It consists of 4-5 members of the academic staff, together with the librarian, and its duties are to help and advise the librarian in the discharge of his/her duties. Routine business consists of decisions about taking, or ceasing to take, journals in which the staff are interested, and about the purchase of new text books. When the extension to the building was in the planning stage, the committee was concerned with the arrangements for the proposed extension of the library, and has since been worried about a possible shortage of shelf space. Other matters which have occupied its attention are losses and suspected thefts of books (more than once); arrangements for evening opening and the manning of the desk during these times by graduate students; the system of classification of the books; and the appointment of new library staff.

(b) The Director of the Laboratory.

Committees may be important, or even essential, for the running of a large department, but much still depends on the opinions and personality of the Head. It is therefore of interest to look at the way in which he has been appointed in Bristol. It is clear that, in the early days, Tyndall occupied the post because it was his own creation. He was not appointed to it: the job just grew up around him. In his "History" he writes "The choice of Mott as my successor and the appointment of Powell to the Melville Wills chair vacated by him, was never in doubt." In fact, the prescribed formalities were observed, with a Joint Committee of Senate and Council.

The change in Directorship had no dramatic consequences. Tyndall had been in the habit of discussing the affairs of the Department with Mott for some years, doubtless with the deliberate intention of smoothing the transition. The enormous increase in the scale and cost of Powell's researches meant that the Cosmic Ray Group had become, *de facto*, an almost autonomous unit, at least financially. Since Mott was the Director of the whole laboratory, this might have given rise to some strained relations between him and Powell, but no sign of any such thing was apparent to the rest of us - or to me, at least. There was some talk about the possibility of the split becoming complete, forming two separate departments, but most people were against the idea, and it eventually died.

When Mott left to go to Cambridge in 1954 there were two matters to be decided - the appointment of a new Professor of Physics, and the appointment of a new Director of the Laboratory. For the latter post, the natural choice would have been Powell. He had been at Bristol for 26 years, had been a professor for six years and an F.R.S. for five. He had received his Nobel prize four years earlier. He was a competent administrator, a persuasive talker, and a good chairman. He had a wide knowledge of physics, and a sound judgement of people; he had a pleasant personality, and an easy manner. He seemed to be ideal for the job. In fact, Pryce, already a Professor at Oxford, was appointed. The story of this episode is worth recording in some detail.

It was widely believed at the time that Powell was *persona non grata* with the University hierarchy, because of his left-wing political views. Many years later, I had this confirmed in conversation by a member of the appointing committee set up to find a replacement for Mott. It was not the first time that such a situation had arisen. In his book "Bird of Passage", Peierls recounts how, in 1950, Powell had been considered for the chair in Birmingham that had been vacated by Oliphant. As is quite common in such cases, unofficial approaches were first made, and all seemed set for the official machinery to be put into motion. Indeed the possibility was public knowledge among his junior colleagues at Bristol, some of whom were to make the move with him; they had got to the point of discussing details. The authorities at Birmingham then had a change of heart. Peierls suggests that this was because some people there disapproved of Powell's politics. However, I have been reliably informed that, true though this may have been, the decisive point was that what amounted to a Government directive on the matter had been received. (It should perhaps be recalled that all this took place at about the time of the McCarthy affair in America.) It was also suggested that, with his special interest in the photographic emulsion technique, he might not be a suitable person to run the big accelerator at Birmingham. Whether this was a reason, or an excuse, is anybody's guess. The one thing that is certain is that Powell did not go to Birmingham. Knowledge of the episode was not confined to the two Universities involved. A letter from Skinner, then at Liverpool, to Piper in Bristol, says "I hear that Cecil isn't going to Birmingham after all."

However the matter may have been explained to Powell by the authorities in Birmingham in 1950, it would have been reasonable if he had expected trouble in Bristol in 1954. In fact, it is known that, at an early stage, he had consulted Pryce in Oxford about the problem, saying that, if he were passed over, he wondered if he ought to resign. Pryce's reply was "Well, as a matter of fact, they have asked me if I would be willing to take the job." This must have been an uneasy moment for both of them. The report of the conversation continues that Powell replied "Oh well, in that case, it will be alright."

The procedure for the appointment of a Professor in Bristol is to set up a Joint Committee of Senate and Council. At the first meeting of this body, on 19th. February 1954, the Vice-Chancellor, as chairman, always very proper on matters of protocol, stated that Standing Orders prohibited Professor Mott from being a member of the committee, but he could act as an "assessor" and give advice. He was therefore called in to the meeting and "reviewed the field of possible candidates in both experimental and theoretical physics, and answered questions on personalities mentioned by members. The committee thereupon decided unanimously that Professor Pryce was pre-eminently the man in whom it was interested" and resolved "that he be invited to meet this committee, and to discuss, without prejudice, the appointment to the H.O. Wills Chair of Physics."
Powell was a member of the Joint Committee, and was present at this meeting. It must therefore have taken place after his conversation with Pryce, reported above, - otherwise there would have been no reason for that conversation. Thus unofficial approaches to Pryce had already been made before the committee met. As mentioned above, this kind of procedure is not unusual. But the only man who could have authorised such overtures was the Vice-Chancellor, Sir Philip Morris. Well-informed though he was, he would have been unlikely to take such a step without advice from somebody knowledgeable about the possible field. And who more likely than Mott. It has been said that any committee with Sir Philip in the chair lasted only so long as it took the other members to come round to the Chairman's point of view. In this case it cannot have been very long: Sir Philip had done his homework well. But it is not difficult to imagine the cynical amusement with which Powell must have sat through the charade.

The Joint Committee, according to its terms of reference, had a double duty. Besides recommending the appointment of the new professor, they were also enjoined "to consider the organisation of teaching and research in physics." They postponed action on this second point until Pryce's appointment had been formally approved by Council. Then, having met again, they recommended that the title of Director of the laboratory be allowed to lapse; that "Pryce be given the responsibility for general administrative co-ordination, his administrative duties being exercised in consultation with his professorial colleagues." (In fact, there was only one - Powell. Frank's promotion to a chair was not recommended until later in the same paper); and that "distinct financial provision should continue to be made in the estimates of the Department of Physics for Professor Powell's research activities, over which he would continue to exercise administrative authority and financial responsibility, subject to consultation with Professor Pryce on matters affecting the laboratory generally." Since the work of Powell and his collaborators constituted, at that stage, a large fraction of the research effort of the whole Department, this was a very sensible arrangement. But if, indeed, Powell's feathers had been ruffled by Pryce's appointment, it also constituted a very effective method of smoothing them down.

It is tempting to see the Machiavellian hand of Philip Morris behind these arrangements, and indeed behind the conduct of the whole business. What we shall never know is the extent to which he, personally, supported the initial decision to by-pass Powell as the Director. The authors of the Royal Society obituary notice make a very apt comment:- "Powell would have known better himself. He never allowed political difference to impinge on his loyalty to a colleague or to the University, or to colour his judgement of scientific work." It is perhaps worth adding that this is the only occasion - so far - on which the Head of Department has been appointed from outside. All other Directors have held professorial appointments within the Department for some years before being given the title.

Pryce's 10-year reign as Head of Department is the longest in its history - except Tyndall, of course. Although he was not formally called Director, his administrative duties corresponded to that title. They were in fact rather onerous, since the period included the preparations for two major developments - the building of the extension to the laboratory, and the launching of the MSc course on the Physics of Materials. He did not particularly like this kind of administrative work, but he did it conscientiously, and well. He actually left in 1964 to take up an appointment in the U.S.A., mainly, I think, for personal rather than academic reasons. The Joint Committee set up to consider the consequences of his resignation was, as on the previous occasion, given two tasks; firstly to arrange for a new man to be appointed as a replacement, and secondly to consider the question of the organisation of the Physics Department. It fulfilled its first task by promoting Bob Chambers. As for the second, it recommended that Powell be appointed to the H.O. Wills Chair, and that he be invited to accept the "headship" of the Department. The word Director was carefully not used. Clearly, with the passage of time, Powell had achieved a certain respectability. He had even been appointed a Pro-Vice-Chancellor. Sir Philip Morris was still Vice-Chancellor, and must have approved of the step.

In parenthesis, a note on labels and titles might be in order at this point; the sequence is a bit complicated. The title Professor of Theoretical Physics had not been used since 1948. Lennard-Jones had been so called in 1927, but when, in 1931, a new endowed chair came into being, i.e. the Melville Wills Chair, then he, and subsequently Mott, was called Melville Wills Professor of Theoretical Physics. The other endowed chair, named after H.O. Wills, was the senior appointment, and so considered to be the more prestigious of the two. Thus, in 1948, Mott became H.O. Wills Professor of Physics (and Director of the Laboratory), and Powell became Melville Wills Professor of physics. Previously, he had just been plain Professor. When Pryce was appointed, it was to the H.O. Wills Chair, (but he was not to be called Director), and Powell continued as Melville Wills Professor. Frank was appointed to a newly created third chair at the same time; this was not endowed, and so had no name. On Pryce's departure in 1964, the game of musical chairs continued. Powell moved up to the H.O. Wills Chair, (and the title of Director was revived), Frank became Melville Wills Professor, and the third chair was occupied by Ziman, with the title of Professor of Theoretical Physics. Chambers was also raised to the rank of professor, making four in all. This situation lasted until Powell's retirement in 1969, when they all again moved up one place, Ziman becoming Melville Wills Professor with no qualifying adjective, and Nye becoming plain professor. Peter Fowler had been Royal Society Professor since 1964, i.e. his salary was paid from Royal Society funds, and Keller was appointed to a personal chair in 1969, with the title Research Professor in Polymer Science. A personal chair is one that ceases to exist when the occupant leaves. (I had thought that this was the status of my own chair, which dates from 1966 and indeed, it was so regarded in the Department when discussions of staffing were taking place. But the formal description was not invented until Keller's appointment, since it was then foreseen - correctly - that the idea would become more popular in the future.) To complete the record of names and titles up to Ziman, when Powell was designated Director in 1964, I was given the title of Assistant Director, which had been in abeyance since Piper's retirement in 1953. This carried with it a salary addition of £200 pa. It replaced my earlier designation of Senior Tutor, which I had held for ten years without financial consequences.

In 1963, when it was known that Pryce was thinking of leaving, there was much discussion among the senior members of the academic staff about the questions that would arise when he did so. As a result, I sent a letter to the Vice-Chancellor setting out some of our views. Our specification for a new Head of Department was that "in addition to outstanding ability in his own specialism, it is most desirable that he should have the widest possible range of interests covering the whole field of physics and should have a sound and mature judgement, good foresight and a good sense of perspective." From memory, I do not think that this was written with any individual in mind. We went on to suggest that, if it should prove not possible to find such a paragon, the idea of a group of people being collectively responsible for major policy decisions should be seriously considered. I do not know who saw this letter in addition to the Vice-Chancellor - if anybody - but clearly the idea was in the air at the time. A Faculty Working Party on Departmental Organisation had stimulated a long memorandum from Pryce which is dated a few weeks later than this letter, and contains similar ideas. After giving details of those administrative duties which have been delegated to others, he goes on to say "However, broad issues of policy tend to be dealt with less democratically, and I am conscious of sometimes exercising arbitrary power in these respects." (Tyndall would not have been so apologetic!). Finally:- " A formally constituted steering committee (the name is immaterial) could have wider responsibilities than advising on academic appointments, and could be a recognised channel, probably through a chairman whose term of office was of limited duration, for dealing with the University authorities." When Powell was appointed to succeed Pryce he had already stated his intention of setting up such a group, which he called "an advisory body". One of his first acts after appointment was to set up the Steering Committee. Had there been any major policy decisions to be taken, this committee would have been ideally suited for the role. In fact, as we have seen above, most of its time was spent on more minor problems which, collectively, gave rise to the effective policy.

In 1969, the impending retirement of Powell gave rise to another little flurry of activity of the same kind. This time it was Michael Berry - responsible for organising staff meetings since 1964 - who channelled the views of the non-professorial staff. The memorandum which resulted suggested that the new Head of Department might well be an internal appointment, and also suggested a "constitution" for the running of the Department. Apart from some differences of nomenclature, this was fairly similar to the existing arrangements. The main difference was that the Head of Department (i.e. Director) and the Chairman of the Department (i.e. Assistant Director) should be elected by the Steering Committee. Any idea of this kind was flatly rejected by the Vice-Chancellor, Alec Merrison, either then or, more likely, at some later date when it surfaced again - I forget which. He took the view that he, and he alone, was going to choose which member of a department was going to be responsible to him for its activities. He also maintained that it was essential that the "Director" should be a person, and not a committee, so that he could telephone somebody for an immediate answer to any question that arose.

The non-professorial suggestion that the new Director should be an internal appointment was echoed by the collective view of the Steering Committee, which discussed the matter at some length. We were unanimous that Charles Frank should be the man, and our views were set out in a letter which I wrote to Alex Fraser, the Deputy Registrar. With his connivance, I made the proposal at a meeting of the Science Professors Committee, from where it proceeded through "the normal channels" of Senate and Council. It appears in the Minutes of Council as a recommendation from Senate that Frank should be appointed to the H.O. Wills chair when Powell retired, and also should be appointed to the Directorship of the laboratory. This was duly done.

A similar operation took place in 1974. In December, Hart called a meeting of the non-professorial staff "to discuss the situation that will arise on the retirement of Professor Frank". This would seem to have been rather premature, since the event was eighteen months away. There is no surviving written record of what transpired, and since I was less intimately involved in such matters at the time, I have no memory of it. But when, in 1976, Frank's retirement was more imminent, Senate was informed that the Vice-Chancellor had "nominated" Ziman as Director, and Chambers as Assistant Director, both for a period of five years. No mention this time of any recommendation from any committee: the new dispensation, under which the VC was a "manager" had arrived. The appointment of a Head for a finite term was also an innovation which was to be continued, although the concept was not new. Ziman's term as Director did indeed terminate after five years, in 1981. Towards the end of this period his research interests had moved away from physics towards what may be called the sociological relations of science, including, for example, questions concerned with the dissemination of scientific information. In February 1982 he wrote a memorandum setting out his views on these matters, and suggesting that the title of his chair be changed to Professor of Science Studies. It would appear that this idea did not find favour, and in September of the same year he resigned, and took up an honorary appointment at Imperial College, in which he would be in a position to follow his inclinations. He was succeeded as Director by Enderby, who had been with us as a professor since 1976. Again the appointment was on the nomination of the Vice-Chancellor, and again was for five years. However after only four years he was proposed by SERC as the British Assistant Director of the ILL laboratory at Grenoble, and was seconded to that post for three years, Chambers taking over as Director in his absence. And at that point, this catalogue must stop.

(c) Policy and Planning.

The purpose of the administrative machinery of directors, committees and so on is to take decisions about the way that the Department is to be run. In this section I propose to look at the way in which this process has operated. Many of the points to be mentioned are discussed in more detail in other sections, but here we attempt an overall view. It is convenient to consider separately matters relating to teaching and research.

The details of undergraduate teaching, such as syllabus content, examinations, tutorials etc. are dealt with in a later chapter. The only additional point to be made here is that almost all of the decisions were made by, or at least endorsed by, the Staff Meeting. The question of student numbers is somewhat different, and is discussed in detail in the section on statistics. During the period with which we are concerned, the structure of the central University government required each department to produce a series of Quinquennial Proposals. The submissions made by the Physics Department make it clear that our policy has always been a general reluctance to increase the size of the Department, as measured by the number of undergraduates, combined with a willingness to do so in response to requests from the University as a whole, or from outside bodies such as National Government. These proposals would have been drafted by the Head of Department, and discussed by the Steering Committee when it existed, but the views of the staff meeting would have been known and borne in mind. On the other hand, there has been a widespread and continuing desire to increase the number of graduate students proportionately more than the number of undergraduates. This also is mentioned in some of the Quinquennial Proposals. The suggestion is justified by a comparison of the post-graduate/undergraduate ratio in Bristol with that in other front rank physics departments. Both of these attitudes represent consensus views in the Department, reflected in the formal statements.

From time to time there have been proposals for more radical changes to the programme of teaching, but for one reason or another these never came to fruition. In 1944, a proposal "to establish a Department of Theoretical Physics, with a professor, two lecturers, an assistant lecturer and two research students" got as far as being included in a formal University statement to the U.G.C. It did not happen. In 1961 there was a suggestion for the creation of a Chair in the Physics of Materials, associated with proposals for joint courses run in conjunction with the Department of Mechanical Engineering. This did not happen, either. At some point (the relevant memorandum is undated) there were discussions with the University of Bath about the possibility of running a joint one-year course on Geophysics, which would form part (? or all?) of the final year of a degree in that subject in both Universities. This was intended to take advantage of the interest of Frank and Nye in such matters, and of the involvement of the Bath department in oceanography. It is perhaps not surprising that this, too, proved to be a non-starter. As recently as 1981 there was a suggestion of joint courses on microelectronics with the engineering faculty, which met the same fate as the others. Thus there have been no major innovations in undergraduate teaching as a result of policy decisions: all changes that have taken place - and in total they have been quite considerable - have come about as the cumulative effect of minor modifications.

This is also largely true for questions relating to research, although here there are exceptions. In 1972 the Department had a visit from the SRC Physics Committee, and in the Foreword to the booklet prepared for their information, Frank wrote:- "This pattern is to some extent the outcome of planning, but also to a very large extent the result of encouraging merit where we find it." The original policy decision of Tyndall and Lennard-Jones, not to have a research policy, but to wait and see what happened, seems to have had repercussions ever since. In 1943 Tyndall corresponded with Mott, Skinner and Powell about the desirability of moving into the important new research field of nuclear physics and, in particular, about whether to build a cyclotron. There is no record of any deliberate decision not to proceed with this proposal, or, indeed, not to re-erect the 700kV accelerator set which Powell had built before the war, but which had been "temporarily" dismantled (see page 51). Powell became engaged, more or less by chance, in the application of his photographic emulsion technique to cosmic ray studies. Skinner never returned to the Department after the war, and Mott became involved in the application of solid state theory to industrial problems. This last, at least, was a conscious policy decision, and one which influenced the character of theoretical research, and to some extent of experimental research also, even after Mott had left. But almost all of the more recent developments, up to the 1980's, fit Frank's description. Perhaps the most striking example is the growth of the large and successful polymer group from the appointment to a temporary research associateship of one man (Keller) who was at that time working here as a graduate student. The time was ripe for the development of this field of study, Keller had the ability, the enthusiasm, and the enormous drive, and Frank provided the opportunity and the encouragement. Another, much later example on very similar lines, is provided by John Steeds' work on electron-microscope techniques. Again the group leader is a man of ability and determination, and, in this case, with a temperament in tune with current tendencies towards co-operative research, particularly with industrial organisations. The Department has provided the opportunities and the facilities to get moving. A less spectacular example is Nye, who was recruited with the possibility in mind that he would help teach the MSc course on the properties of materials. He gradually built up a reputation in glaciology, and eventually a small, but important research group on that subject. The work of Hart on the interferometry of X-rays was moving along similar lines when he was tempted away by a secondment to the Central Policy Review Staff in the Cabinet Office, and never returned to Bristol. In none of these cases was there a deliberate decision by the Director or by the Steering Committee that we would expand in this or that direction. The decision was to encourage, and provide facilities for, an able research worker already in post to allow him to continue work on which he was already engaged. The policy was not always successful. Aplin produced promising ideas for experimental work on gravitational radiation, but after a year or two meeting and overcoming serious difficulties, the ideas showed signs of not living up to their original promise: the work was overtaken by events elsewhere, and was discontinued.

The growth of the theory group is a kind of intermediate case. Ever since Mott's original appointment there have been theorists on the staff. Pryce was active himself, and supervised a number of research students, but showed no obvious inclination to build up a "group". When he left, Ziman was appointed and clearly had the intention of doing just this. His views must have been known to the appointing committee, and it seems likely that he was appointed partly for this reason. He had the full support of the Steering Committee in his efforts. Thus, while theoretical physics was not a new line of research, its expansion under Ziman can be said to have been consciously planned, and the success of the expansion was due to the personality and ability of the one man.

It was not until 1976 that the first real planning decision was made to embark on a new line of experimental research. Enderby was appointed as a professor with the understanding that he would bring with him equipment and staff from Leicester, and would continue his work on the structure of liquids. Ziman had an interest in liquid metals and amorphous solids which provided a link between existing work and the new development, but it was essentially a new departure, which has flourished ever since.

Other attempts at planning have not worked out so well. At one stage there were several staff members working on topics that were described as "solid state (various)". They had been allowed to follow their own inclinations, and, at that stage, these had not led them in the direction of building up sizeable research "groups". We had Gibbs on dielectrics, Kay on X-ray crystal structures, Gill on magnetic resonance, Ashbee on ceramics, and Lang on various X-ray techniques. The Steering Committee had the idea that it would be desirable if the efforts of at least some of these able people could be combined, or at least co-ordinated. Attempts at amalgamation without coercion were not really successful and in the end common sense prevailed over considerations of administrative tidiness, and they were allowed to continue making their contributions to the life of the Department in their own way.

There have also been proposals for new research developments which never really got off the ground at all. Astronomy, suggested by Pryce, is one of them. This was thought of as being a real research group, but the idea was dropped when Pryce left. The successful undergraduate courses on the subject are quite a separate matter. The story of how we nearly acquired a National Institute of Applied Optics is worth recording. This was more than just a bright idea. It involved several eminent people outside the University intermittently for a couple of years, and very nearly happened. Since it was intimately concerned with C.R. Burch, it is included in the chapter on Research and need not be repeated here. (see p.73). But another scheme, also involving Burch, should be mentioned. Around 1966, he became very interested in problems of mineral separation using techniques involving fluid flow. For a period he had working with him Richard Mozeley, a young man whose initial training had been at the Camborne School of Mines. He held a series of research appointments in the Department, financed either by SRC or the National Research and Development Corporation, interspersed with periods in laboratories elsewhere. This led to the suggestion of the setting up of a Fluid Motion Laboratory, which would involve Burch and Mozeley and staff from our Department of Applied Mathematics. Again, nothing came of it, and Mozeley finally left in 1971, returning to Cornwall, where he set up and ran a very successful company manufacturing the kind of mineral dressing machinery with which he had been concerned in his researches.

Another proposal which never really came to fruition is biophysics, and this merits discussion at rather greater length. It has a long history, all mixed up with the fortunes of the Medical Physics Department of the Bristol Hospitals. This had existed since 1938, at which time its staff consisted of one half-time physicist (myself). While I was away during the war my place was taken by John Munson, who had been one of Tyndall's research students. By the late 1950's, the Department had grown to consist of several physicists and technicians headed by Herbert Freundlich. In addition to his other duties, Freundlich ran a one-year course, with about three students per year, leading to a "Post-graduate Certificate in Medical Physics", awarded by the University. In December 1958, Dr. Mayneord, of the Royal Cancer Hospital, wrote to all people likely to be interested, drawing attention to an urgent need for scientists knowledgeable about the biological effects of radiation, a need that was likely to increase with the increasing use of high energy radiations in science and industry. Powell saw this letter, and immediately revived an idea, floated some years previously, for setting up a post-graduate school in radio-biology - a much more substantial proposition than that envisaged by Mayneord. He suggested that Freundlich's existing course could form the first year, to be followed by one or two more years of research, leading to either an MSc or a PhD. The Vice-Chancellor was sympathetic, but advocated proceeding slowly. One of the first steps was to give Freundlich the status of a University Lecturer in Medical Physics. (Thereafter he used to give part of our 1st. MB course in the Physics Department.) But further progress in this direction was effectively stopped by a letter from Mayneord pointing out, quite correctly, that Freundlich's course was far too closely tied to the technology of radiation therapy to constitute a proper basis for any scientific qualification.

In the following year (December 1959) a memorandum written by Pryce, leading up to the formulation of our 1962-67 quinquennial proposals, listed biophysics as one of four possible lines of development. There were no definite proposals, except that the emphasis would be on research. About the same time, a committee in the Medical Faculty made similar suggestions, equally vague. Nothing came of either of them. At the end of 1964, when discussing the next set of quinquennial proposals, the Steering Committee invited Freundlich to one of its meetings. He proposed the establishment of a research group consisting of two staff, two technicians and two research students, concerned with "radiation physics", to be housed in the Physics Department, and - apparently - financed by the University. It is even described as a "Biophysics School" or a Sub-department of Physics applied to Biology and Medicine. But the final version of the quinquennial proposals contains no more than a statement that "Mr Freundlich's department could, with advantage, be more closely integrated with the University". Three years later, the Steering Committee minutes for April 1967 record a discussion of "the establishment of a Biophysics Unit in the Physics Department" again including mention of suggested staffing levels. There were difficulties arising from a shortage of UGC money, but the minute concludes "we still approve in principle the establishment of a Biophysics Unit here, and hope to offer it accommodation, but probably not financial or technical support." Freundlich was present at the next meeting. He was not unduly distressed by the lack of financial support, but still hoped that a biophysics group could be set up in the Physics Department. His idea now was that it should be under the direction of Peter Wells, who had been doing some very successful work in the hospital on the use of ultrasonics in medical diagnosis. The financing of this work had given rise to a considerable pile of correspondence, involving the Vice-Chancellor, the Secretary of the Hospital, the Medical Research Council and the Ministry of Health, as well as Freundlich and Powell, who was now Head of Department. Powell was sympathetic.

In one letter he wrote "For some years we have had under consideration the establishment of a Biophysics group, in the Department of Physics here ..."He agreed to write to the Vice-Chancellor recommending that Wells be recognised as a Lecturer in the Department of Physics (this duly happened) and that Freundlich and Wells constitute a Biophysics group within the Department, to be provided with a room, but no U.G.C. money. This did not happen, but Powell did sign an application to MRC asking for continuation of support for Wells' work. This was approved - but Wells continued to work at the hospital until he left to take up a more senior appointment at Cardiff.

The topic next surfaces in 1970, by which time Frank had taken over as Director. Without being fully informed of the complicated previous history, only hinted at above, he sent off to the MRC an application for yet another extension of Wells' research grant. This got a rather frosty reception: the grant had been originally made in 1963, for one year. But in the course of related correspondence between Frank and the Vice-Chancellor, Frank says "Freundlich's views are that he would like us to declare an intention to form a Biophysics Unit within the Department of Physics, and to press the MRC to finance it...". However, "neither I nor the other Professors of Physics are strongly sold on the idea." Wells got the extension of his grant, but nothing more has been heard of the biophysics unit.

Looking at the protracted story with hindsight, one can probably see why nothing really happened. Powell was always in favour of the idea, but nobody else in the Department was much interested at that time. Everybody was too much concerned with their own research to get enthusiastic about new developments in unrelated fields. Even Powell - or perhaps particularly Powell - was fully occupied with other matters, with no time to do more than support an idea in general terms. Freundlich was always trying to expand, with bursts of enthusiasm which sometimes outran his judgement. But the idea was not central to his thinking. It would be nice if it happened: it would increase the status and prestige of his Department considerably. But if it didn't happen, he was both able and willing to carry on as before. And he was not so eminent in his own field that anybody else would be moved to take up his case. Nobody was involved who possessed all the necessary qualities to get a considerable new venture off the ground. So nothing came of it.

However, in preparing the quinquennial proposals in 1975, Frank was able to write that we had acquired "our long-desired niche in biophysics". This had come about because the polymer group had become interested in the structure of biopolymers, a class of materials of widespread occurrence. The leading figure was, and still is, Atkins, although Keller and others have made contributions. In the previous year, the annual symposium run in Bristol under the auspices of the Colston Research Society was on the topic of "The structure of fibrous biopolymers." It was a big conference, with more than a hundred participants, organised by Atkins, and several of the contributors were from the Physics Department also. The word "niche" is very apt, since the topic represents only a very small corner of biophysics. But the work is interesting, important, continues to flourish, and has generated contacts with more than one biological department in the University. It is interesting to note that it grew spontaneously out of the existing interests of staff members, where thirty years of planning proposals had led nowhere.

As far as I know, there has been only one exercise that could be described as "planning" in more general terms than those already mentioned. In January 1980, when planning was becoming a fashionable word, the University undertook a "review of planning up to 1983/4", in preparation for an official visit from the UGC in May. A Physics "Departmental Planning Group" of eight people was set up, with Ziman as the prime mover. By January 1981 it had produced a monumental report, of 80 pages or more, with the title "Policies for the next five years". It has chapters on staff, finance, buildings and equipment, undergraduate teaching, and on the major research groups individually, together with a general introduction by Ziman. It appeared on the agenda for staff meetings in 1981, but it would seem that time did not permit any real discussion. The minutes for June 10th. with Ziman in the chair, say that "a meeting open to all members of the permanent staff .... will be organised specifically to discuss matters arising out of the document". There is no written record extant of this meeting having taken place, and it is not mentioned in the next set of minutes, for September 4th. This was in the next academic year, and Enderby was chairman, Ziman having completed his term of office. It is not mentioned at all in the minutes of the Committee of Professors, although it must be admitted that these are not quite complete for the relevant period. It appears to have sunk without trace, and to have had no direct effect on the affairs of the Department. It may be that the preparation if it was useful in that it influenced the thinking of the various authors; and Ziman's contribution on staffing is of permanent value. It should also be remembered that it was in July 1981 that the UGC bombshell on financial stringency landed on universities, so, in some respects at least, our own planning proposals were overtaken by events - as so often happens.

CHAPTER 4

RESEARCH AND RESEARCH WORKERS

The end of the war period in 1945 makes a convenient starting point for an account of the research work of the Department. On page 9 there is a list of the active research fields in the pre-war years. Of the people there mentioned, Tyndall, Piper and Potter had stopped doing research by 1945, the two first-named because of the pressure of administrative duties, and the last owing to ill-health. Except for Powell and Jackson, the rest had left the Department for one reason or another. The following ten years saw the establishment of two main research groups. Firstly there were a number of theoreticians working under Mott's general guidance on problems in the theory of the solid state, together with one or two experimentalists working on related problems. Secondly there was a rather more closely knit group under Powell, working on cosmic radiation and nuclear physics. There were other research activities also, but these two constituted the major effort. The question which had exercised Tyndall in the early years, of the desirability of specialising in one field, had settled itself.

In the following sections we consider this research under a number of separate headings, so as to provide a more coherent account. (1) Theoretical work under Mott and his successors. (2) The work of Powell and his successors on cosmic radiation and nuclear physics. (3) Experimental solid state physics, including Jackson's work on low temperatures. (4) Burch on applied optics. (5) Keller and the polymer physics group.

(a) Theoretical Physics.

While still in government service, Mott had found time to lay the foundations for his work in Bristol on his return. In a letter dated November 1944, he mentions having talked to potential recruits to the University staff who were working with him at Fort Halstead. He says that he told them "how we intend to set up a school for industrial theoretical research, and to get industry to finance it." Clearly the idea was very important to him. As a result of this kind of contact both Devonshire and Mitchell arrived eventually in Bristol from Fort Halstead. He was also assiduous in making industrial contacts, both in conversation and by letter. In addition to providing funds to pay for research in his department, he regarded these contacts as part of a process of educating industrialists about the benefits to be derived from theoretical physics. His pre-war contacts with Kodak and with E.R.A. have already been mentioned, and he also had dealings with the Iron and Steel Research Council, ICI, BSA, and Robinson's (? the Bristol paper firm?). The net was later cast even wider, to include the Anglo-Iranian Oil Company, A.E.R.E., Metropolitan-Vickers, and the Rayon Research Association. By November 1944 he was able to list seven organisations from whom he "hoped" to collect about £4,000 per year. This, he says, should provide for six men at £500 per annum, with something left over for overheads. £500 was about the salary of a lecturer. They would have to be people on short-term contracts, since firms were reluctant to commit themselves for more than two or three years.

Most of this came to pass. In the Department as a whole, for the nine years from 1945 to 1954, an analysis of the funding of research workers, including research students but excluding University staff, shows the following results:-

(a) About 40% from sources specifically existing for the purpose, such as D.S.I.R. research grants and maintenance grants, the Further Education and Training Scheme for ex-service men, and University graduate scholarships.

(b) About 30% from overseas to finance visitors working in Bristol. Canada and Australia provided more than one-third of the total, and European countries about a quarter.

(c) About 20% from industry or industrial research associations or government research laboratories. ICI and E.R.A. provided the biggest contributions. The figures are in "man-years". Since some of the people were here for less than a full year, and some were paid from more than one source, the figures are not exact, but they do serve to show the substantial contribution from industry. If we consider Mott's theory group alone, the industrial contribution is appreciably higher, forming as much as 30% of the total.

In the same period of nine years, about 210 papers were published from the Department on topics which can be included under the broad heading of solid state physics. About three-quarters of these were primarily theoretical, and about 35 were written by Mott himself, only rarely in collaboration with another author. This was the period when the development of dislocation theory was proceeding apace. There were considerable advances in the theory of dislocations in an elastic continuum, to which various members of the Department, notably Eshelby, made contributions. The results of such calculations were also applied to problems of the mechanical behaviour of crystalline solids, in particular to the work-hardening and creep of metals, and to the theory of precipitation hardening. Mott himself, although no mean mathematician, preferred to deal with problems in which his physical intuition and predilection for general, semi-quantitative arguments, could have full play. The topics were usually suggested by problems encountered in technology, but the resulting papers often dealt with somewhat idealised versions - "basic problems underlying those of immediate technical interest". The results were therefore not always relevant to the particular industrial application which had produced the initial stimulus, but they gradually built up a corpus of knowledge which has been very influential in further developments. Friedel and Nabarro made significant contributions. Other fields of study were the oxidation of metal surfaces, involving questions of diffusion through crystal lattices, and the intricate details of the mechanism of photographic sensitivity, involving the behaviour of point defects in ionic crystals. There was also the major topic of crystal growth. In this field the moving spirit was Charles Frank, of whom we shall have more to say later. The activities of all these people, and of others working with them whose names are less well known outside circles of specialist interest, served to build up for Bristol an international reputation in the general fields of solid state physics and dislocation theory which placed it among the foremost laboratories in the world.

This is perhaps the place to mention one of Mott's major initiatives which was entirely in keeping with his views on the relations between university research and industry. In July 1935 he had organised a "Summer School and Conference" with the title "The Metallic State". This set the pattern for a whole series of similar events continuing at the rate of about one per year while he remained at Bristol, except for the interruption of the war years. This first one, and some of the others, were organised in two parts, as the title suggests. The first part, the Summer School, consisted of a series of lectures which provided a moderately elementary introduction to a subject, and/or a state-of-the-art review. They were often, but not always, given by members of the departmental staff, and their target audience consisted of graduate students from both Bristol and elsewhere, and - more especially - technical people from industrial laboratories. The subject matter was always related to theoretical topics in which the Department was interested and the purpose of the lectures was frankly propagandist - to sell theoretical physics to industrial scientists. The second part, the Conference, followed immediately, and consisted of more specialised research papers on topics related to the subject matter of the school. The Conference speakers were frequently from outside the Department, and often from overseas. The possibility of attending the Conference on favourable terms was sometimes used as a bait to persuade them to give lectures to the school. The concept was novel - indeed scientific conferences in general were comparatively rare in those days - and the atmosphere was very informal. They were generally agreed to have been very successful. Although their main purpose was propaganda for academic theoretical research, directed towards industry, they also served as a good advertisement for Bristol in the academic community. It was also arranged that they ran at a slight profit, and this provided the nucleus of a fund which Mott kept under his own control. It served to finance research workers in his group for short periods, e.g. to bridge the gap between one appointment and another, and also to provide small sums of money for other contingencies which could not readily be coped with through the normal channels.

The first "Summer School" in July 1935, was on "The Metallic State" and the second, two years later, was on "The Conduction of Electricity in Solids." There is no record extant of even the titles of the papers given on this second occasion. The third school, in July 1939, was on "Internal Strains in Solids", and was run jointly with The Physical Society of London; the papers were published in 1940 as part of the Proceedings. The series was resumed after the war break with "Theoretical Physics Applied to Industrial Problems" in June 1946 - a rather longer course of a slightly different character, given mainly by Mott and Fröhlich. Thereafter we had, at the rate of about one a year:- The Strength of Solids; Dislocation Theory (exact title not known); Oxides and Ionic Crystals; The Theory of Crystal Growth; Fundamental Mechanisms of Photographic Sensitivity; Theoretical Physics of Solids; Semi-conductors and Transistors; The Theory of Plastic Deformation of Metals; Defects in Crystalline Solids; and Dislocations in Metals and Inorganic Crystals. In the later years the courses were sometimes given under the aegis of, and with help from, The Institute of Physics, and sometimes the detailed organisation was done by the University Extra-Mural Department. The above list of titles takes us up to the mid-1950's, when Mott went to Cambridge. By this time the scientific conference was becoming a much more common event, and those held at Bristol had lost the character and purpose of the original "Mott Schools".

Mott also became interested in general questions of physics education, as he has been ever since, and this sowed the seed of my own continuing interest in such matters. One of his particular concerns at that time was the pernicious influence (as he saw it) of the Oxbridge system of Open Entrance Scholarships on education in schools. This was a topic with which he continued to be involved when he moved to Cambridge, where he was in a better position to do something about it.

When Mott left, the scale of the activities of the solid state theory group fell dramatically. The rate of publication of papers on all aspects of solid state physics dropped to about half of its previous value, and now only about a quarter of this output was on theory. Frank and Devonshire were the only two theoreticians on the permanent staff that remained. Frank became more and more interested in the work of the burgeoning polymer physics group, and Devonshire continued to plough his lonely furrow in the field of ferroelectrics. All other work on the theoretical aspects of solid state physics in which Mott had been interested ceased abruptly, for the time being. In 1953/54 the departmental list of academic personnel included nine theoreticians in addition to Devonshire and Frank. In 1954/55 there were none. So ended Mott's venture into the realm of theoretical industrial research. It is perhaps idle - but nevertheless interesting - to speculate on what might have happened if his idea had led to the formal establishment of an "Institute", as he had suggested at one stage.

On Mott's departure, two professorial appointments were made. Frank was promoted to a chair, and by this time he could fairly be described as a theoretical physicist. In addition, a new appointment was made from outside the Department to the H.O. Wills endowed chair vacated by Mott. This was M.H.L. Pryce, a distinguished theoretician who already held a professorial appointment at Oxford. The detailed story of this appointment is given elsewhere. The activities of the theoretical group under Pryce, and afterwards, will be considered shortly, but before doing so something must be said about Charles Frank.

In 1946, R.V. Jones, later professor at Aberdeen, had suggested to Mott that Frank, who had been Jones' deputy in the Air Ministry intelligence service for a large part of the war, might welcome an invitation to come to Bristol. Mott duly arranged for him to have a one-year appointment (paid from funds derived from the Shell oil company!). Towards the end of the year, Lennard-Jones invited Frank to Cambridge, and Mott reacted by persuading Bristol to find the money to give him a more permanent post here as a Research Fellow. He remained in the Department until his retirement in 1976, apart from a sabbatical year spent at the Institute of Geophysics of the University of California at La Jolla in 1964/65. Originally a physical chemist, he rapidly became involved in the work of Mott's theory group, while also retaining an interest in other topics, which surfaced from time to time. He always worked closely with experimentalists who were interested in related subjects. No one who was present will forget the occasion, during a lecture which formed part of one of the Mott-schools, when he predicted the existence and form of what became known as a Frank-Read source, and its importance for crystal growth. Thereupon Griffin emerged from the audience with a slide showing a growth spiral on beryl, which agreed exactly with the prediction. Griffin subsequently joined the Department at Bristol, and continued with his experimental researches under Frank's guidance. At all times Frank exerted a stimulating influence on many aspects of the work of others. The list of his own published papers gives a good impression of the variety of his own interests. Most of them deal with topics that can be included under the general heading of solid state physics, but they range over crystal structure, the growth and dissolution of crystals, dislocation theory, deformation mechanisms, the properties of surfaces and interfaces, and molecular arrangement in polymers. Later he became interested in the deformation of the solid earth, and hence in geophysics generally, and also in extending the discussion of molecular arrangement to liquid crystals. His work on dislocation theory and on crystal growth was of particular importance. He supervised the work of a number of research students, both experimental and theoretical, yet he never built up a closely knit research group. He was always willing to discuss anybody's research problems with them, usually with the consequence of shedding a novel light on the question. He was appointed to a chair in 1954, received an O.B.E. in 1946, was elected to the Royal Society in 1954, and received a knighthood in 1977.

Frank's scientific work, like the more modest efforts of Devonshire, was not affected by the replacement of Mott by Pryce. Apart from these two, Pryce came to a department which had no active group of theoreticians, and left it ten years later in more or less the same state. His approach to the subject was quite different from that of Mott - which is, in a way, quite surprising, since Mott effectively chose him as a successor. Pryce was essentially a "pure" physicist, with little interest in technical applications of the subject. This attitude extended to the undergraduate curriculum. For example, to him optics - in those days mainly geometrical optics - was "technology" and therefore should have no place in a degree course in physics. The same was true of electronics: the records show that the only contact that our students had with electronics was a 10-lecture optional course in their third year. He was however, extremely knowledgeable on a very wide range of topics in what he would have called real physics. He could - and did - act as a one-man vetting panel for our draft examination papers for third year honours students. But his specialist research interest was in a rather narrow field - the spectroscopy of ions in crystals. While still at Oxford, before coming to Bristol, he had acted as part-time Head of the theoretical physics division at Harwell. Of this period, Peierls writes in his autobiography "Bird of Passage":- "He could be a devastating critic, and it is said that after each of his visits to Harwell, someone had to go round and comfort the young people he had seen, and assure them that there was still a chance that they might turn out to be competent theoreticians"

His relations with people at Bristol were always amicable, so far as I know, - except with one young man who was something of a difficult character in any case - but he did continue to be rather outspoken in his opinions. The rest of us were considerably embarrassed at one colloquium when he disagreed in no uncertain terms with the opinions of the visiting speaker, who was a person of some eminence. Like Frank, he made no attempt to build up a large research group with interests similar to his own. But he did have a succession of PhD students that he supervised, and he also made one or two appointments to Research Assistant posts in fields more or less closely related to his own. Three of these were experimentalists: Baker, who left for Manchester immediately after Pryce's departure, Llewellyn, who left for an industrial appointment in America, and Gill, who has stayed with us. One of his PhD students, Stoneham, later became head of the theory group at Harwell. This situation is reflected in the list of his published papers in the period. They are almost all on spectroscopic subjects. He was sole author of about 40% and joint author of about 30%. The remaining 30% appeared under the names of his research students. Of those with joint authorship, about two-thirds were written in collaboration with people outside the Department, and often outside England. The appointment to the staff in 1956 of G.N. Fowler was intended to be more permanent than those mentioned above. His interest was - and still is - in theoretical particle physics, and he was appointed because Pryce wanted to broaden the scope of the Bristol theoretical group, and to strengthen this aspect of the Department's work. He left in 1962, however, for an appointment at Newcastle, and later became professor at Exeter. Greenwood was appointed in 1962, and is still with us. Again although a theoretician, his interests did not coincide with those of Pryce, and have included both nuclear physics and problems of liquids and disordered solids. Bohm was appointed to a Research Fellowship in 1957, as befitted his seniority. This could have been thought of as a permanent appointment at the time, but in fact he stayed only four years. It was during this period that he and Aharanov published their famous paper.

During his ten years at Bristol, Pryce was very much occupied with administrative duties. Although he had deliberately not been given the title of Director of the laboratory, the terms of his appointment stated that he was to act as Head of Department. In addition to the normal routine work which this implies, he was heavily involved with the committee work relating to the building of the extension to the laboratory. Planning for this started in 1959, but the actual construction was not completed until well after he had left. It was also the period when the University was making plans to deal with the sudden increase in undergraduate numbers arising from the peak in the birth rate in the immediate post-war years - known familiarly as "the bulge". In addition to all this, we were already engaged on the rather protracted advance planning for the MSc course on Materials, which started in 1969, after Pryce had left. It is thus hardly surprising to read in a memorandum on proposals for the 1962-67 quinquennium, which he wrote in December 1959, " If I could be relieved of my present duties as head of department, I would like to become a professor of theoretical physics, which I look upon as a full-time occupation." Early in 1963 Senate approved the creation of a new Chair of Theoretical Physics: Pryce was still in post, with the title of H.O. Wills Professor, and the new development is particularly interesting in view of his remark quoted above. The post was advertised with this title

- a rather unusual step at that time, when the old-boy network was more usually called into play. The appropriate committee recommended in June 1963 that John Ziman should be appointed, the appointment to date from August 1964. I think, but am not sure, that the long delay was due to the fact that Ziman already had commitments elsewhere. Pryce submitted his resignation in June 1964, just before Ziman's arrival. I suspect that he had been thinking of making a change for some time, and doubtless Ziman's appointment made it easier for him. He actually left in the summer of 1964.

At the time of his appointment, Ziman was on the staff of the University of Cambridge, and he took up his duties with the deliberate intention of building up a strong school of theoretical physics. In this he was well supported by the Steering Committee, which agreed to successive new staff appointments when he proposed them during the next few years. Apart from Frank, and Devonshire who was no longer doing any research, the only theoretician on the staff was Greenwood, appointed in 1962. The next few years saw the appointment of Lloyd (1964), Berry (originally as a Research Fellow, and appointed to an established post in 1966), Pollard (1966), Alcock (1968), Cottingham (1969), Gyorffy (1970), and Evans (1972). Of these, Lloyd left in 1969 to take up an appointment in his native Australia. The others are all still with us and active at the time of writing (1988). Ziman had a flair for picking bright young men. Of those mentioned above, only Cottingham, Gyorffy and Pollard were appointed direct to established posts. The others initially held junior positions; indeed Berry had just completed his PhD thesis at St. Andrews, and had not yet been awarded the degree. All have been very successful appointments, the subsequent careers of Berry and Gyorffy being particularly distinguished. In arranging for these appointments, Ziman was careful to preserve a balance between the different interests of the experimentalists already on the staff. Pollard, Alcock and Cottingham were all high-energy particle physicists before arriving, and have since continued in the same general field. Ziman himself was an authority on solid state physics: his book "The Principles of the Theory of Solids" appeared during his first year in Bristol. Lloyd had similar interests, and the two worked closely together. Gyorffy too worked on similar topics, although by the time of his arrival, Ziman's concern was showing signs of moving away from physics, as already mentioned. Berry was a physicist of exceptional mathematical ability and insight. He has produced notable contributions to a wide variety of subjects, from wave propagation to quantum theory to the mechanics of chaotic motion.

For close collaborators Ziman relied heavily on a succession of visitors and temporary post-doctoral research associates. He was a dedicated attender at conferences, and an indefatigable correspondent, so that he built up a world-wide network of personal connections with overseas universities. A memorandum written by him in 1969 lists 18 of them, and concludes "and so on". Lloyd, for example, was invited to Bristol as the result of a meeting with Ziman in Australia. The same memorandum lists 9 post-doctoral research assistants and 10 research students working with him in Bristol, and adds that the group is not expected to grow any larger. Since he had arrived only in 1964, this is a considerable achievement. They were financed either by their home university or home government, in the case of overseas people, or else from a series of research grants, in the case of British nationals. In the ten years from 1966 to 1976, Ziman collected a total of about £92,000 in grants, mainly from SRC, but with some contribution from the Ministry of Technology (Defence). This total, of course, included no element for apparatus or equipment. He contrived to arrange matters so that at least some of these grants were for research in a field of study specified in rather general terms. They provided funds under his personal control in such a way that he could use them to support research workers whose identity was not known at the time that the grant application was made. The publication record tells the same story of steady growth. The three years from 1964 to 1967 produced 16 titles from the group: from 1971 to 1974 the number was 76, not counting some 20 reviews of other people's publications from Ziman's pen. The subjects of the researches showed some changes. From being concerned almost entirely with crystalline solids, mainly metals, the interest extended to disordered, glassy solids, and to liquids. Greenwood had previously worked on what we now call disordered systems, and Evans was concerned almost entirely with liquids and liquid surfaces from the start of his appointment. This topic received a further boost from the arrival of Enderby in 1976, since he had a very active experimental group on liquid structure.

In addition to this shift of emphasis in physics, Ziman's interest in the sociology of science continued to grow during the same period. His book "The Social Dimension of Science" was published in 1969, and thereafter papers and articles on this kind of topic slowly displaced work on physics from his published output. He left Bristol in 1981, taking early retirement to do so, and took an honorary appointment at Imperial College, which enabled him to pursue his interests freely. By this time the theory group was sufficiently long established, sufficiently numerous, and included sufficient people of outstanding ability that it was able to continue happily. Subsequent developments will therefore be left to the writer of the next instalment.

(b) Cosmic Rays and Particle Physics.

The largest coherent research group in the Department in the immediate post-war years was that investigating nuclear and particle physics under the general direction of Professor Powell. The early history of the technique of using photographic plates to detect fast atomic particles has already been mentioned. One or two preliminary notes about the results were published in Nature in 1939/40, but more substantial accounts in P.R.S. were delayed until 1944. Tyndall missed no opportunity for pointing out the merits of the technique, and advocating its use. It was in fact used to provide numerical data for the atomic energy project, the source of the high energy particles being usually the Liverpool cyclotron. These measurements were "classified" and led to no published papers at the time. The Bristol 700kV Cockcroft generator, which had been used for some of the early work, had been dismantled to make room for one of the Admiralty sections which occupied parts of the building. Powell's 1943 memorandum on "The Development of Nuclear Research in Bristol" has already been mentioned. In it, he claimed that the re-building of the H.T. set could be done quickly, and would cost nothing, since the Admiralty were responsible for replacing it. It would provide excellent training for young research workers, since the photographic emulsion technique would be important "whether or not a cyclotron is constructed in Bristol".

Two years later, in December 1945, Tyndall wrote another memorandum on the same subject, clearly intended for some unspecified, but influential, committee. He says "... it is essential that the (Bristol) laboratory should be equipped with a machine for giving high energy particles ..." but goes on to claim that the re-erection of the old 700kV machine would not now be justified, since it would have too restricted a field of application. Higher voltages were needed. Perhaps it would be best, he said, to concentrate for the immediate future on the development of the photo-emulsion technique, rather than to duplicate machines that were already in existence, or were being built, elsewhere. However, an accelerator in Bristol should not be delayed for more than 2-3 years. The opinion of the committee would be welcomed. (It would be interesting to try to find out what the committee was, and what its opinion was!) By the end of 1946 it had been decided not to re-erect the H.T. set. The reasons for the decision are not recorded. The major components had been supplied in the first place by the firm of Metropolitan Vickers. In November 1946, a letter from Allibone at AEI. (the parent company then controlling Met. Vick.) suggested that they would like to buy back some of the parts. In the end they took back the seven high-voltage capacitors and the big transformer, for a total of £500. They originally offered £450, but Tyndall squeezed them a bit. The transformer was in doubtful condition, since it had been drained of oil during the war, to reduce the fire risk. By March 1947, the Department had agreed to drop its claim against the Admiralty (for £800) to pay for the re-building of the set, and had settled for £500 "compensation". Tyndall comments with some satisfaction "I think we have done quite well ".

Bristol never did get its accelerator. An alternative proposal by Gibbs and Fertel to build a pulsed linear accelerator that would give high voltages but low currents, using surplus war-time radio equipment, never came to fruition. The Department did make use of accelerators elsewhere, principally at Liverpool and Birmingham, and later at Harwell and CERN. What had not been foreseen was the great expansion of work using cosmic rays to provide the energetic particles. There are considerable advantages in carrying out such experiments at as high an altitude as possible, so as to avoid complications arising from the passage of the radiation through the atmosphere. Since the essential equipment was small and light, the photographic emulsion technique was ideally suited for such studies. Bristol, with its expertise in this technique, was therefore in a very strong position. Of the many publications which resulted, one in particular must be mentioned. In the issue of Nature for May 24th. 1947, there was a short article under the names of Lattes, Muirhead, Occhialini and Powell, (Note the alphabetical order!), which included pictures of particle tracks recorded in emulsions exposed on the Pic du Midi. It was this experiment which cleared up a mystery of some ten years standing. The term "meson" or "mesotron" had been in use for some time to describe particles with a mass intermediate between that of an electron and a proton. The mystery concerned the relation between the mesons observed experimentally in cloud chambers and the hypothetical meson of Yukawa theory. The assumption that the two were identical had been giving rise to difficulties, and the emulsion photographs showed conclusively that there existed, in fact, two kinds of meson of similar mass, now known as "pi" and "mu", the former decaying spontaneously to give rise to the latter. Two weeks earlier, but after the Bristol letter had been submitted for publication, a letter reaching similar conclusions, but on less firm evidence, had appeared from Perkins, at that time working at Imperial College. His plates had been exposed on an R.A.F. plane. The agreement of the two, quite independent, investigations, provided strong evidence for the reliability and precision of the photo-emulsion technique, as Powell was not slow to point out.

These events were the effective starting point for the flood of publications from the Powell group in the following years, which established Bristol as one of the leading centres for the study of high-energy particle physics, and which led up to the award of the Nobel prize for physics to Powell in 1950. The citation draws attention to his "purposeful" work over the years, devoted to the development and perfection of the emulsion technique, and to the discovery of phenomena associated with mesons. What it fails to mention is that, when the work on photo-emulsions was started, the whole body of accepted doctrine was that it would never be successful. Indeed, in later years, (e.g. in an article in "Science" in 1960) Powell wrote that he was unfamiliar with the literature at the time, and that if he had known of previous unsuccessful work, he would probably never have tried. In the same article he also says that he was singularly lucky in those first observations, in that he chose an emulsion (Ilford half-tone) which, as it happened, was particularly suitable, and also that he happened to hit on a good batch of plates.

In the period from 1948 (when Mott took over as Director, and Powell was made a professor) to 1969 (when Powell retired) about 350 publications of various kinds originated on the "fourth floor"- as the Powell empire was usually called. Something like three-quarters of them were concerned with experiments in which the source of the energetic particles was the cosmic radiation; the remainder had used a big machine in some laboratory other than Bristol. During this period there were considerable changes in the technique. The original experiments in 1939 had been done with standard commercial half-tone plates. These were far from perfect for the job, since they were not sensitive to either very fast or very light particles. After the war, development work on special, so-called nuclear emulsions was undertaken by both Ilford and Kodak. Powell gives credit to Occhialini, who had arrived in Bristol in 1945, for stimulating this work. These new emulsions had a higher concentration of silver bromide than normal, and were thicker, so giving a larger sensitive volume. This also meant that processing was more difficult, and took longer. The Ilford C2 emulsion (c.1946), the Kodak NT4 "electron sensitive" emulsion (c.1948), and the Ilford G5 (c.1949), each represented an improvement on its predecessors. They were not only more sensitive but also permitted more accurate measurements of the tracks, and elaborate statistical methods were evolved for extracting the maximum amount of reliable information from an exposed plate. The use of "stripped emulsions", which dispensed with the use of the supporting glass plate, clearly also represented an improvement.

It soon became obvious that great advantage would arise from the use of higher altitudes than could be reached on mountains. Perkins' early work had been done using aeroplanes, and at a later date Fowler obtained some very long exposures by arranging for plates to be carried on the newly-developed Comet aircraft during its proving flights to and from the Middle East. Such arrangements called for the co-operation of outside bodies, which could not always be regarded as certainly forthcoming. In any case, even greater altitudes were desirable. There remained balloons, eminently suited for use with photographic plates, but clearly not appropriate for cloud chambers and big magnets. It is ironic to recall that, in 1933, during his period of enthusiasm for nuclear research at Bristol, Skinner had dismissed without a second thought the use of cosmic rays as a source of energetic particles for just this reason. A great deal of effort at Bristol was therefore put into the development of balloons, and the combination of photographic plates and balloons proved to be a winner.

The first balloon flights, around 1949, used rubber meteorological balloons, some 4-6 feet in diameter, which were readily available and fairly cheap. A group of up to six would be used to obtain the necessary lift, and when one of them fortuitously burst, it would be arranged that the rest of them supported the load in approximately level flight for a considerable time, until another burst, and the load descended. However, rubber was not a suitable material for prolonged flights at high altitudes where it would be exposed to both low temperatures and intense ultra-violet light. Moreover, larger sizes were not available. The group therefore set to work to design and manufacture their own balloons out of polythene sheet. This was also readily available, being produced in large quantities for the packaging industry. Co-operation from the manufacturers, ICI, was willingly forthcoming, and the material proved to be eminently suitable. Although only 0.0015 inches thick, it was impermeable to hydrogen. It was unaffected by ultra-violet light and by the low temperatures that would be encountered. But, unlike rubber, it was for all practical purposes inextensible, so that the balloons had to be made with an opening at the bottom, and containing only a limited quantity of hydrogen at the top when launched. This circumstance was turned to advantage since, by suitably matching the total load to be carried, the volume of the fully distended balloon, and the quantity of hydrogen introduced before launching, it could be ensured that the balloon rose to a pre-determined height, and stayed there.

The space under the glass roof of the Royal Fort building, originally conceived as suitable only for storage, was pressed into service. A table was built running the whole of its length, to be used for welding together the panels of which a balloon was composed, using for the purpose a specially designed torch giving a controlled blast of hot air. Eventually, balloons were being made which, when fully inflated, were 30m. in diameter and 40m. long. The biggest had a total volume of half a million cubic feet. It should perhaps be pointed out that all this was done without any previous experience or outside help. The whole process of designing, manufacturing and launching the balloons had to be developed from nothing. (In fact it was learned later that similar developments were taking place in America, but as it was "classified" work, funded by the U.S. navy, it did not help the Bristolians.) It is unfortunate that no complete and authoritative account of this considerable achievement was ever published. There is only a rather brief note by Hans Heitler (Walter's brother), who was very much involved, in the Transactions of the Plastic Institute for April 1954.

As time went by and experience accumulated, the flight programmes became more ambitious. Larger volumes of emulsion were used, so as to collect more data from one flight: in the so-called G-stack, a 22-litre block of stripped emulsion was flown, weighing 83kg. Ever greater heights were aimed at: 120,000 ft. was discussed in 1959. There was greater complexity in the air-borne instrumentation, designed, for example, to ensure a rapid rise to a pre-determined height, a level flight for a pre-determined time, and a rapid descent. The total weight flown had risen to quantities of the order of a ton by 1959, and this, in turn, needed improved launching equipment. All this resulted in increasing costs, and called for additional man-power. To cope with such problems, it was arranged that several laboratories would co-operate in organising and carrying out one flight. Since the early 1950's the venue of some of the flights had been changed from England to the Mediterranean region. This had the scientific advantage that, by working at lower latitudes, the earth's magnetic field cut off the particles of lower energy, while leaving those in the higher energy ranges, which were of greater interest. There were also important incidental practical advantages. The density of commercial air traffic was high over southern England, and it was not often that the weather forecasts were sufficiently reliable to justify the launch of a balloon with confidence that it would not cross an air corridor. In addition the possibility of damage when it descended somewhere in a densely populated area had to be considered. (British Rail were not amused when one came down on an engine on one of their main lines). In the Mediterranean, the weather was more predictable, the density of air traffic was less, and the chance of damage being caused on descent was very small. The Italian navy and air force were very helpful about locating and retrieving balloons that came down in the sea. On all these counts, there was much to be said for flights over the Mediterranean, launched from Italy. There were also some flights over the mid-west of America, where the weather is also more predictable than in England. There were difficulties about obtaining "security clearance" for these operations, but eventually the restrictions were lifted. For good measure, according to two ex-Bristolians working there (Waddington and Friedlander) "the Americans have tracking and recovery well organised". In the end, it proved more convenient, and cheaper in real terms, to use balloons which, by this time, were available in U.S.A., and the last one was made in the Royal Fort about 1963.

An outstanding example of the international co-operation developed in this programme is provided by three flights over the Mediterranean in 1952-54. These involved 23 universities from 12 countries. The exposed emulsions, after processing at Bristol, were distributed for analysis among the participating departments. Since the track of one particle might well appear, in part, in the films held by more than one university, close and careful co-operation was needed in the analysis of the data. The result was a series of papers with multiple authorship, the record being set by one in which the list of 36 collaborating authors occupied the whole of the first page. This kind of international co-operation at a very practical level was just the kind of thing that appealed to Powell. But it did involve an enormous amount of time and effort, and others at Bristol were also heavily involved. Not least among these was John Davies, who, for many years, combined the duties of a research physicist with those of a kind of administrative officer for the whole group.

The construction in other laboratories of bigger and better accelerators, producing more numerous and more energetic particles, meant that the cosmic radiation became progressively less important as a source of fast particles. The machines could produce particles of known mass and fairly well-defined energy moving in a pre-determined direction, and, moreover, produce them in large numbers. At the same time, the development of spark-chambers, scintillation counters and Cerenkov counters, together with electronic circuitry of increasing complexity, meant that the photographic plate technique for detection was gradually superseded. The electronic detection devices could be used "on line" with the accelerators, and thus introduced the additional element of time into the analysis of nuclear events. The photo-emulsion was an integrating device: it recorded - with greater or less efficiency - all the events which took place within the sensitive volume, and the investigator later picked out those that were of interest. The electronic detectors were selective: it could be arranged that only one particular type of event was recorded, and all the rest were rejected. The particles were so numerous that even comparatively rare events could be recorded in sufficient numbers for statistical analysis to be meaningful. This was fine if you knew what you were looking for, but it would have been quite useless, even if it had been possible, in the early, exploratory phases of the work.

It was about the mid-1960's that Powell's group split into two parts - although there was a good deal of interaction between them. To oversimplify the position somewhat, there was a "cosmic ray group" using balloons and photographic emulsion techniques, and a "particle physics group" using big machines and electronic detection methods. It was a natural development that the former should move its centre of interest to the study of interactions at very high energies (1,000 - 100,000 GeV) - beyond the reach of the then existing machines - and to the characteristics of the primary cosmic radiation. They continued to use balloons to get high altitude exposures, and in 1966 a flight over Texas produced the first examples of "ultra heavy" cosmic ray particles, some with $Z>70$. It was in this connection that Plastic detectors, instead of photographic emulsions were first introduced. They have since found applications in many and diverse fields, but a description of this work, although done at Bristol, would take us beyond our period.

The leader in all this work was Peter Fowler. He had been a student at Bristol whose degree course was interrupted by war service, and was completed in 1948. As an undergraduate student he had helped with the fourth floor research work in his vacations, and when he finally left the Air Force, he was appointed to the staff immediately after completing his degree. Following a sabbatical year in 1957-58 spent at the University of Minnesota, he was offered a full professorship in the Department of Physics there. In the end the proposal fell through, since the authorities there raised some objection to his early health record. His departure would have been a serious loss to Bristol, but he stayed on, and was appointed to a Royal Society professorship in 1964, being elected F.R.S. in the same year. He had worked closely with Powell on the development of the photographic emulsion technique, and almost all of his published work was concerned with experiments on cosmic rays. In the early days, the interest was mainly in the identification of the particles which caused the tracks, and in the elucidation of the nuclear reactions involved. Later he was much more concerned with the characteristics of the primary cosmic radiation itself, and the cosmological implications of the results. He retired, nominally at least, in 1988.

People who worked with Fowler included Rodney Hillier, a Bristol graduate (BSc 1954, PhD 1959). He started by using the emulsion technique, but after submitting his thesis, he and Standing (a "visitor" on leave of absence from Winnipeg for one year) suggested the use of balloons to investigate gamma-radiation of extra-terrestrial origin. Powell, always willing to try out a promising new idea, approved. By using arrays of counters, the detector system could be made directional, i.e., a gamma-ray telescope. Work on these lines continued alongside Fowler's experiments on the primary cosmic radiation, and led to wider interests in other aspects of astronomy, which continued to develop in subsequent years. He gave lecture courses on both physics and astronomy to undergraduate classes, and was - and still is - in much demand for Extra-Mural classes on these subjects. He also continued to play an active role in research.

The leaders of the particle physics section were Martin Gibson, Don Perkins and, later, John Malos. Gibson came to Bristol from Cambridge in 1949, spent the years

1953-57 as a lecturer in Belfast, 1959/60 in charge of the emulsion group at CERN, and from 1960 until his retirement in 1984, he was back in Bristol. At various times he had collaborated with people at Birmingham, CERN. and Harwell, using the photographic emulsion technique in the early stages, and changing later to counter methods, sometimes in collaboration with Malos. John Malos, who worked almost entirely with accelerators, was an Australian whose early training had been in engineering, but who switched to physics before graduating. He first made contact with the Bristol group while collaborating on one of the Mediterranean flights in 1961. In the same year, Powell arranged for him to come to Bristol, paid from a D.S.I.R. research grant. He was given a "temporary" one-year staff appointment in the following year, and is still here at the time of writing in 1988. His interest, expertise and enthusiasm for electronic methods had a considerable influence on the work of the Powell group, and, indeed, on the work of the Department as a whole. His work on the development of spark chamber methods of particle detection was particularly important.

Don Perkins arrived from Imperial College in 1949, holding a Royal Society McKinnon studentship. Around 1951 he was taken on to the staff for one year - like Malos. Powell later wrote to the Registrar that he would like the appointment to be extended for a second year, "but it should be made clear that it is unlikely to be further extended". In fact, he stayed until 1965, when he left to take up a professorship at Oxford. Initially he worked with Powell and Fowler on the cosmic-ray experiments, but in 1963 he spent some months at CERN, using a bubble-chamber. These devices were a fairly recent development, and combined some of the advantages of the emulsion and counter techniques. When Perkins returned, arrangements were put in hand for the formation of a bubble chamber group in Bristol. There was never any suggestion that we should build a chamber - they were very expensive - but the idea was that we would analyse the photographs taken on equipment elsewhere, particularly at CERN. The apparatus for doing this work had already been ordered when Perkins left for Oxford. Much of it was transferred to Oxford with him, and the other resources destined for the work were diverted into counters and associated equipment.

Other members of the group must, perforce, have an even briefer mention. Waddington (BSc Bristol 1952, PhD 1955) worked with Fowler on the composition of the cosmic radiation, and continued research on related matters until leaving for an appointment in the University of Minnesota in 1962 - the same post that Fowler was unable to take up. Occhialini, already mentioned, arrived in 1945, having previously worked with Blackett. He was a considerable driving force during the early work on cosmic rays, and left for a post on Brussels in 1948. Lattes (1946 to 1947) became a professor in the University at Berkeley in California. Muirhead (Bristol BSc 1946, PhD 1951) joined the staff of the University of Liverpool in 1951, staying until he retired. Others were Menon, Camerini, Friedlander and Prowse. In addition to those holding university appointments, sometimes "temporary", there were varying numbers of people described as "research students and visitors", so that the total number of research workers in the group at any time fluctuated between 10 and 20. Some were PhD candidates, usually on D.S.I.R grants. Many - sometimes as many as half - were from overseas, either as PhD candidates or as visitors, being in either case financed by their university or by their government at home. The "visitors" were very welcome to

Powell. They tended to be older and more experienced than the PhD candidates, and the fact that they were financially self-supporting was very useful.

To assist with the time-consuming operation of searching the exposed photographic plates for interesting "events", a number of assistants were employed. They were officially known as "scanners", but as they were almost all young girls, they soon became known as "Cecil's Beauty Chorus". No previous experience was called for - indeed, none was possible - but after a short period of on-the-job training they rapidly became very proficient. Their job was to note the co-ordinates of any interesting "event", which was subsequently examined in detail by a physicist. There is no doubt that the research work would not have been possible without them, and Powell always made a point of recording, on every published photograph, the name of the girl who had found the event. The number of scanners fluctuated considerably, and their names even more so, as new girls came and others departed, but at the peak of the activity there were about twenty of them. Almost the whole of the group, staff, students, scanners, technicians and secretaries, adding up to something in excess of 40 people, were accommodated on the fourth floor of the building. One result of this overcrowding - it could hardly be called less - was to develop a notable *esprit-de-corps* within the group. They always held a firework party on the roof of the tower on the fifth of November. However, on the debit side, this closeness, combined with their physical isolation on the top floor of the building, meant that they did not interact much with the rest of the Department.

All of this adds up to the fact that the Powell empire grew, as a result of Powell's efforts and of its own success, until it constituted a considerable fraction of the research work of the whole Department. For one period its finances were kept separate from the rest, and there was at one time a suggestion that it should become a completely separate entity. The majority opinion, however, was against any such sub-division, as it has been against other similar suggestions at other times, and the idea was dropped. Within the group there was competition for resources, both for money and equipment, and for the services of technicians. At one time, around 1964/5, this almost broke out into open warfare, when Malos felt that he was not getting sufficient support for his growing section on electronic methods. However, Powell, always completely in control in his own gentle way, was able to defuse the situation with characteristic diplomacy and tact.

The last paper describing the results of original research, on which Powell's name appears as a co-author, was published in 1956, but his full publication list includes more than 60 subsequent items. To begin with, these were on topics in, or related to, physics - summary accounts of earlier work, review articles, invited lectures and the like - but after about 1962, articles on the politics of international science and on educational topics predominated. This was a reflection of his activities in general. Following his Nobel prize in 1950, he was much in demand for such contributions. From 1961-63 he was Chairman of the Science Policy Committee of CERN; from 1965-68 Chairman of the SRC Nuclear Physics Board; from 1956-69 President of the World Federation of Scientific Workers; and from 1967-69 Chairman of the Pugwash Conference, having been effectively chairman for some time before this, deputising for Bertrand Russell, who usually missed the conferences. Nor did all this prevent him playing a full role in University affairs. He was Dean of the Faculty from 1959-64, Pro-Vice-Chancellor from 1964-67, and Director of the laboratory from 1964 until his retirement in 1969. In spite of all these activities, he was still very much involved in the work of his research group. From 1958 to 1965 for example, there were regular "programme meetings" with the section leaders on the fourth floor, which discussed and agreed upon not only general policy for future research, but also detailed arrangements for the implementation of the decisions taken.

There is no doubt that he was a man of remarkable talents.

(c) Experimental Solid State Physics.

Apart from the work of Potter and Sucksmith on magnetism, which had a long history going back to the 1920's, much of the experimental research in the Department on various topics in solid state physics was stimulated, either directly or indirectly, by the theoretical work of Mott and his collaborators. But, unlike the theory, the experiments continued and proliferated after Mott left in 1954. The existence of pieces of laboratory equipment, either home-made at the cost of considerable trouble, or purchased at considerable expense, tends to give a greater element of continuity to an experimental group. For example, work on the photographic process, under Mitchell, and my own experiments on the mechanical properties of metals, both directly due to Mott's interest in such matters, both continued long after his departure. The setting up of the MSc course on the Physics of Materials, and the necessity of having available members of staff to run it, provided another element of continuity, with diversity of detail. This section gives an account, more or less chronological, of these matters.

We begin with Jackson, who arrived in Bristol in 1927. His first researches were concerned with magnetic susceptibility measurements on rare-earth salts, often in the form of single crystals, which he grew from solution in beakers of about 1 c.c. capacity. His apparatus - home-made, of course - was similar to that developed by Sucksmith for work on ferro-magnetics. He remained in Bristol during the war years, and carried a substantial part of the load of undergraduate teaching during that period. When active research was resumed after the war, he became interested in super-fluidity, and particularly in the helium film phenomena. He also had an occasional research student working with him; previously it had been a one-man show. One of the first was a Bristol graduate, E.J. Burge, who, after a subsequent period reading theology at Oxford, returned to physics at Bristol, and eventually became professor at Chelsea College. In 1948 Eric Mendoza arrived as an additional member of staff interested in low-temperature physics amongst other things, and stayed until he left to go to Manchester in 1957, subsequently moving to Bangor and to Haifa. Jackson himself left in 1956 to go to the Royal Military College of Science in Ontario. In the same year he was awarded the Duddell Medal of the Institute of Physics. He was replaced by Chambers, and the group was further enlarged by the arrival of Gugan and Parsons in 1960, Milne in 1961, and Priestley in 1966. There was a corresponding diversification of interests to include a variety of phenomena involving solids at low temperatures, including Fermi surface studies, the de Haas-van Alphen effect, resistivity and magneto-resistivity, specific heats etc. The number of research students in the group at any one time also increased, as might be expected, and was usually about five or six. In the period from 1958 to 1975, the group produced an average of about four papers a year. If this appears rather small for the size of the group, it should be remarked that the staff involved were all very active in the teaching and administrative work of the Department: there was no tendency towards empire

building, by taking on large numbers of research students and visitors. Indeed Milne eventually devoted all his considerable energies to teaching activities, while maintaining a spectators interest in the work of the others.
Apart from Jackson, and Potter who did no research after the war, I was the only experimentalist whose activities bridged the war years. Pre-war work on the electrical properties of bismuth alloys related directly to a theory of Mott and Jones. Post war experiments on the mechanical properties of metals arose from Mott's interest in dislocation theory. A slow but steady trickle of research students worked with me on problems of creep and work-hardening: the latter led to a more substantial effort on hardening under alternating cycles of stress, and thence to fatigue in general. We usually produced a couple of papers each year until 1966. A year spent in America at that time interrupted my direct involvement with experiments in Bristol, and I never took it up again on my return.

David Gibbs arrived in the Department in 1945, straight from a war-time appointment with the Admiralty Signal School. Apart from one year sabbatical leave with Bell Labs, arranged by Pryce, he stayed until retirement in 1983. His output of published work was modest by some people's standards, but he usually had one graduate student working with him, e.g. on dielectric materials or ferroelectrics. However this sentence says less about Gibbs, and more about the unreasonableness of basing judgements on such criteria. He was, in fact, one of the most valuable members of the staff. He made many valuable contributions to the development of the laboratory work in the undergraduate teaching course. He had one of the most acutely critical minds in the Department, and gave unstintingly of his time to help anybody who sought his assistance, whether undergraduate, post-graduate or staff member. Nor were members of the Physics Department the only beneficiaries. He lectured on acoustics to music students, on optics to architects, and co-operated with several members of staff from biological and medical departments on problems of instrumentation and measurement.

J.W.(Jack) Mitchell was one of the people that Mott recruited in 1945 as a result of his war service at Fort Halstead. He was a New Zealander who had previously spent three years at Oxford and two years school-teaching. Originally a physical chemist, he became heavily involved in problems of the photographic process and related matters concerning ionic crystals. He ran a research group of half a dozen students. It was said of him that "he works with an unusually high concentration of energy" and he expected his research students to do the same. This was to put it mildly: one of the students put it more bluntly by describing him as a slave-driver. He was awarded the C.V. Boys prize of the Institute of Physics in 1955; this is given for "distinguished work in experimental physics which is still in progress". In the following year he was elected to the Royal Society. He eventually left in 1959 to take up a professorship in the University of Virginia at Charlottesville, where he stayed for the rest of his working life. Among his more eminent students we may mention Douglas Keith who held junior staff appointments here for five years, and left in 1957 to take a job in Bell Labs, where he did notable work on polymeric materials. Perhaps the most distinguished, however, was E.W.J.(Bill) Mitchell - no relation. He was seconded to Bristol by the Metropolitan-Vickers Research Labs in Manchester to take a PhD. On leaving Bristol he was appointed to the physics staff of the University of Reading. His distinguished subsequent career involved professorships at Reading and Oxford, and the chairmanship of SERC. He became very influential in the politics of physics research, and received a Knighthood in 1991.

Kay was appointed as an assistant lecturer in 1947, with previous experience at Manchester and Cambridge in the field of X-ray structure determinations. He continued work on this and similar topics until his retirement in 1981. He usually had a small group of PhD students, and their researches were concerned mainly with ferro-electric, piezo-electric, and electro-strictive materials - the same general area as interested Devonshire in his theoretical work. He was also very keen on the physics of sailing boats, particularly the historical aspects.

Nye arrived in Bristol in 1953. He had worked with Orowan in Cambridge on problems related to the plastic deformation of metals, and was recruited as part of the policy of strengthening this field of research in Bristol. It did not work out quite that way. Mott left in the following year, and Nye's main interest had already shifted to the plastic deformation of ice. He continued to make important contributions to glaciology until his retirement in 1988. For a lot of this time he was almost a one-man research group, with occasional research students who rarely numbered as many as two in any one year. Around 1965 there was talk of the expansion of the departmental interests into the subject of rock deformation, in collaboration with the departments of geology and civil engineering. Both Nye and Frank were interested, but the plans never matured. In 1969 Nye was joined by Walford, who rapidly developed similar interests, and the research students and "visitors" thereafter became rather more numerous. Although never very large, the group has made important contributions to its field of study, both by devising new experimental techniques in the laboratory, and by using them in the field. There have been expeditions to Norway and Antarctica, for example, usually in collaboration with other institutions. On one of these expeditions he set up a camera on a fixed, rocky point and photographed a glacier at regular intervals of a few hours. On returning home, these pictures were transferred to cine film, which, on being projected, gave a time-lapse record of the movement of the glacier surface. Although perhaps less important than his more "scientific" work, the film is said to be most impressive. In 1962, one of the glaciers in Antarctica was officially named "Nye Glacier". In 1966 he was elected president of the Glaciological Society, a job which involved a good deal of participation in international committees. In 1970 he was promoted to a chair of physics. His previous appointments had always been, exceptionally, as a lecturer or reader in "Experimental Physics". The reasons for this are lost in the mists of antiquity, but it was never very appropriate. Of all the experimentalists in the Department, he probably made more significant contributions to the theory of his subject than most. His book "The Physical Properties of Crystals" (1956) became, and still is, the standard text on theoretical aspects. When, therefore, it was suggested that he should become a Professor of Experimental Physics, he demurred, and asked that it should be just Professor of Physics. He was elected to the Royal Society in 1976.

Forty was a Bristol graduate who took a PhD here in 1953, with Frank as his supervisor. He then spent three years as an instructor with the R.A.F. and two years at Tube Investments Research Laboratories, returning to Bristol as a lecturer in 1958. He remained until leaving for a professorial post at Warwick in 1965. He had an active group of students who were concerned with a variety of subjects, including crystal growth, stress corrosion, light sensitive crystals, and ultra-violet microscopy. One thing led to another in this sequence, but the early influence of Frank can be detected. At Warwick he gradually became increasingly involved in administration and university politics; for a period he was Pro-Vice-Chancellor, for example. Eventually he accepted the post of Principal of the University of Stirling.

Frank was also responsible, in a way, for the accession of Andrew Lang to the staff. After research experience at Cambridge (England) he went to Harvard, and, while there, spent a profitable three months leave of absence at Bristol. While he was here, he was considered as a candidate for a permanent appointment. He was anxious to work in the department which housed Charles Frank, and Frank in turn was pleased to give the appointment his support. He was due to start as a lecturer in August, 1959, but - characteristically - he asked for the starting date to be postponed for a term to permit him "to complete some of many research projects already started at Harvard". His many research projects continued to occupy him full time - or more than full time - up to the date of his nominal retirement in 1987, and beyond. His researches were almost all concerned with the investigation of defects in crystalline materials, often diamond, using mainly X-ray techniques, but also cathodoluminescence and other phenomena. The methods used were ingenious and delicate, and often involved the design and construction of novel pieces of equipment. One, at least, was later sold commercially as a "Lang Camera" for X-rays. At one point he was criticised - if that is the right word - for being more interested in the perfection of an experimental technique than in the results obtained by using it. Nevertheless he, too, was given the C.V. Boys prize of the Institute of Physics (in 1964) which, as quoted above, is given for "distinguished work in experimental physics". He attracted a continuous stream of post-doctoral research workers from overseas, in addition to looking after a group of PhD candidates. His visitors came notably from Japan, but also from Russia, Czechoslovakia, and Germany. He was elected to the Royal Society in 1975.

Although Pryce made no serious attempt to build up a research group in theoretical physics, he did arrange for three staff appointments for experimentalists working on subjects akin to his own interests. Llewellyn was appointed to a lectureship in 1957 having previously spent some time in America, following his PhD at Oxford. He was interested in paramagnetic resonance phenomena associated with impurity ions in crystals. He left in 1962 to take an appointment with Varian Associates, in California, eventually becoming Research Director there. Baker arrived in 1960. He was a physical chemist, also interested in paramagnetic resonance, and expert in the use of millimetre-wave techniques. When Pryce left in 1964, and after an attempt at collaboration with our Chemistry department had run into difficulties, it became clear that there was little use for his particular expertise in Bristol, and he went to Manchester University. Gill was appointed in 1962 to the post vacated by Llewellyn. He had previously worked at Jodrell Bank, and at R.R.E. Malvern, and wanted to move into a job which involved teaching as well as research. The transition proved to be successful, and he has remained in the Department ever since. His research continued to be on various aspects of radio-frequency spectroscopy, particularly spin-lattice relaxation phenomena, but in recent years he has changed (very successfully) to a quite different field - that of charge-density waves in solids.

Ashbee was appointed in 1964, with the title of Lecturer in Metallurgy and Ceramics. It was intended that he should play a major role in the MSc course in the Physics of Materials which was starting at that time. This he did with great energy and success, and at the same time contrived to do a considerable amount of research, and to supervise several PhD candidates each year. He also attended numerous international conferences and spent short periods working by invitation at industrial laboratories, e.g. in America. As a result of this, and of his growing reputation, he received a succession of offers of jobs elsewhere and the University was persuaded to promote him to a readership in 1970. He eventually succumbed to the temptations of a senior appointment in the University of Tennessee in Knoxville which had ample resources and scope for his innovative mind - to say nothing of the splendid title of the Ivan Racheff Chair of Excellence. He left Bristol in 1986.

Hart provides an excellent example of the benefits of the departmental policy of "encouraging merit where we find it". He obtained a BSc and PhD at Bristol, working for the latter with Lang, who started him along the path of precision X-ray measurements. After two years at Cornell, he returned to Bristol as a Research Associate in 1965. While at Cornell he had met and worked with Bonse, who came from the University of Munster, and they continued to collaborate when each had returned home. Taking advantage of the availability of large, almost perfect single crystals of silicon, produced for the electronics industry, he made notable advances in the field of X-ray optics. His X-ray interferometer, for example, was a device in which all of the essential components were carved out of a single crystal of silicon. This found numerous applications in metrology, and permitted a precision re-determination of the value of Avogadro's number, (or, more precisely, as I am reminded, should have permitted - if the NPL had ever completed their part of the job.) He too, like Lang, was awarded the C.V. Boys prize of the Institute of Physics (in 1971). He was well described by a referee, on one occasion, as characterised by "rapid action without fuss, and a strict concentration of his activities on what is essential". In 1975, by which time his quality had been recognised by a promotion to a readership, his name was suggested by the Vice-Chancellor (Merrison) as a possible scientific advisor on the Central Policy Review Staff of the Cabinet Office, popularly known as the Think Tank. He was duly appointed, and spent two years there on secondment. In fact, he never returned to Bristol, moving on to a chair at Kings College, London, in 1977, and subsequently to Manchester.

Finally in this catalogue, Steeds arrived in 1967, having spent six years in Cambridge working in Hirsch's laboratory. This had generated an interest in electron microscopy, and he continued to make important contributions, both experimental and theoretical, to this subject. In addition, he became responsible for the purchase and maintenance of the electron microscopes and related pieces of expensive equipment, which the Department acquired in subsequent years. Always a man of considerable initiative and drive, he made, and is continuing to make, substantial contributions to the work of the Department on a broader field. He was elected to a Fellowship of the Royal Society in 1988.

(d) Applied Optics.

It is probable that anyone who has known the Department for no more than, say thirty years, will be surprised to find a substantial section devoted to the subject of applied optics. But it seems worth while to give an account of these almost-forgotten events, since they involved a considerable amount of effort at the time, and there exists no other coherent record. A good deal of the relevant paper-work has been preserved, and the details are of some interest in themselves. In addition, had events taken a different turn, the outcome might have affected the present character of the Department quite a lot.

Most of the story centres around C.R. Burch, a most remarkable man whose career is well described in the Royal Society "Biographical Memoir" (Vol. 30, 1984). His early scientific work had been done with Metropolitan Vickers, in Manchester, but for personal reasons he left them and went to Imperial College in 1933, to work on problems in optics. He was invited to come to Bristol by Tyndall in 1935, for reasons that are quite unknown. There had been no previous experience of work on optics at Bristol, and Burch's intention was to work on the theory and practice of the use of non-spherical surfaces in optical instruments. His appointment at Imperial was approaching its end, and it may be that Tizard had brought him to Tyndall's attention as a promising scientist looking for a job. Tizard, a person with a penchant for organising things, was Rector of Imperial College, and had been one of the guests of honour at the Colston Society dinner in 1930, at which Tyndall had proposed the toasts; or it may be that Tyndall just knew a good man when he saw one. Burch was originally appointed as a Research Associate, and later (1944) as a Research Fellow. In 1948, he was elected to a Warren Research Fellowship of the Royal Society, (largely at the instigation of Tizard), a post which he held until his retirement in 1966. It was never intended that he should take any part in the formal teaching in the Department, and he never did.

He began by completing the task of re-figuring a 36" telescope objective, which he had started while at Imperial, and did some other work on both the theory and practice of this kind of problem. In the course of this work he made contact with Linfoot, a lecturer in Pure Mathematics, who developed an interest in Burch's work, and became a kind of pupil-apprentice. In March 1941, Burch and Linfoot met some members of the Photographic Research Committee - a war-time organisation under the aegis of the Ministry of Aircraft Production. It appears that Tyndall had been instrumental in bringing about the meeting, which was to discuss the possibility of developing a Schmidt camera. By the use of a concave mirror instead of a lens, and a slightly figured, almost plane, plate to correct some of the image errors, it was possible to achieve both high resolution and a high light-gathering power, together with a flat field for good measure. It was to be used for high-altitude aerial photography. Also involved was Dr E.R. Davies, of the Kodak Research Laboratories, where an experimental camera on these lines had already been built. It was agreed that Burch and Linfoot should do further work on the optical system, while the Kodak Laboratories should be responsible for the mechanical parts. Financial support from M.A.P. was arranged at the end of 1941.

The work proceeded for three years, but the story is not a happy one. Both Burch and Linfoot had other work to do. Burch was helping the Admiralty Signals Establishment with the re-figuring of an air camera lens. (A Section of that organisation occupied the second floor of the Wills Laboratory at the time.) Linfoot had his normal teaching duties in the Maths department. Nevertheless, the optical parts of the proposed camera were ready before the mechanical parts from Kodak. It appears that nobody, either at Kodak or at the Ministry, thought that the project had very high priority, since it was believed unlikely that it could become operational in time to be of use in the war. By January 1944, Burch seems to have dropped out completely; there was little love lost between him and Linfoot by this stage. Linfoot was aggrieved that his work was not appreciated, and that his instrument was being condemned for failing to do things that it was never intended to do. There seems to have been a singular lack of co-ordination and direction about the whole business. Tyndall was apprehensive that the M.A.P. committee might stop the work completely, and he was anxious that work on similar lines should continue in Bristol after the end of the hostilities. His fears were justified, and in October 1945 M.A.P. were transferring responsibility for the Schmidt system to D.S.I.R. This, presumably, meant NPL, although enquiries there in 1982 produced no record of any such work having been done. The project just seems to have expired, and the M.A.P. contract with Bristol was formally terminated on 31/12/1945. (In May 1947, some of the apparatus that had been used and which, it was said, had already been modified and incorporated into other equipment, was sold by M.A.P. to the Department for the nominal sum of £5. They had asked for £10, but Tyndall beat them down.)

However, research at Bristol in the same field did indeed carry on. Linfoot was still involved, but the most active worker was Dr Dorothy Hawkins, a mature student with private means who had joined the group in 1943. Linfoot left in 1948 to take up an appointment as Assistant Director of the Cambridge Observatory, where he remained until retirement. But the group carried on under Dr Hawkins, with a succession of PhD students and - most important - Bill Lewis, a very able and versatile technician. In May 1950, an application was made to the Paul Fund of the Royal Society for financial support for a project to construct a meteor camera on the Schmidt principle. The application was made by Mott who, by that time, was Director of the Laboratory, but it was clearly inspired and written by Dr Hawkins. In fact, it states that a prototype is already being constructed by her "after considerable theoretical research" and it is clear from the application that a good deal of experimental and development work had also already been done. The construction of a camera for photographing very faint and fast-moving objects would have been a natural development of the work done for M.A.P., but our records contain no indication of whether any stimulus for the work came from outside the Department.

The initial estimate was that the project would take two years, and the request was for a grant of £4,000. Professor L.C. Martin, of Imperial College, and Dr Linfoot, by this time at Cambridge, were appointed as assessors, and Burch was willing to act as a "consultant". The initial comment of the assessors was that the project needed the services of another experimental research worker and another mechanic; that an additional £5,760 would be needed for the first year's work; and that the whole project would take at least three years and would probably involve another £15,000. Nevertheless, they approved the application as a good start. In the end, the cost was £20,000, and the work extended over six and a half years. The second research worker was not appointed, for reasons that are not clear, and Dr Hawkins carried through the whole project, assisted by a succession of research students and one (later two) mechanics. The assessors paid visits to the Department at six-monthly intervals, and their reports to the Paul Fund Committee always speak favourably of the progress made, and particularly of the work of Dr Hawkins "whose organising ability is clearly exceptional". (She was, I think, also a City Councillor and a JP.)

The design called for the development of several novel construction methods. The meniscus lenses, two in each camera, were 19" in diameter, about an inch thick, and nearly hemispherical in shape. To have carved them out of the solid would have involved starting from a cylindrical block 19" in diameter and about 9" thick - a formidable task. The technician associated with the work, Bill Lewis, suggested starting with a glass plate that was a little more than an inch thick, putting it in a furnace and allowing it to sag under its own weight into a mould made of graphite or ceramic. No commercial organisation was prepared even to consider the proposition, so, having obtained a suitably large furnace, the technique was developed in the Department. The resulting rough blanks then had to be machined to shape, and ground and polished to the required accuracy and concentricity. With such sharply curved surfaces this again required the development of completely new techniques. The same bending process was used to form the main mirror, which was 28" in diameter for the largest camera built. There were many other problems in the construction of both the optical components and of the mountings for them, and it would be no exaggeration to say that much of the success of the whole project was due to the ingenuity and skill of Bill Lewis.

The programme developed along the lines of producing first a small model using plate glass, in order to become acquainted with the unforeseen difficulties, then a 2/3 scale pilot model using optical glass, and finally the full-scale model. After two years from the start, it became clear that the whole business was going to take much longer than had been originally estimated. But the assessors recommended that the grant be extended, since so much had already been achieved, and the promise for the future remained good. The proposal was supported by an independent report from Dr J. Guild, of NPL, and the work proceeded. However, in May 1953, Dr Hawkins was taken ill with what was described as a nervous breakdown, and in the end had to give up work completely. In view of the key role that she had played, this could have been a disaster, but the remaining members of the group, PhD students and technicians, agreed to carry on. Ray Cooper, and later
Bradford, took the lead, with some help on the administrative side from Dr Piper, who was on the point of retiring. On this basis the work progressed satisfactorily. The small plate-glass model had been finished in 1952: the 2/3 scale pilot model was completed in 1954, and the full scale model in January 1957. Dr Bradford, who was largely responsible for the later stages, was on secondment from the University of Manchester, and all three cameras went there when he returned, the two larger ones to the Department of Radio-astronomy for use in meteor research. As far as the design and construction of a unique instrument is concerned, the whole can be regarded as a rather remarkable success story. Whether the subsequent usefulness of the device in scientific research justified the effort that went into its production, I am not in a position to judge.

In 1942, Burch had taken on as a research student J.W. Bates, who had just graduated at Bristol, and the two worked together for many years on problems in optics. In 1947, for example, they visited Grubb Parsons in Newcastle, to assist in the testing of the recently completed 74" mirror for the Pretoria telescope. Bates was a most valuable member of the Department in many ways, not only in research and teaching, but also in what would now be called the pastoral care of students. He eventually put in a PhD thesis in 1948, and stayed with us as a member of staff until 1961, when he left to take an industrial post, and later went to the University of Aberdeen. Most of his work with Burch was concerned with the interferometric testing of optical components, and with the design and production of a reflecting microscope. Burch had been experimenting with instruments of this kind for some years, and this eventually grew into a major development and production enterprise. The story of how this came about is both interesting and instructive, and, between our departmental records and the archives of the Nuffield Foundation, is very fully documented. I have thus been able to produce a very detailed account, but since this would be quite out of proportion if included here, I have made it into a separate paper. The following paragraphs give a shortened version.

The advantage of a reflecting microscope is that it is completely achromatic for all wave-lengths, and can thus be adjusted using visible light, and then used in either the ultra-violet or the infra-red. It also has a large working distance between the specimen and the optical parts, which makes it invaluable for certain applications, such as the examination of thick or tilted specimens, or for working with material not at room temperature. The disadvantage is that at least one, and preferably both, of the mirrors involved have to be aspherical. In June 1947 Burch wrote a proposal for further research and - more important - for development work on the production of aspherical surfaces, aimed at manufacture on a commercial scale. No optical firm could be found that was interested and so, *faute de mieux*, Burch and Tyndall agreed that the job should be tackled in the Physics Department. Financial support was promised from the Nuffield Foundation. But before the formal application was submitted to them in its final form, Burch changed his mind about the development and production element, and deliberately went away from Bristol so that he would not be available to sign the letter in time. Tyndall, in good faith, forged his signature, and the application was successful. In view of all the previous discussions, Burch then felt obliged to proceed with the project, which he did with great energy. But the incident soured his memory of his relations with Tyndall for the rest of his life. The Nuffield grant was originally for £18,000, spread over five years, and included £6,000 to enable the University to subsidise a small local engineering firm, Willcocks of Clevedon, to help with the work. In addition to Burch, Bates was heavily involved, and two of our technical staff, F. Bannister and V. McGregor, were employed almost full-time. There was also a succession of PhD candidates, attached to either Burch or Bates, the most distinguished being K. Keohane, who was subsequently very active in the field of physics education. Because of the considerable demands on staff time made by a PhD student, it is doubtful whether the presence of these students made a net contribution to the project.

The original proposal was to design and build a batch of ten microscopes for sale. Tentative enquiries had been received from some 40 potential customers. However, progress was slow. It proved impossible to train the average technician to carry out the highly skilled final figuring operations on the aspherical surfaces. Thus this tedious and very time-consuming operation had to be carried out by either Burch or Bates in person, at the same time as they were designing and making improvements to the machinery which would have the effect, in the end, of relieving them of the task. Many other unforeseen difficulties slowed down progress very considerably. The work had begun in the second half of 1948, and the original hope was to complete the batch of ten by the end of 1950. In fact the first standard model was not finished until May 1951, and No. 7 was completed in 1954. No. 2 had been sent to the NPL optics division for assessment, and although the optical performance was highly praised, there were serious criticisms of the mechanical design, particularly from the point of view of the convenience of the user. But it was probably not this, but rather the long delays in delivery which caused the list of potential customers to evaporate to the point at which it was difficult to find a buyer for No. 7. The rest of the projected batch were never built. The total disbursements on the project by the Nuffield Foundation amounted to about £25,000, and the work was effectively subsidised by the University (Bates' salary and overheads), by the Royal Society (Burch's salary), and by Willcocks, who must have lost money on the exercise. The total output was seven microscopes, and a decision not to proceed any further. This can hardly be called a success, and, with hindsight, we can see that the project was probably foredoomed to failure from the outset. Possible causes for this are discussed in the separate paper already mentioned. Perhaps the most permanent result is the existence in the Optics section of the Science Museum in South Kensington, of Burch's original polishing machine, sitting in isolated glory in its own large glass case, and looking rather out of place among the other shiny brass optical instruments. I am told that there is also one of the microscopes in the Museum of the History of Science, at Oxford, (? or the Museum of the Royal Microscopical Society ?) but I cannot vouch for this.

The papers that have been preserved give a tantalising glimpse of a third episode in the field of applied optics which overlapped in time with both the Schmidt camera and the reflecting microscope. It appears that on April 12th., 1945, H.H. Plaskett, of the University Observatory at Oxford, had visited Bristol to talk to Burch and Linfoot about the instrumental side of astronomy. This was possibly connected with the Royal Society exercise which had produced the "Report on the needs of Research in Fundamental Science after the war" a few months earlier (see p.15). This report includes the statement that "Council has appointed committees to consider further the special needs of astronomy, ...", and Plaskett may have been engaged on this matter. A letter from him to Tyndall, written two days after his visit, makes out a case for the setting up of an Institute of Applied Optics (his capitals!) associated with the Wills Laboratory in Bristol. Its functions were to be research on, and development of, optical instruments, (particularly, in view of his own interests, of astronomical instruments); the training of new recruits to the discipline; and the actual production, on a limited scale, of useful instruments. The second of these objectives required close association with a university, while the first suggested the choice of Bristol since, in Plaskett's view, we "had a corner in the optical talent in the country" i.e. Burch, Linfoot and Bates. He added, rather as an afterthought, that he expected that the work would not be confined to astronomical applications. The proposal was to be put to the Committee on the Future Needs of Astronomy - doubtless the Royal Society committee mentioned above - and the letter implies that Tyndall had already indicated that he was not unsympathetic to the idea. No copy of Tyndall's reply exists in our files, but there is another letter to him from Spencer-Jones, the Astronomer Royal, which is dated 15/7/46, i.e. more than a year later. This says that the writer is pleased to hear that the Vice-Chancellor of Bristol (at that time Philip Morris in his first year) would like to see such an institute set up. He adds that it could, for example, be of great assistance in the design of the 100" telescope, expenditure on which had only just been approved by the Chancellor of the Exchequer. He wished to meet Tyndall to discuss the matter in more detail.

The topic next surfaces a year later again, in August 1947, in a letter from Tyndall to Spencer-Jones, setting out "what our reactions would be to a proposal, if it were made, that one or more 100" telescopes should be developed at Bristol". The matter had been brought to his attention as a result of a report from Burch about a meeting of the Newton Observatory Committee, and he had gone so far as to have had "several discussions" with Mott and the Vice-Chancellor. (Tyndall was confident that Mott would succeed him as Director when he retired.) The matter is discussed in the letter at considerable length, and the following "tentative" conclusions reached:- that it would be unwise for the University to be involved in the design and production of the proposed 100" telescope; that - with less confidence - it might also be unwise to establish the proposed Institute of Applied Optics at Bristol; and that the University saw its best contribution as providing facilities for Burch to continues his work on microscopes, "about which he is very enthusiastic".

The case was well argued, but the Astronomer Royal was not convinced about the Institute of Applied Optics. He thought that the work likely to be done by Burch on reflecting microscopes, by Bates on interferometers, and by Linfoot on the theory of new optical combinations would form a considerable and suitable nucleus of research. Tyndall in turn was not convinced, and added the very down-to-earth point that, whatever decisions were taken, it was most unlikely that post-war problems with the supply of materials and labour would permit the erection of new buildings to house the proposed Institute, at least for several years, by which time the situation might have changed.

In July 1949, i.e. about two years later still, Burch attended a meeting of the Isaac Newton Observatory Board, of which he was a member, at which the Astronomer Royal raised the matter again. He said that he had hoped that the optical parts for the proposed 100" telescope would be made at Bristol. Burch did not turn down the idea outright and - characteristically - for the next couple of months debated with himself what his proper course of action should be. He was heavily involved with work on the microscope. He weighed up his obligations to the Nuffield Foundation who were financing the microscope; to Bates, who had already put in so much work on the project; to the University; to Grubb Parsons, the firm most likely to secure the contract if it did not come to Bristol; and to the world at large. He wanted to make a "constructive" response to the suggestion. In a letter to Farrer-Brown of the Nuffield Foundation, written about July 6th. he concluded "I am thinking of telling Spencer-Jones that present commitments preclude my tackling the creation of a National Institute". In a conversation with Farrer-Brown about three weeks later, however, he appeared not to rule out the possibility of agreeing to undertake consultative work on technical matters, provided that arrangements were made to ensure that he would not be asked to do more. But by the following day he had decided to postpone his letter to Spencer-Jones telling him of the decision.

Dr Hawkins, who was very keen on the idea, had found two tunnels in which the testing of the telescope optics could be carried out. The steady temperature and freedom from vibration which they provided made them ideally suitable for this delicate operation. Their location is not mentioned. One was probably in the Redcliffe Caves, which open off the corporation wharf near the swing bridge in the middle of Bristol; the other is anybody's guess. Burch now felt that he had to consider her interest also. He writes of "my refusal to direct that show (i.e. the Institute project) till the microscope job is finished". But a few days later, another letter, to Farrer-Brown says "I must withdraw that decision about declining any possible request to take directive responsibility in respect of telescope work done here ..." pending further discussions with Plaskett, Mott and the Vice Chancellor. A letter to Plaskett written about this time - undated, but almost certainly later - says "I take the decision that until the microscope development and research programme is finished, I will not undertake directive responsibility in respect of an institute of optics, or indeed anything else. Consultative responsibility, yes; directive, no." and "I think it would be a thousand pities if my action has sunk the Bristol large scale lab ..." He was still entertaining the idea that Bates might change from working on microscopes to telescopes. A little later still, after a telephone conversation with Plaskett, and discussions with Bates, " what pleases me is that the idea that if anything is to be done, I must do it, seems to be dropping out of the picture." And again, in September, " I am out of the telescope situation. Mott has agreed that I may decline responsibility 100%, and has offered it to Bates, who is making up his mind."

The last paper in the file is a copy to Burch of a letter from Plaskett to Spencer-Jones, dated 28/9/49, which summarises the position admirably. Bates and Miss Hawkins should undertake the work on the 100" telescope, Burch would be available for consultations. The available tunnel is admirably suited for the job. Mott and the VC are satisfied that the project is feasible. But Bristol is concerned about the situation that would exist when the job was finished; and Plaskett himself is hesitant about taking the job away from Grubb Parsons. Both doubts would probably be resolved by the setting up in Bristol of a permanent Institute of Applied Optics, with the 100" mirror as its first job. Thus Plaskett. Some memoranda in Mott's handwriting are clipped to the letter, and indicate that the idea had been discussed in some detail.

But nothing came of it. As far as this account is concerned, the reasons must remain a matter of speculation. Did Bates refuse the job? Perhaps the necessary finance was not forthcoming? Or did caution and conservatism direct the contract to the established firm of Grubb Parsons?. Doubtless answers to at least some of these questions could be found by consulting the files of the Isaac Newton Observatory Committee, the Astronomy Department at Oxford, the Royal Society, Grubb Parsons, and so on. But not by me; somebody else, perhaps. For our present purposes, the point to note is how near we came to having an Institute of Applied Optics associated with the laboratory. Also the crucial role played by C.R. Burch. His behaviour was quite characteristic - an obsessive concern about his responsibility towards other people, his vacillation and his reluctance to take a firm decision. Had he been other than he was, the story of physics at Bristol might have been quite different. But then, he wouldn't have been Burch.

(e) Polymers.

In March 1955 Andrew Keller arrived in Bristol to take up an appointment as a Research Associate. He had taken a chemistry degree in Budapest and had completed all the work for a doctoral thesis, but as a result of the worsening political situation, had left Hungary before the essential oral examination. From 1950 to 1955 he had been employed by ICI in Manchester, working on the morphology of crystalline polymers. The project was part of a Ministry of Supply contract with ICI and when the contract was terminated, it was arranged that Keller should continue working on similar lines at Bristol, financed by a grant from the Ministry. The move was arranged by Professor Astbury of the physics department of Leeds University, who was a consultant to ICI, and who was in touch with Frank at Bristol. The original Ministry contract provided funds for one year - but with the saving phrase " in the first place".

From these small and almost accidental beginnings has grown one of the larger and more distinguished research groups in the Department.

Keller submitted a thesis for the PhD degree in Bristol in 1958, and it must be one of the very few such theses to include a list of 14 previously published papers, and another list of 10 more, either published or "in press", arising out of the PhD work itself. The title of the thesis was "The Texture of Crystalline Polymers", a general field of study in which the author has been involved with exemplary singleness of purpose ever since. In 1963 he was taken on to the University staff as a lecturer, and in the same year was awarded the "High Polymer Physics Prize" by the American Physical Society - the third recipient and the first non-American.

In January 1961, the prestige of Bristol physics in the study of polymers had received a considerable boost from the holding of an international conference in the Department with the title "The Physics of Polymers". This was arranged by The Institute of Physics and the Physical Society, as it was then called. The conference lasted for three days, with about 35 substantial papers being presented by people from British universities and industrial laboratories, as well as some from Germany and the U.S.A. (The Proceedings appear in B.J.A.P, vol. 12.). The papers included one by Frank and Tosi on the theory of the crystallisation of polymers. At this time Frank was fairly closely involved with the polymer work on the theoretical side, and in addition to a number of PhD candidates, the group also included one or two visitors from Japan - a practice which continued for a long time. In 1966, the group was strengthened by the arrival of Ian Ward, already a fairly senior person with some 14 years research experience, mainly with ICI fibres division at Harrogate. His particular interest was the mechanical properties of polymeric materials, particularly those anisotropic materials which result from industrial manufacturing processes. The relation of these properties to the arrangement of the molecules complemented Keller's interest in the details of these arrangements. Ward's appointment was also related to the MSc on Materials which was starting at about the same time, and in which he played a significant role. After appointment, he continued to collaborate with former colleagues, and for a couple of years around 1967 was formally employed only part-time at Bristol, and part time at the ICI laboratories at Runcorn.

About this time the polymer group, in addition to Keller and Ward, included two established staff members. These were G.A. Bassett, an electron microscopist who had arrived from an industrial laboratory in 1960, and who left to go to Warwick University in 1966, and J. Dlugosz, also an electron microscopist. There were also usually one or two Research Assistants/Associates on finite-term appointments paid, for example, from SRC funds; perhaps one or two other temporary "visitors" paid from other outside funds; three or four research students and two or three technicians. But Keller, always ambitious (for his subject, rather than for himself personally) wrote a memorandum in 1965 suggesting that the group needed at least three more senior staff (a crystallographer, a physicist, a chemist, and preferably also a theoretician) and 3-4 more technicians, before it would be really viable. He was even considering the possibility of leaving, if these facilities could be offered elsewhere but not at Bristol. In fact, within the period 1960 - 1969 he was offered professorships at four universities in Great Britain and six in America in addition to half a dozen senior industrial appointments in the U.S.A. But he didn't leave. Although there were no dramatic changes, the established academic staff grew steadily, by appointments made initially to junior posts, and there were also increases in the number of research workers paid from other sources. By 1973/4, there were 7 permanent staff, 13 other "staff and visitors" 8 of whom were paid from funds originating with SRC, 6 research students who were candidates for PhD and 5 technicians. Keller had been made a professor in 1969. His title was "Research Professor in Polymer Science"; this was very appropriate, since in addition to his intense involvement in research, his only teaching duties were directed to research students, i.e. those on the MSc course. He was elected to a Fellowship of the Royal Society in March 1972.

In 1968 Ward was appointed to a chair of physics at Leeds. This was a considerable set-back for the Bristol group, and the problem was accentuated by the fact that the big N.M.R. magnet and associated equipment which had recently been purchased for Ward's use with SRC funds, went with him to Leeds. After a gap of a year he was replaced in Bristol by Bob Arridge, who had also had considerable research experience (about 18 years) in industry, with British Nylon Spinners and Rolls Royce. His background was in maths and engineering, and his main interest developed around the problems of fibre-reinforced materials. Although less intimately concerned with polymers than Ward had been, he was able to make valuable contributions to the work of the group. He also fitted well into the gap created by Ward's departure in both undergraduate teaching and in the teaching of the MSc course on the physics of materials. The last important staff appointment in the period covered by these notes was that of Ted Atkins. He started as a Research Associate in 1969, having previously worked in the Biophysics department at Leeds, and his particular contribution to the work of the group here was in the field of X-ray diffraction. However, his interest in biological materials was to have a considerable influence on the work of the group as a whole. By coincidence, Keller had been introduced to work on collagen during one of his numerous visits to American laboratories so that he was sympathetic to the new departure. There was also a flourishing group working on similar topics in the Bristol Biochemistry department, with which contact was soon established.

In January 1968, SRC had woken up to the importance of polymers, and had set up a "panel" of which Frank was a member, to look into the matter. The report of the panel in February 1969 advocated considerably increased support for polymer research in universities, and also proposed the concentration of the extra finance in a small number of places - "centres of excellence", although the phrase had not been coined at that date. At the same time, a re-organisation of the structure of SRC resulted in the formation of a new Polymer Science Committee. This was placed under the wing of the Engineering Board, instead of the Science Board, which had previously looked after such matters. The new committee consisted of seven academics and three industrialists, with one member of mixed provenance. The Universities represented were Liverpool, Manchester(2), Imperial College, Queen Mary College, City, and Loughborough. The committee was empowered to implement the recommendations of the panel report. This had named eight institutions which "appeared to have specialist experience, knowledge or expertise" in the particular aspects of polymer research singled out for preferential treatment. In this list Bristol appeared twice, and accordingly hopes were running high that we would receive some of the extra money. In the event, only five places were nominated for support, namely Liverpool, Manchester, Imperial College, Queen Mary College, and Strathclyde/Glasgow. No reasons for the choice were made public, and not unnaturally, Bristol felt somewhat aggrieved at the outcome. The five places named were described as "interdisciplinary groups" - a magic word that was just becoming popular - although the brief statement of their activities in the report did not give much justification for the phrase. However it was known that the emphasis of the Committee's thinking had been on technical and industrial questions, and the work at Bristol did not have much direct connection with such matters. Also, it was known that the Committee "had noted the inadequate and badly structured support for polymer work by the University (of Bristol)" A minute in the records of our departmental Steering Committee (14/6/69) quotes Frank as saying that "SRC is likely to support polymer work at Bristol if, and only if, Keller is made a professor" (In the panel report, the head of the Bristol group is given as Professor Frank.) In fact, the machinery to promote Keller to a professorship was already in motion at the time, but even so, no extra funds came our way.

Later developments did not justify the feeling of gloom which these events engendered. Most of the money for research from sources other than the UGC had come from either D.S.I.R. or the Ministry of Aviation. In the period 1958-69 the former had provided £72,000 and the latter £53,000. From 1969 to 1974 the corresponding figures were £157,000 and £14,000, - the names were now SRC and the Ministry of Technology, but they were the same pockets, really. Some of the money was in the name of Ward, but most of it was for Keller. Atkins later broke new ground by obtaining £7,000 from the Arthritis and Rheumatism Council. Most of this income was spent on paying the salaries of Research Assistants or Associates on short-term appointments. The building of the extension to the laboratory brought with it UGC money for equipment, of which the polymer group obtained £40,000. So the work was not starved of resources, and SRC contributed quite a lot.

Another interesting side-light on the growth of the group can be obtained by looking at the list of publications. In the eight years up to 1963/4 there was a fairly steady output of about five papers per year. From then until 1975/6, the rate rose from about 13/year to about 24/year. Throughout the whole period Keller was personally involved in much of the detail of the research, rather than just exercising a general supervisory function. As a result his name appears as author or co-author on almost every paper - except, in the later stages, those that were the responsibility of Ward, Arridge or Atkins. All of the Keller papers were concerned essentially with various aspects of the structure or morphology of crystalline polymers. Frank's name also continued to appear occasionally as a co-author throughout the period.

The later growth and successes of the group are really outside the period covered by this account. But one important event may be mentioned. In April 1974 there was another major conference at Bristol, this time under the auspices of the Colston Research Society. The title was "The Structure of Fibrous Biopolymers", and it was organised by Atkins. The organisation was a considerable task, since well over a hundred people attended, about sixteen of them from overseas. Bristol itself provided a contingent of about thirty, mainly - but by no means entirely - from physics. Perhaps this may be taken as indicating that biophysics had at last been successfully established in the Department.

CHAPTER 5

TEACHING

(a) History at Faculty Level.

Before describing the development of physics teaching, it is useful to look at the background against which the changes took place, since the general structure of all undergraduate curricula is determined by University and Faculty regulations. The structure undergoes the usual processes of slow modification and, in addition, there was in our case, a major reorganisation which came into effect in 1967: in these changes the physics Department played a considerable part. In the late 1940's, there had been no major change for several years. Special war-time rules and practices were gradually withdrawn. It had been possible, for example, to qualify for the award of a degree in less than the usual three years, if a period of National Service intervened; and there had existed a separate subject called "Principles of Radio" available to students of physics. Both of these features disappeared in 1946/7. For many years it had also been possible, in principle, to obtain a degree of B.Sc. "by research" rather than by examination: admission to this programme formally ceased in 1960.

The science degrees available in the late 1940's were three in number:-

(1) The Ordinary Degree, which required two years of study after passing the Intermediate examination. The curriculum consisted of either two "Ordinary" subjects or one "Ordinary" and two "Subsidiary" subjects. An Ordinary subject occupied one-half of a student's time for two years, while a Subsidiary subject made the same demands on time, but for one year only. It was not until 1961 that admission to the Intermediate course was suspended, although for some time the numbers had been quite small e.g. 5-10 per year. Thereafter it was impossible to satisfy the over-riding requirement for three years residence before graduation, without repeating some part of the Ordinary Degree course, and the weaker students frequently did just this. It was thus only a natural corollary that the admission of students to the University as Ordinary Degree candidates should also be discontinued in 1964. In practice, all admissions had been formally to Honours courses for some time before this, and the award of an Ordinary degree was made only to those whose performance did not justify the award of honours.

(2) The Honours Degree, in a named discipline, was thus the objective of every student on admission. Historically, this required one year further study after reaching Ordinary Degree standard. In fact, the nomenclature of Part 1 Honours and Part 2 Honours, taken after two and three year's study respectively, which reflected this state of affairs, persisted until 1969: the influence of Oxbridge practice was clearly considerable. A student who obtained exemption from the Intermediate examination on the basis of results obtained at school in A-level (or its predecessor, the Higher School Certificate) would study for three years, and so both complete the prescribed curriculum and satisfy the residence requirement. In addition to the study of an Honours subject for all three years, the regulations called for the successful completion of either one Ordinary course or two Subsidiary courses in other subjects, - except for a degree in Mathematics, which was always the

exception to all the rules. These outside studies were expected to be completed in the first two years, and thus the student, in effect, spent the equivalent of two years full-time study on his honours subject, half-time during years 1 and 2, and full-time during year 3.

(3) The list of possible honours subjects had included since the mid-1940's the entry "General Science". This possibility had been introduced in response to complaints - even then - of over-specialisation. In particular, it was intended to provide a curriculum suitable for intending administrators and teachers. The regulations called for the study of either three Main (i.e. "Ordinary") subjects, or two Main and one Subsidiary subject, with an additional course in the third year on "The Development of Science". This last was an ad hoc series of lectures, given by staff from several departments. Nine combinations of subjects were listed as being acceptable for the General Science degree, of which only one included physics, namely Physics and Chemistry Main with Mathematics Subsidiary. In 1946 a tenth group was added, - Physics, Pure maths and Applied Maths, all Main - and in 1947 the restriction to specified groups of subjects was dropped.

In 1948, the degree of "BSc in General Science, which may be awarded with Honours" was listed separately from the other honours schools in the prospectus, but there was no change in the curriculum. The list for 1950/51 included mention of an "Advanced Course". This was conceived as being something in between an Ordinary and an Honours course, and could be spread over either two or three years. In physics it consisted of the Ordinary course in the second year, and selections from the second- and third-year Honours courses in the third year. In 1953, however, the curriculum requirements for the General Science degree were changed so that they called for the study of either one Advanced and two Ordinary courses or one Advanced, one Ordinary and one Subsidiary course, together with the Development of Science in both cases. The change in the rules clearly had the effect of raising the standard of the General BSc, at the cost of making it slightly less general. Previously it had required essentially three subjects to Ordinary degree level, (whereas the Ordinary Degree itself required only two), but after the change one of these was to be taken beyond that point, but not as far as Honours final standard.

After this, only minor changes took place until 1960. In that year, Professor Pryce (physics) and Professor Peel (geography) almost simultaneously submitted papers to the Faculty Board critical of the overall degree structure, and, in particular of the role of the Subsidiary subjects required by regulations as part of an Honours curriculum. The original intention of the rules was probably to oblige students to broaden their education, and Pryce questioned whether it was part of the function of a university to do this. More particularly, even if it were to be done at all, he doubted whether the process should be continued beyond the first year of residence. One consequence of such rules was that students were often obliged, for adventitious reasons depending on time-table details or the availability of lecture rooms, to study subjects such as philosophy, psychology or economics, in which they had no great interest, but which, alone, required no pre-university experience. If, on the other hand, the role of the Subsidiary subject was thought to be primarily one of giving support to the Honours subject, then uniformity of practice across the faculty was difficult to justify, since different honours schools had different requirements. Peel presented a similar case in rather more detail. Dr. Folkes (botany) and some colleagues from other biological departments wanted to reduce the size of the minimum unit of study from the then current Subsidiary course, which represented half-time study for a whole session, to something much smaller, - e.g. one third of the size, - to be described as a Unit course.

These, and other, arguments led to the calling of two special meetings of the Board of Faculty at the end of 1960. These were called Conferences, and were intended to discuss the whole problem. As a result, several causes of dissatisfaction were aired. Clearly, all was not well with the General Science degree. Although when awarded with honours - as was usual - it was supposed to have parity of esteem with Honours degrees in particular subjects, this was not in fact the case. Outside the University, and particularly in schools, it was frequently thought of as a "pass" i.e. non-honours qualification. This possibly arose from its name, carrying as it did associations with the more familiar London General Degree, which was not of honours standard. Inside the University, it was manifestly regarded as second-best. Repeatedly, at Board meetings, a student was said to be not good enough to continue in his honours school, and so should be transferred to the General degree: reverse transfers were very rare. There was also a strong demand for a less rigid system; flexibility was the watchword. This plea was sometimes supported by the assertion that the impending increase in student numbers would inevitably result in a wider spectrum of ability ("more means worse"), and that the existing organisation did not provide sufficient incentives to study appropriate to such a more heterogeneous group. This was particularly urged by the advocates of unit courses.

The Ordinary Degree also came in for a good deal of criticism. For historical reasons, as we have seen, the curriculum as prescribed by regulations really consisted of two years of full-time study following Intermediate, i.e., according to the practices then current, following admission. Although Ordinary Degree candidates were, by this time, all rejects from honours schools, it frequently happened that their examination record was such that they did indeed qualify for the award of the Ordinary Degree in two years. They were then obliged to spend a "supplementary third year" in order to satisfy the residence requirement. This was manifestly unsatisfactory, both for the student, who too often regarded it as a waste of time and took little interest in these enforced studies, and for the department which had to find space to house him. (This topic is discussed further on p. 121.) The number of Ordinary Degree candidates was not small; the total recorded as being in residence, in all three years of the curriculum taken together, was of the order of 10 - 15% of the total of undergraduates in the faculty.

Other matters raised in the very wide-ranging discussions were;- the possibility of arranging for Subsidiary subjects outside the normal Faculty boundaries - Economics had only recently been added to the two traditional fringe subjects of Philosophy and Psychology; the status of Subsidiary subjects, as raised in the original memorandum from Pryce; the need for new curricula which overlapped traditional subject boundaries; the role and status of the General Degree; the possibility of a 4-year first degree, to consist of two years of broadly-based study leading to a pass degree, followed by a further two years of more specialised work for a fraction of the cohort, leading to an honours degree. There was, in addition, a rather basic criticism of the existing system which relegated students to the inferior status of "ordinary degree candidate" too readily and too soon. Physics Department spokesmen pressed for a general review of the whole degree structure, rather than patchwork modifications.

The result, as might have been predicted, was the setting up of a committee - or, to be more precise, of a succession of committees - to consider the problems in detail. Their deliberations gave rise to changes in regulations spread over the next seven years. At the end of 1962 the Faculty Board agreed to permit three Unit courses to be substituted for one of the Subsidiary courses required for the Ordinary degree. This came into effect in the 1964/65 session, and shortly afterwards was extended to Subsidiary courses forming part of an Honours curriculum. At the same time, the requirements for the Ordinary Degree were increased from two Ordinary courses (or one Ordinary and two Subsidiaries) to two Ordinary courses and one Subsidiary (or one Ordinary and three Subsidiaries). This, in effect, increased the total load from two years of full-time study to two and a half years. Except in a very few special cases, the change removed the need for the unpopular Supplementary Third Year. At the same time, Ordinary Degree candidates were allowed to take Honours courses in part-fulfilment of regulations, if this appeared likely to be to their benefit. The Ordinary Degree thus became a reasonable qualification, and not just a certificate that the holder had survived for three years at the University - as it had been described in its previous form.

The General Degree was discussed at great length, and a movement for its abolition gained support. It was suggested that it should be replaced by a number of Joint Honours degrees, the details of each one to be the subject of negotiation between the two departments involved, but all of them to conform to the same general pattern, and all to be treated on the same basis as the traditional Honours degree in one subject. The objective was two-fold:- to attempt to achieve that parity of esteem that the General Degree had never attained, and to permit the development of cross-disciplinary studies. Draft regulations on these lines were produced in January 1965, but they were to be much changed before being finally adopted. The departments of physics and chemistry actively supported these ideas, and in a memorandum dated 12/2/65 the Physics Department put forward detailed proposals. These included also another major change from past practice - namely that all students in the Faculty should be required to study three subjects in their first year, instead of two, equal weight being given to all three. The detailed discussions in the Faculty took place in three parts, dealing respectively with the Physical Sciences, Biological Sciences and Earth Sciences. By drawing mathematics into the suggested scheme alongside physics and chemistry, it would be possible to give a student the opportunity to proceed, in his second year, to any one of several curricula, some Single and some Joint Honours. Although it was clearly desirable that a similar pattern should obtain throughout the Faculty, the three departments in the physical sciences group were prepared to operate it alone, alongside the older pattern in the rest of the Faculty, if this should prove to be necessary. In the end the proposal was adopted by all departments, and appears for the first time in the published regulations for 1967/68. The new structure necessitated a revised nomenclature, and the name Standard Course was adopted for one-third of a year's work, being further subdivided, where appropriate, into three Units. Although the total teaching load was unaffected, the change from two to three subjects in the first year meant that, on average, each department had to deal with 50% more students. The fact that they only attended for 2/3 of the time was of no help towards a solution of the problem of finding the necessary space, particularly in laboratories. The problem was eventually solved, although for the first few years a system of rationing of places had to be adopted, to prevent one department accepting students, too many of whom wished to take some other particular subject as their second or third option.

The general scheme adopted is still in operation at the time of writing (1983). It certainly achieved one of its objectives, namely that of introducing more flexibility into the undergraduate curriculum. In the session 1981/82, for example, 10% of all Stage I students proceeded in Stage II to a course different from that for which they had originally registered. Their examination records show that they were by no means the weaker members of the cohort. The Joint Honours schools have been less successful. There were originally eleven, but four have been abandoned at various times for lack of adequate support, while two others have transformed themselves into Single Honours schools. Joint physics-chemistry became chemical physics, and joint botany-zoology became biology. Of the remainder, only three have a typical annual intake of more than half-a-dozen students, and in the transfers between courses, joint schools still tend to lose more students than they gain. Parity of esteem is clearly still elusive. It would take us too far afield to speculate on the causes, but it is interesting to note that the problem was, and is, by no means confined to Bristol.

One consequence of the changes was that the regulations for the Ordinary Degree had to be re-written in the new terminology. The degree continued to exist, as a safety-net for those students who had embarked on an Honours curriculum, but had found the courses too demanding. The work-load was specified as three Standard courses per year, and the requirement for the award of the degree was the successful completion of six Standard courses, of which at least two had to be at the level of either Stage II or Stage III. It thus again became possible for a student to satisfy these requirements in two years of residence. If he had done so, he would also have satisfied the requirements for proceeding to the third year of an Honours course - if his passes had been in appropriate subjects. But in 1970, the Board recognised that such a student might not wish so to proceed, for any one of a variety of reasons. The prospect of the resuscitation of the Supplementary Third Year, in order to permit him to satisfy also the three year residence qualification, was not viewed with favour. Instead, since 1971, it has been possible for a student so placed to elect to leave the University after only two years of residence with the award of an Ordinary Degree. Only one or two students each year take advantage of this provision, but it is important in that it represents the first breach of the sacrosanct three-year residence rule.

Before leaving this subject there is one further episode that can with advantage be recorded, even though it gave rise to no changes in practice. In February 1971 it was becoming clear that the degree structure introduced three years previously was not perfect. I therefore suggested to the Undergraduate Studies Committee that we might look at the possibility of introducing modifications that would lead in the direction of a "course credit" structure. Variants of this were at that time in operation in London and one or two other places. Very briefly, it is an arrangement under which a student is allowed to study any courses in any order, free, as far as possible, from constraints imposed by regulations. There would inevitably be some constraints imposed by the availability of laboratory places and by time-table clashes, and, in some cases a pre-requisite course might be thought desirable. An efficient system of tutors to advise students on what would be reasonable for them to attempt would be essential. At the end of the three years residence - and not before - a decision would be taken on the basis of the whole three year record as to whether a candidate qualified for an

Honours degree in X or a Joint Honours degree in X and Y, or a General Honours degree, or an Ordinary degree, or whatever, and also as to the class of honours to be awarded.

The advantages claimed for a structure of this kind are:-

1) It gives the maximum freedom to tailor a curriculum to the interests and abilities of each student - even more than the single/joint honours arrangement and the three-subject first year, which were not proving quite as flexible as some of us had hoped.

2) It provides a student with courses of study which are at all times at a reasonable level, and neither so elementary as to be boring, nor so difficult as to be incomprehensible.

3) It provides at all times incentives to study of the kind appreciated by the average student. ("I have an examination coming up in 3 weeks/ 3 months.")

4) It avoids the necessity of pre-judging a student after, say one year of residence, and predicting that, two years later, he will or will not be "of Honours calibre".

5) It can accommodate all degrees of specialisation from the existing single honours to something even more general than the old general honours.

The disadvantages are also fairly clear:-

6) It needs a very carefully designed time-table covering the whole Faculty which must remain constant - or at least change only very slowly.

7) The problem of deciding what selection of courses would qualify a student for which degree would be very complicated, and might well lead to a good deal of controversy.

8) The related problem of devising a formula that would relate the class of degree to the whole three year record might be even more difficult.

Predictably, the response of the Faculty to these suggestions was to set up a Working Party, with - predictably - me as chairman, to look into the question. We met for lengthy discussions on four or five occasions and the one thing that became clear was that, with all the good-will in the world, it would be almost impossible to agree on a scheme that would satisfy everybody present - let alone those who were not present. It might have been just possible to hammer out some kind of compromise, but at this point I was made Assistant Dean, and I did not think it proper to use whatever authority this gave me to press the matter too strongly. Regrettably - with hindsight - I therefore allowed the matter to lapse; we never even got as far as writing a report on our deliberations. I say regrettably since, shortly after I retired, the matter arose again in a slightly different form, and all I could do was to hand over my rough notes to those concerned. Nothing came of that, either. However the effort was possibly not entirely wasted. It may be that it contributed to some extent to a move within the Faculty towards a much less rigid interpretation of rules, and to a willingness to tailor courses to the needs of an individual within the existing framework.

But I still think that a course credit scheme has much to commend it.

(b) The Physics Curriculum.

Within the limits set by Faculty rules, the story of physics teaching has been a mixture of steady development and rather violent oscillations of policy. The main interest in undergraduate affairs has been the special honours curriculum. Up to 1966 this was fairly static. First year students took physics as a Main subject and pure maths as a Subsidiary. The second Subsidiary subject required by regulations was always applied maths; there was no choice. The course was given by the staff of the Mathematics department, and only physics students attended it. Unique in Faculty practice, it was given in two parts, one in each of the first two years. (For a short period around 1963 - 66 there had been a slightly different pattern in which the mathematics teaching was re-arranged to consist of a single course called auxiliary maths, but still given by the staff of the maths department. It was specially tailored to meet the needs of physics students who, again, were the only customers. It had the status of two Subsidiary subjects, and extended over two years, so complying with regulations. It was examined in two parts, one at the end of each year.) In the more normal pattern, physics students in their first year had about 100 lectures per session (i.e. 4/week) in each of physics and pure maths, and 50 in applied maths, a total of about 250. With the introduction of the three-subject first year in 1967, the pattern changed to physics, maths, and another subject, each with 72 lectures per session, a total of about 216. There were also related changes in Stage II. Previously, a physics student had taken 130 lectures in physics and about 50 in applied maths. Under the new scheme, he took about 220 lectures in the session, all in physics. Thus the total load in all subjects, and in both sessions together, hardly changed at all, as indeed was the intention. But the load carried by physics staff increased from about 240 to about 290 lectures. This corresponded to the shift in the responsibility for the teaching of applied maths from the maths to the physics department.

The organisation of the final year of the physics course has been subject to much more variation. By 1947, the concept had been established of a set of "compulsory" lectures, supplemented by a second "optional" set, from which the student could make a selection. At that time there were about 80 compulsory lectures per year, and about six ten-lecture courses in the optional group, from which the student was to choose two or three, making a total load of about 100-110. Gradually the number of compulsory lectures crept upwards, until by 1964 it had reached about 140 - plus some options. In 1965, options were abolished, and all lectures became compulsory. In 1973, practice swung to the other extreme, and all the courses were “optional". Thirteen 20-lecture courses were offered, from which the student could choose to be examined in not more than eight. However, only his best six marks were counted towards his final assessment, so that the effective total lecture load was between 120 and 160. This arrangement lasted for only two years, and in 1975/6 the Department reverted to a core-plus-options system, which has persisted up to the present time. Originally the core consisted of four 12-lecture courses, on Quantum Mechanics, Statistical Mechanics, Nuclear Physics and Electromagnetism, together with a fifth, 24-lecture course on Solid State Physics, a total of 72 lectures. The number of optional courses available, and their subject matter, has varied from year to year, being usually about ten in number, each of about 20 lectures. Students chose not more than five in which to be examined, only the best three marks being counted in assessment. The total lecture load thus lay between 132 and 172. Later modifications have been the addition of a sixth compulsory lecture course, with the title "Fluids" or, later, "Continua", and a change in the number of optional courses which count towards assessment from three to four. The minimum total lecture load thus stands at 164, and the maximum at 184. All of these changes of policy were agreed, and probably suggested, by a staff meeting, for what doubtless seemed at the time to be good and sufficient reasons. Clearly it cannot be said that the teaching policy has ossified. The one feature which remains constant is the slow but inexorable increase in the lecture load on the student.

Courses other than the single honours curriculum must be mentioned briefly. The Joint Honours schools have already been discussed in general terms. There were, initially, three schools which involved physics, namely those with maths, with chemistry and with geology. Physics-geology was discontinued from 1974; it was attracting insufficient students to be viable. Physics-maths is still flourishing, while physics-chemistry changed its name in 1972/3 to the single honours school of chemical physics. This reflected the fact that, unlike some of the other joint schools, the curriculum did not just consist of suitable bits from the already established courses of the participating departments; it included also a substantial element of subject matter peculiar to itself, and not available to other students. This arrangement, and other features that have contributed to its success, were largely due to the efforts of Dr Barron from chemistry, and Dr Milne from physics, whose work has given the group not only a good academic standing, but also an *esprit de corps* not commonly encountered. The joint school of physics-philosophy was set up in 1975, mainly owing to the efforts of Professor Ziman. Its numbers remain small, as is perhaps not surprising.

In 1947 I had suggested that there should be two first-year classes running in parallel, and this later became the normal practice. They have had various names at various times, but, essentially, one was designed to form a basis for further study in subsequent years (often called Stage I Honours), while the other was a terminal course, covering more ground in a more superficial manner (usually called Subsidiary). The arrangement persisted until 1969/70 when, as part of the re-organisation which followed the introduction of the three-subject first year, the Department reverted to a single general-purpose Stage I course. Such a step was highly desirable if the full benefit of the new flexible system was to be obtained. However, in 1973/4 the process was reversed, and two parallel Stage I courses were again offered. Other departments were moving in the same direction, and, in agreeing to the changes, the Faculty Board asked that special arrangements be made for those few students who, having taken the Subsidiary course, changed their minds and wished to proceed to Stage II in physics. This was done by instituting a programme of vacation study, followed by a pre-sessional examination.

Until 1967/8, the Physics Department, rather exceptionally in the Faculty, also ran two parallel Stage II courses, usually known as Ordinary and Honours. The former was again a terminal course, while the latter led on to Stage III honours. After the re-organisation in 1967, the scheme was dropped, and the Stage II lecture programme was divided into nine "Units", any group of three counting as a Standard course. Honours physics students were expected to take all nine, while others could take a selection as a supporting course to an honours degree in another subject, or as part of an Ordinary Degree. This was administratively convenient, but it did mean that non-specialists were not provided with a balanced programme to meet their needs, but were often making do with a collection of fragments, sometimes chosen for no more valid reason than the exigencies of the time-table.

Also as part of the general re-organisation in 1964/5, the Physics Department offered three separate Unit courses, in "Optics and Microscopy" (for biologists), in "Optical Crystallography" (for geologists), and in "Electricity and Electronics" (for anybody). They were all elementary courses, the first two not even requiring any previous knowledge of A-level physics. They were never very popular, and only survived for a few years. In 1971/2 we offered one single Unit and one double Unit in Astronomy. This took advantage of the interest and expertise of three staff members, Smith, Hillier and Fowler, who had already had some experience of teaching Extra-Mural classes on similar subjects. In 1973/4 the Units were replaced by a single Standard course on Astronomy. This, too, required only a modest previous knowledge of physics, and proved to be quite popular as a "third" subject for first-year students. A succession of graduate students from the cosmic ray group has so far been forthcoming to act as lecturers and so permit the tradition to be continued.

Returning now to the discussion of the single honours curriculum, something should be said about the content of the lectures, which is even more important than their number. It has been in a constant state of flux in matters of detail, and a series of ad-hoc sub-committees and working parties have discussed it. They have been concerned either with the scope and balance of the lectures as a whole, or with the treatment of some particular topic, such as electronics. From 1972 these activities evolved into a permanent body, the Course Co-ordination Committee, charged with the task of keeping the matter under constant review. Two leading themes run through many of those discussions of which a record exists. The first is that the lectures contain too much material. There is always a tendency for a lecturer to insert new material into Stage III courses, particularly if the subject happens to be his special interest. He tends to be carried away by his enthusiasm, and to give free rein to a desire to tell the class about the latest developments. The students have an ambivalent attitude to this process. While they sometimes complain that a particular course contains too much material, they do like to think that they are up at the forefront of progress. As a result of this process, the more elementary and more basic material becomes ever more tightly packed, and the pressure percolates down into Stage II and thence into Stage I. (One of the more pernicious, if non-essential influences working in the same direction is the now almost universal "overhead projector". With no more effort than that involved in preparing a set of lecture notes, the lecturer can present to his class in two seconds as much detailed material in the form of equations and the like as would have taken him ten minutes to write on a blackboard. If the students are expected to copy down even part of the text, the result is disastrous; if they are expected merely to assimilate it, the result is almost as bad. Duplicated notes handed out before the lecture escape criticism under the former heading, but not under the latter.) In 1968 a subcommittee charged with the task of re-organising the Stage III curriculum was urged by a staff meeting to use the opportunity to prune the content somewhat, and this they claimed to do. But in 1972 I was still so concerned about the increasing content that I called for a 10% reduction by the simple expedient of deleting the content of the last lecture of every 10-lecture course and then rationalising what remained. The staff meeting acquiesced and complied - at least in some cases - but I suspect that the material slowly crept back in again.

The second recurring theme in many of the discussions is that the course is too theoretical, abstract, mathematical, academic - various adjectives are used at various times. There are a number of factors which tend to produce this result. Abstract theory is often regarded as intellectually more respectable than applications to real problems. Maurice Pryce, for example, regarded anything with a flavour of applied physics as "technology", and therefore automatically excluded from a proper physics degree. There is also a tendency to regard applied physics as easier, and therefore more suitable for less able students. In 1957 a scheme was introduced with the objective of allowing weaker students to continue as honours candidates instead of being relegated to an Ordinary degree. There were two parallel sets of third-year lectures. In some cases the same topic was treated in both sets, but at different levels of difficulty. In others the subject-matter was different, the lectures for the brighter students having a more theoretical bias, while the others tended more towards applied physics. The scheme ran for two years only, and was discontinued for reasons that are not recorded. If views of this kind are accepted, then, in order to maintain a high standard in the final honours year, the whole course would have to be "theoretical" rather than "applied". The fact that opinions of this kind were not universally held is attested by the frequency with which the subject keeps appearing in the minutes of the staff meetings.

In 1953 Mott was involved in an episode relating to this same theme. At that time there was a good deal of discussion on the subject of "technological education" and the need for more and better technologists in industry. In July, Mott wrote to the Vice-Chancellor suggesting that the intake into Bristol physics courses should be increased by some number between 15 and 30: at that time it stood at about 50. In due course this was to lead to the establishment of two alternative Stage III courses, one with a strong technological flavour and involving some co-operation with the Faculty of Engineering. The time was opportune since additional space was becoming available in the Department because of the extension to the building. The proposal received the informal blessing of the VC, and discussions were under way with the Maths department about subsidiary courses, when, by coincidence (?) on August 4th., a letter arrived for the Vice-Chancellor from the UGC saying, in effect, that more money was available for "Higher Technological Education", and would Bristol like to make a bid for some of it. There followed some discussion with the engineering professors, and a rough outline syllabus was drawn up. Mott wrote "You will see that what we propose is a third-year course with an emphasis on classical physics, and suitable for students going into industry. It would not be in any real sense technology, but ...". The formal reply from the University to the UGC included this proposal. The only other item in the letter was from the engineers, and said that they were proceeding with plans that had already been drawn up as part of their proposals for their new building. This reply was sent on August 30th., and nothing more was heard of it. I have not even been able to find in the University records any acknowledgement of its receipt by the UGC. One cannot help wondering what the present shape of the Physics Department might have been, had the result been otherwise.

Some fifteen years later there was another episode in the story. In 1968 I wrote a memorandum for discussion at a staff meeting at which I was unable to be present, pointing out that much of the theoretical material taught, particularly in Stage III, was not very relevant to the work which many of our students would be doing when they found employment. It proposed no dramatic changes, but suggested that the emphasis of the course as a whole should be shifted in the direction of what was called "applicable" rather than "applied" physics. The proposal was supported by a memo. from Ziman, who went further and suggested a close liaison with engineering departments with a view to the introduction of courses that would bridge the gap between the two disciplines. The general thesis, that our courses were too mathematical and abstract, found favour with the staff meeting, and the result, as might have been predicted, was the setting up of another working party. Their report considered, but eventually rejected, the idea of splitting the third year class into two streams, and suggested instead some modest changes which finally resulted in the addition of two new lecture courses to the programme, one on Astrophysics, and one on Materials Science. This was in the period when all Stage III courses were "compulsory", and although there was some reduction of the time allowed for the existing material, the net effect was - of course - an increase in the total load. When, in 1975, the system reverted to the core-plus-options pattern, the core was mainly pure and rather mathematical physics, while the options included a considerable proportion of "applicable" physics, and even a couple of courses on "applied" physics. Perhaps this represents a reasonable balance.

The teaching of electronics is a rather similar question. In 1947 it was formally agreed that some electronics should be included in the electricity courses in both Stage I and Stage II, and there was a 10-lecture compulsory course for Stage III. By 1953, it is not specifically mentioned in the programmes for Stages I and II, and in Stage III it had changed to an optional course. This situation persisted until 1966, when a three-week full-time course on electronics, including both lectures and laboratory work, was introduced for Stage I students, to keep them occupied in the interval between the end of the sessional examinations and the official end of the summer term. The waste of this potential teaching time had always been a sore point with the staff, but the innovation was received without much enthusiasm by some of the students. A similar course for Stage II was introduced in 1968, and both continued until 1971.

It will be seen that, for a long time, the teaching of electronics was regarded as a peripheral activity. There was a widespread belief that either you have green fingers for electronics, and it is all very easy, or else you have not, and it is useless to try to learn the black art. The existence of a service group of experts, whose business it was to satisfy the electronics needs of the rest of the experimentalists, tended to perpetuate this attitude in both staff and students. One may even suspect that in some quarters it was - and perhaps still is - not regarded as physics at all, but as another branch of technology, suitable for laboratory technicians rather than real physicists. However, there has been a gradual shift in the meaning of the word. It used to mean the ability to design and build a device from components, or groups of components on a "breadboard". It now means rather a knowledge of how to assemble standard "chips" to provide an "interface" between one piece of apparatus and another, or between apparatus and computer. It is indeed acquiring a certain respectability. It is probably becoming more difficult for any experimentalist to get by without skills of this kind, and it may be that the subject will get more emphasis in the future than it has in the past.

The above was written in 1983: the situation continues to change rapidly.

(c) Laboratory Work and Tutorials.

In addition to lectures, the other traditionally important method of teaching physics is through laboratory classes. In Bristol, students in all years have always been required to attend such classes although it has sometimes been doubted whether some of the time thus spent might not have been more profitably occupied in other ways. In the 1940's, Stage I students spent nine hours per week in the laboratory, and in both Stage II and Stage III the time allocated was twelve hours. By 1957, the increasing number of students led to the duplication of Stage I laboratory classes. From time immemorial these had been held on Monday afternoons and Tuesdays; additional classes were now arranged on Thursdays and, later, on Fridays also. A similar duplication of provision for Stage II students inevitably followed soon afterwards, so that by the early 1960's all available laboratory space was being almost fully utilised throughout the week. About this time the requirement for laboratory work from Stage I Subsidiary (i.e. non-honours) students was reduced from nine to six hours per week. Then, in 1967/8, in order to fit in with the new arrangements for a three-subject first year, a further reduction to five hours per week was made; the hour from 10 to 11 in the mornings was required in order to accommodate the more complicated pattern of lectures. For Stage II and Stage III the requirement stayed at twelve hours per week.

Laboratory work for final honours students in the 1940's consisted of a collection of traditional advanced laboratory exercises. In 1965/6 "projects" were introduced for about half of the class, and three years later the scheme was extended to the whole class. The students then devoted the whole of their twelve hours per week to the investigation of a single problem, under the immediate supervision of a member of staff. A staff member might supervise one, two, or three projects: rarely more. At the time I, personally was opposed to the idea. I thought that the time could be more profitably spent in becoming familiar with a variety of experimental techniques. Never have I been proved to be more wrong. Previously we had been faced with a long-standing problem of persuading the students to take their laboratory work seriously. Average attendance rates were very low. After the change it was difficult to prevent them from spending so much time in the laboratory that their other work suffered from neglect. Although they may not learn the same things under the two systems, it has become clear that they are no less well educated. Around 1967/8 we introduced the practice of requiring third year students to return to the University a couple of weeks before the official start of the Autumn term, so as to permit them to get really started on their experiment before being disturbed by the necessity of attending lectures.

The normal arrangement, in all years, during the period under consideration, has been for the students to do their laboratory work in pairs. In the first and second years they were allowed to choose their own partners, if they so wished. When projects were first introduced some attempt at direction was tried, to try and obtain pairs with roughly similar abilities, but this was later abandoned, and a free choice given. Students working on theoretical projects were expected to work without a partner. Apart from the obvious advantage of requiring less space, and only half the amount of apparatus, there are more positive gains from working in pairs. There are - or used to be - occasions when two observers are needed in order to carry out the operations involved. But the principal advantage is that the two people do tend to educate one another by discussing their work; and they also learn how to co-operate. The disadvantages appear when it becomes necessary to allocate some kind of mark to each student as part of a general assessment process. In Stages I and II this is not of vital importance, since the weight carried by the laboratory work as part of the total assessment is not large, but in Stage III it is crucial, as will be discussed later.

The third common teaching technique is the tutorial - although the word can denote a considerable variety of different activities. In 1946, and probably even earlier, there were "tutorial classes" for Stage I students, and for Intermediate student also, I believe. These were held in a lecture theatre, where the students worked through numerical problems from published collections, with three or four junior staff or graduate students moving among the class to give help when needed. This arrangement was still in operation ten years later, but in the meantime - probably around 1948 - it had been supplemented by "tutorial groups" or "supervision groups" for Stage II and Stage III students. These consisted of, usually, four students who met a member of staff, their tutor, once per week, or sometimes once per fortnight. The general purpose was to help the students with their work, but exactly how the time was spent was left to each tutor to decide for himself. The present system has gradually evolved from this state. If, in the earlier stages, it was thought not possible to give equal treatment to all groups of students, the bias was always in favour of allocating more staff effort to the later years of the course, and to special honours students rather than others. The composition of a tutorial group did not persist from one academic year to the next, and it was also arranged that each group should have two different tutors in each year. Sometimes the small tutorial groups alternated, week by week, with larger "problems classes", and sometimes, in Stage I, the staff tutor alternated with a graduate student. "Problem sheets", that is, a collection of numerical problems relevant to material recently dealt with in one of the lecture courses, were introduced about 1968. The students were supposed to work through these in their own time, but they were also much used by tutors as a basis for discussion. At times, tutors were encouraged, or even required, to collect from their students written solutions to a selection of these problems, and to return them after marking. The slightly different concept of a "personal tutor" was introduced in 1961. The staff member responsible for each group in the first term of their first year became the personal tutor to each one of them severally. In this capacity his duties were to maintain contact with his tutees throughout their undergraduate years, and to act as counsellor and personal adviser as occasion arose. The system of a personal tutor plus a succession of academic tutors for all years is now (1980) well established.

In the following section on Assessment, mention is made of the introduction into the examination of "problem papers". These consisted of a number of problems, usually quantitative and usually concerned with the more elementary topics in physics, but not relating to any particular course of lectures. These were almost always rather badly answered by the candidates, and it became apparent that this was because they called for the exercise of a kind of skill, important in the armoury of a professional physicist, but not taught in our undergraduate curriculum. Essentially, it consists of the ability to tackle a problem when the context gives no clue about where to start or how to go about it. This seemed to me to be a serious defect in our undergraduate programme. An attempt was made to rectify it by the introduction, in 1974, of "skill sessions", a concept and a name due to Paul Black in Birmingham. A session consisted of a one-hour period devoted by the class to such exercises, under conditions which permitted and encouraged the students to discuss the work in small groups amongst themselves. Two or three staff members were also present, mixing with the class, to give advice and assistance whenever it was called for. To be honest, the innovation was not entirely satisfactory, except in alerting the staff involved to the realities if the situation. A memorandum by Chambers comments that those staff who had taken part in the exercise had emerged "considerably chastened". Several variations of the scheme were tried, suggested by experience. The duration of the class was increased from one to two hours. It was dropped for Stage III students, but continued for Stage II. In 1978 Priestley developed an alternative scheme, in which similar problems, combined with a step-by-step guide towards a solution, were printed and given to students for solution in their own time and subsequent discussion with their tutors. This abandoned part of the original conception of co-operative discussion in a small group, but made less demands on staff time. Other variants have been considered, but I doubt if the problem of teaching students how to solve problems has itself yet been solved.

Arising out of this question, several people suggested that it might be a good idea to make a collection of the better problems that had been devised over the years and make them available to students, or even publish them in book form. The matter was discussed in a desultory fashion over coffee, and the consensus appeared to be in favour of publication, and also strongly in favour of including something in the form of "answers", or at least hints towards answers. It was felt that, as a self-teaching aid, the book would be much more valuable with this addition. I agreed to act as editor, provided that the other staff members would help with the provision of the model answers. The publishing arm of the Institute of Physics, under their imprint of Adam Hilger, agreed to publish the collection, and we eventually chose the title "Thinking Like A Physicist", suggested originally by Don Gugan, if I remember correctly. The work of editing proved more time-consuming than I had thought, since it soon became clear that the answers would benefit by being extensively re-written, so as to produce some uniformity of style. The book appeared as a paper-back in 1987. I had agreed with the staff that the proceeds of the sales, if any, would be handed over to the private departmental hospitality fund, which is used as a float for financing staff-student parties and the like. This was done, and has provided a useful minor source of income. Up to the beginning of 1991 the total sales had been about 1500, and were continuing at the rate of about 200 per year.

The preceding paragraphs may have given the impression that our concern with undergraduate teaching has been solely with changes in details of the system. This is not entirely true. From time to time there have been discussions on broader topics. These have never led directly to major changes, but may have influenced practice through a series of minor modifications. For example, we have always felt that it is not possible to cater satisfactorily for a group of students with very diverse interests and aspirations. The honours curriculum has always been intended to produce professional physicists, and although a appreciable fraction of our graduates end up in other professions, we have not thought it possible or desirable to arrange the teaching programme so as deliberately to facilitate this process. We do not offer third-year optional courses on economics, or psychology, or management although the possibility has been considered. But continuing efforts to make the lectures less abstract and theoretical, and to bring them a little closer to the real worlds of technology and industry reflect an appreciation of the problem. At one time the possibility of a Joint degree with Engineering was contemplated, but the idea withered through lack of enthusiasm on either side. A second example of a broader issue that has been discussed at length is the question of extending the three-year undergraduate course to four years. This has arisen several times, and as a principle it always finds favour, because lecturers always feel that they do not have sufficient time to explain their subject-matter as thoroughly and expansively as they would like. But the question is never followed up, for reasons concerned with finance, or accommodation or educational politics. There was a more marked flurry of activity on this front around 1971, following the publication in Nature of a paper by Pippard et al. This suggested that the standard three year course for all undergraduates might not be the best way of producing the "trained man-power" needed to run a modern industrial society, nor yet the most efficient way of using the facilities available in tertiary education. An alternative proposal, which came to be known by the name of the principal author, was a two-year course for all students, followed by a further two years for the fraction who wished, and were able, to proceed (e.g. in physics) to the higher flights of research and development in universities and industry. The matter was widely discussed, and called forth memoranda from the Vice-Chancellor, Merrison (a physicist) and from Ziman. But in the end, the built-in conservatism of the system prevailed, and nothing ever came of what seemed to me to be a very sensible suggestion.

The same conservatism frustrated consideration of another general topic which I, personally, felt to be important. Almost all discussions of undergraduate teaching programmes were concerned with what should, or could, be included in the syllabus: the content was the dominant consideration. Almost never did we discuss the skills which the students were acquiring and which they would need to deploy when using their knowledge. Nor yet did we consider whether we were doing enough to help them to acquire those skills. The institution of the general paper, and the various kinds of skill session were attempts to foster the art of problem-solving, but even these modest steps met with some opposition from the traditionalists. Communication skills, both in writing and in speaking, received little attention. Electronics was always regarded as a Cinderella subject, and computing became respectable only when it became a field of research activity. Most important. the balance between these and kindred activities on the one hand, and the acquisition of knowledge on the other, in relation to both teaching and assessment, were hardly ever considered in general terms. This seems to me to be a serious defect in the system; but I would predict that it is likely to persist for a long time.

There is one other topic concerned with undergraduate teaching which should be mentioned briefly. For about 20 years, starting around 1950, there was a sentence in the Regulations which required Special Honours students to obtain some workshop experience between the second and the third year of their course. They were encouraged to obtain employment in an industrial organisation, not on a production line, but under conditions where they would obtain experience of a variety of techniques. Some of the larger firms, notably the Bristol Aeroplane Company as it then was, took a group into their apprentice training school. A minimum of 3-4 weeks was specified. The course appeared in the Regulations as "compulsory", mainly to support the case of students applying to grant-awarding bodies for extra maintenance support. For those unable, or unwilling, to arrange such a course for themselves, we took one or two groups into our departmental workshops. The workshop staff were most co-operative in providing some instruction, but the numbers that could be fitted in were rather small. The reason for this requirement was that, in those days, when research apparatus was less sophisticated, and technicians less numerous, it was very useful if a junior member of a research group was able to make small pieces of equipment for himself, in a little general purpose workshop provided mainly with this use in view. This was true in both academic and, to a lesser extent in industrial laboratories, and was very important in a school. The scheme began about 1953, but had to be discontinued after 1972. At that time responsibility for student grants was transferred from L.E.A.'s to universities, but with quite inadequate additional funds to meet the costs. In addition, financial stringency made it increasingly difficult for industrial organisations to take on such passengers, even for a few weeks. While it lasted, the arrangement provided many students with an opportunity of acquiring manual skills which would otherwise not have occurred, besides contributing something to their general education.

(d) Examinations and Assessment.

The character of the examinations, and of assessment procedures in general, are of prime importance in determining the character of a degree course as a whole. The period under consideration saw the same fluctuations of practice, superposed on a steady evolution, as was observed in the lecture courses. One of the steady trends was a gradual movement towards "continuous assessment". In the traditional pattern, the class of degree to be awarded to a student depended on the marks obtained on four or five written examinations taken in the June of the final year. Other, earlier, examinations served, in effect, only to determine whether he would be allowed to sit his "finals" at all. It had always seemed to me to be unsatisfactory that so much importance should attach to his/her performance on a few days which, for any one of a variety of reasons, could be unrepresentative, while the rest of three year's work should count for so little. One of the first moves away from this system took place in 1963, when the mark obtained in the Stage II examinations was added in to the Stage III total, with a weight of about 15%, to form the basis of the discussion of degree classes at the examiners meeting. This arrangement still exists, although the weighting factor has fluctuated between 12% and 20% over the years. In 1970 another similar step was taken when the Stage I sessional mark was added in to the Stage II total with a weighting of 10%.

Besides giving a slightly more representative measure of the student's performance over the three years, the procedure has the incidental effect of making him regard the earlier examinations rather more seriously. It is very common for a new student, suddenly released from the closer supervision to which he has been accustomed at school, to make the most of his new-found freedom with a variety of "extra-curricular activities", to the detriment of academic studies. The belief that it is possible to neglect work now, and make up for it by a period of intensive study near the end of the term/year/course appears afresh in each new cohort of students, and any system which causes the work-load to be spread more evenly can do little but good. It also reduces the incidence of last-minute panic, and "nervous breakdowns".

As part of the same process, the results of the terminal examination at Christmas in Stage I were given a small weighting (c.10%) in the total mark at the end of the year.

In this case the reason was mainly the incentive or propaganda value of the innovation, since its effective weighting in the final degree list was a small fraction of one per-cent. Somewhat later, about 1969, as similar change was introduced into Stage II, but for a different reason. The lectures were at that time arranged as eight self-standing units, each examined separately, and the examinations held early in the Spring term related to lecture courses that had been already completed. The marks were in this case given the same weighting as those from similar papers taken in June. The same principles were applied to the Stage III examinations a year or two later. These general practices continue up to the time of writing (c.1980).

The award of a numerical mark for laboratory work in Stage III, to be added in to the total for the year, dates from the late 1960's. Previously, a student's laboratory record would be "taken into account" in the discussion at the examiners meeting. The introduction of project work into Stage III, seen as an important element in a student's education, was a strong reason for giving explicit recognition to his performance. This was difficult so long as only half of the class were doing projects, and in 1966/7 laboratory work was given only a modest weighting of about 13% - the same as one of the written papers. But by 1968/9, when the project scheme had been extended to the whole class, the weighting had increased to 24%, and it has stayed at about this value ever since. (In passing, one may suspect that this considerable weighting has contributed to the attitude of the students in taking it so seriously.) At 24%, the mark constituted a considerable fraction of the total assessment, and, as a result, an elaborate scheme of double marking and cross checking was devised, in an attempt to ensure that, as far as possible, justice was being done. Two people, a "supervisor" and an "assessor", evaluate the work of each pair of students independently, and the results are discussed at length at a meeting of all those involved in the exercise. This complication is thought to be necessary, since the large size of the class (of the order of 100) makes it impossible for any one person to make any sort of valid comparison of the work of all the students. In Stages I and II, where the laboratory mark carries less weight - about 10% - and the fine detail of the total mark list is less vital, less elaborate arrangements suffice.

A second general feature of the period has been a tendency - deliberate, at least on my part - to move away from the strong reliance on the traditional type of examination question. This depended heavily on the ability of the student to remember, and reproduce, the content of lecture courses and, to a lesser extent, of text books. It has seemed to me that there are other attributes which are, collectively, more important as indicators of a student's ability as a physicist. The increased weighting given to laboratory work, just discussed, is one example of this trend. A second was the institution of the General Paper. This was normally a three-hour written paper of the usual format, but the questions were all in the nature of problems to be solved, and did not relate to any specific lecture course. They were sometimes described as "difficult questions on elementary physics", and a wide choice was always offered. Sometimes a (compulsory) part of the paper consisted of a larger number of short-answer questions. Sometimes one of the problems proved to be too difficult to be done in the time available: this was not a serious defect - a useful indication of the quality of a student was given by his unsuccessful attempts. The element of recall was kept as small as possible, and the emphasis was on the application to unusual or unfamiliar situations of knowledge of basic physics which should be the common property of all physicists. Usually the answers to the questions were of a rather low standard, and, not surprisingly, the marks obtained did not correlate well with those obtained on the more traditional papers. So far from being a criticism, this has always seemed to me to be a considerable virtue. The general paper clearly called into play some type of ability not tested in the remainder, an ability which, in my view at least, is an important attribute of a professional physicist. The steps that we took to try and improve the level of performance have already been described in a previous section. A general paper was introduced into the Stage III examination in 1967 - or possibly a little earlier,- and into Stage II in 1971. Both carried the same weight as an ordinary paper, or sometimes up to 50% more. They have not been popular with all members of staff, but I am pleased to note that both still exist (1985).

A third, and more controversial, step in the direction away from traditional examination technique was the introduction, in 1968, of "open book" examinations. Students were allowed to take in to the examination room any quantity of their own manuscript notes, but no text books. This last limitation was made primarily to preserve the library. The change was aimed more at the examiners than at the students. Faced with the new situation, it was hoped that they would cease to set questions which depended mainly on pure recall of lecture material. The examination would serve to find out not so much what the students knew, but rather to discover whether they could use what they knew. The examination situation became less artificial, and more like the real world. An investigation, carried out a few years later, showed that this objective had indeed been achieved. Bristol papers had a much lower content of memory work than any other one of a sample of some half-dozen other Universities. Some papers, particularly Quantum Mechanics, remained in the closed book form, since it was said to be impossible to set problem-type questions which would not be either trivially easy or impossibly difficult. The technique was also never applied to the general paper, where it would have been inappropriate, as well as useless. It was originally introduced into Stage III papers, and in the following year it spread to Stage II. Modifications were subsequently introduced. Some discretion was allowed to individual examiners to set either open-note or closed-note exams, provided that the students were informed of the decision well in advance. Some staff preferred to issue to their class during the year a fairly comprehensive list of all basic facts, relevant formulae etc. - in fact, skeleton lecture notes - and these were permitted instead of the student's own notes. In spite of some opposition and criticism, the system still (1985) survives in principle, and has undoubtedly produced a more reasonable type of examination paper over the years.

A fourth, and rather, smaller step in the same direction was taken in 1971/2, when a weight of 5% of the total mark in Stage II was given to the marks obtained on an essay written in the student's own time in the Spring term. This took advantage of the introduction by Professor Ziman of a course of lectures on "Science and Society". (After Ziman's departure, it acquired a slightly different emphasis, and was renamed "Case-studies in Science".) Our purpose in requiring this essay was to give the student some practice in writing continuous and coherent prose, which was otherwise only called for, and that imperfectly, in accounts of laboratory experiments. It was given a weighting in the Stage II assessment more to induce the students to take it seriously, rather than for any effect it might have on the final order. The difficulty with this, as with so many desirable reforms, is that if it is to be well and fairly done, it is expensive in staff time.

Finally, mention should be made of two other features of the examination arrangements which emerged during the period under review. 1966 saw the establishment of the first "vetting panels". These were groups of staff members, usually four or five in each group, charged with the duty of checking the draft examination papers before they were sent to the External Examiner for approval. This was a job that used to be done by the Head of Department, or his nominee, but as the number of papers increased, and their subject matter diversified, it became too much for one person. The duties of the panels included the obvious one of removing mistakes of all kinds, and the less obvious, and more difficult, one of ensuring that each paper as a whole was reasonable in content and standard. Co-operation from the question setter was essential, and was always forthcoming. Ideally, model answers were to be submitted along with the questions, for the information of both the panel and the external examiner, but these did not always appear. Initially there were two panels, later increased to three. Their responsibility covered all examinations set. The system is now well established.

The introduction of Optional courses in Stage III brought with it a problem of its own. In spite of all that a vetting panel could do - and this was often not very much in this case - there were inevitably variations of standard between one optional paper and another. Since the students were free to take whatever selection of courses they pleased, this introduced an undesirable element of arbitrariness into the final mark. A crude method of applying a correction was to adjust the marks on each Optional paper so that all had the same mean. But it was soon realised that a better method would allow for the possibility that those students taking option A were, on average, brighter than those taking option B, and would thus be expected to obtain a higher mean mark. A second approximation therefore used the mean mark obtained by each group on the compulsory papers to provide a calibrating factor that would allow for this. Experience soon showed, however, that the corrections were always quite small, and thus fairly crude methods of applying them were sufficiently accurate. A suitable simple procedure was evolved, sometimes called the Devonshire correction, and this is still used.

(e) Graduate Teaching.

During the period of my memory, there have always been courses of lectures at post-graduate level in the Physics Department. I recall listening to lectures on cosmology by Walter Heitler, and he left Bristol in 1941. The earliest record on paper dates from 1949, and announces lectures on the Theory of Solids, Wave Mechanics, and Nuclear Forces, but I would be fairly confident that Mott had given lectures on Dislocation Theory before that date. In the 1949 announcement, the lectures on solids were given by Mott, and were "intended for students starting research in theoretical physics": this purpose has always been the primary motivation. The topics dealt with have either been basic subjects, such as wave mechanics, group theory etc., or else have reflected the research interests of the Department at the time. Dislocation Theory, given by Frank and Eshelby, appears in 1950, as does Dielectrics, given by Devonshire. The scale of the provision in the mid-1950's was to have about four courses running simultaneously, at the rate of one lecture per week, of which first-year graduate students were "expected" to attend two. By the mid-1960's it had grown slightly, and added up to more than 100 lectures in the year. The Department has always been - and still is - exercised by the problem of persuading graduate students to continue their general education in physics in this way, against the pressures exerted by their research supervisors (sometimes) and their own inclination (always) to concentrate on the special problem which constitutes the subject of their research. In 1957, for example, there was a Post-graduate General Examination at the end of a student’s first year, consisting of a three-hour written paper. Candidates were instructed to answer two questions (out of about twenty) "one of which should be on, or near the field of your research, while the other should be on a field remote from your speciality". There was a similar paper in the following year, but surviving records do not show either when the practice was started, nor when, or why, it was discontinued.

In 1960 consideration was being given to the possibility of starting an MSc course on the Physics of Materials. This course, in fact, existed from 1965/6 to 1982/3. It is discussed in more detail in a separate section later, but we may note here that, while it existed, the lecture courses given were often included in the lists of courses open to PhD candidates who were not registered for the MSc. A circular dated 1961 announces a 30-lecture graduate course on Solid State Physics, to be given by Lang, Frank, Chambers, Kay and Thompson. This was repeated in the following year with a slightly different team giving 50 lectures. There was then also a similar course on Nuclear and Elementary Particle Physics, given by Fowler, Perkins, Gibson, Bowler and Powell. There appears to have been also a parallel course on Wave Mechanics, given mainly by Pryce, but details have not survived. These developments showed not only an increasing effort, but also increasing co-ordination. The whole question of post-graduate teaching was discussed at some length at a staff meeting in 1966, and it is interesting that the first of a number of conclusions reached is concerned with the necessity of "some kind of filter" before graduate students even start on their first year. This probably arose from one or two problems relating to students from Middle Eastern countries. It was - and still is - difficult to form an estimate of the standard of their previous education, which may not have been such as to enable them to profit from the courses taken by our own graduates. The problem is exacerbated by the fact that, in many cases, if the student returns home after the prescribed period of his stay without having obtained a PhD, he may be expected to repay any grant that he has received, and will, in any case, have lost face to such an extent that his future career will be in jeopardy. Having had one or two rather distressing cases of this kind, we wrote a lengthy and carefully worded memo., tactfully pointing out that acceptance as a graduate student did not automatically lead to the award of a PhD after three years of residence, and spelling out in detail the other possibilities. A copy of this was sent to all overseas applicants before they were accepted. The same staff meeting also put it on record that it was the responsibility of a student's research supervisor to assess his potentiality for research at the end of his first year. It was also thought that some incentive, in the form of a test, was necessary, but that this should not take the form of "the present May examination".

Around 1969 Ziman took over the responsibility for organising the graduate teaching. By this time a distinction was being drawn between those groups of lectures intended for first-year graduate students, the adjective "theoretical", used by Mott, has been dropped) and other, more advanced courses on more specialised topics. Ziman's scheme formalised this distinction by introducing two groups of lectures, “A" on basic material, to be repeated annually, and "B" on more specialised topics, to be repeated on a two- or three-year cycle. In this way, any graduate student had the opportunity to attend any set of lectures during his stay in the Department. The arrangement has persisted in broad outline, although it has not by any means been strictly adhered to.

The related problems of incentives and assessment, already mentioned in passing, have had a more varied history. In 1966 the authorities imposed a stricter three-year time limit on the duration of a PhD course. This caused the Physics Department to be more flexible about its requirements for students to attend post-graduate lectures. First-year graduates were still "expected" to attend a selection of these, but no firm rules were laid down. The policy was set out in some detail in a memorandum by Ziman, first written about 1969, and issued unchanged from year to year until at least 1974. This starts with the unequivocal statement that "It is the agreed policy of the Physics Department that all graduate students should attend some courses of formal instruction in addition to carrying on with their research." It goes on to place on the supervisor the responsibility for both advising on the courses to be taken and reporting on the student's progress. However, the lecturers giving the courses were to "devise some method of assessing the progress of those attending" and advise the supervisor of the results. It was suggested that attendance at three of the courses in the A-group should be regarded as a minimum for a first year student. Although it had ceased to be a formal requirement that students should attend some courses unconnected with their research topic, they were strongly advised not to neglect this aspect of their education. A glance at the list of names of the staff involved in giving the lectures shows that theoreticians considerably outnumbered experimentalists. Even the courses given by experimentalists tend to have a theoretical flavour, while those given by theoreticians could often be described as mathematics. All this is understandable, although not perhaps inevitable. A proposal for some of the lectures, given by outside speakers, to be on industrial or commercial topics, on finance, management, organisation and the like, which was made at one stage, seems to have been ignored. As far as lectures are concerned, the only contact which the graduate students have with those topics, even within physics, which are not among the research interests of the Department comes from the regular Monday colloquia - and by no means all of these fit the description.

This system remained nominally in existence for several years, although the extent to which it was put into practice varied considerably from one member of staff to another, and things were gradually allowed to slip. A memorandum dated 1975 notes that "this assessment is no longer taken very seriously", and puts forward suggestions for a revised procedure based on prescribed text-books rather than attendance at lectures. However, lectures of the same kind, and in about the same numbers continued to be given. A discussion of the success of new solutions to the problem of graduate education would take us outside the period covered by these notes, but it may be in order to record that in 1975 the SRC set up a "working party", and the CVCP, had a "study group" on postgraduate education. The former placed great emphasis on course work other than the main research project, and suggested that, for potential PhD candidates, there should be compulsory course work in their first post-graduate year, with some kind of assessment hurdle before they were allowed to embark on the second year. An important sentence in the conclusions of the latter body suggests that the suitability of a PhD candidate should be independently assessed, i.e. by somebody other than his research supervisor, at the end of his first year. Most of the suggestions, in both papers, were not new to us in Bristol, but we did adopt the practice of appointing a small "panel" for each student, to share the responsibility for critical decisions with his supervisor. But I suspect that the problem of postgraduate education is not yet solved.

(f) Extra-mural Teaching.

The story would not be complete without a mention of the extra-mural teaching undertaken by members of the Department. My own recollections start from 1962, but I would be surprised if there had not been similar activities before then. In that year Mike Smith joined the Department as Administrative Officer, having previously been on the staff of the Extra-Mural department. His previous place was taken by David Wilde, but he continued taking extra-mural classes for many years, mainly on topics related to astronomy. Wilde proceeded to build up a strong connection with the Physics Department. He persuaded several of our staff - including Gugan, Parsons, Milne and, notably, Hillier - to give courses in various parts of his empire. In addition, the Department was happy to provided him with facilities in the evening in which he could conduct his own courses on electronics and computing, which often involved an element of laboratory work. Both Smith and Wilde have now (1988) retired, but I understand that the co-operation between the two departments continues to flourish.

CHAPTER 6

PREMISES

The Bristol Physics Department can, quite properly, be regarded as a memorial to the life-work of Arthur Tyndall. While nobody, least of all Tyndall, would maintain that the standing of a department is determined by the buildings that it occupies, nevertheless, spacious, convenient and well-equipped premises are an asset when attempting to recruit first rate staff - and students. Tyndall regarded the erection of a fine laboratory as the first step towards the building of a fine department. For these, if for no other reasons, it seems to me that no apology is needed for giving an account of what we, the subsequent inhabitants, have done to the building that we inherited.

The laboratory was formally opened in 1927, although it had been in at least partial use for some time before that. An account of the opening is given in Tyndall's History. When I arrived in 1933, it was still far from fully utilised, one or two of the rooms being almost empty. There was little change in this general state of affairs until 1939, the increase in staff numbers in this period being very small. The arrival, during the war years, of the staff and students from Kings College, evacuated from London, must have involved a fair amount of squeezing, but since I was away for the whole of the period, I had no personal experience of it, and, so far as I know, there are absolutely no written records of what happened. The situation was made worse in May 1940, when the entire second floor of the building was taken over by a section of the Admiralty Signals School, evacuated from Portsmouth as a precaution against enemy invasion. By the time I returned in 1945, both Kings College and the Admiralty had departed, and things had almost returned to normal. In 1944, another Admiralty department had erected a pre-fabricated corrugated iron building - a "Nissen hut" - on the site of the present workshop. This was left behind when they departed, and was quite useful, serving first as a woodworking shop (with a circular saw, planing machine, pillar drill and the like), and subsequently being used to house the very large annealing oven needed for bending the glass plates for the Schmidt camera project. (See Section on Applied Optics, p.66). The hut was not removed until 1955, when it disappeared as part of the site clearance for the workshop.

After the end of the war there was a considerable increase in the number of students (ex-service people), and of staff, (those returning from secondment, and new appointments), and space was at a premium. Powell's group on the fourth floor, in their efforts to manufacture ever bigger balloons, had moved into the space under the glass roof. Originally, this had been intended only for the storage of the usual collection of junk, which no physics department can bring itself to throw away, "because it might come in useful". Around 1949, the group also began the process of building little cubicles along the west side of the same space, and continued until there was room for no more. These were used as offices by staff and research students, and were irreverently known as nesting boxes. About the same time, another useful development began. Tyndall has remarked in his History on the unnecessarily great height of the rooms in the original building. In 1949, by way of experiment, we caused to be erected a balcony in one of the rooms occupied by two of my research students. On this we could place writing tables, chairs and bookshelves, so freeing the floor-space for experimental equipment. There was just enough head-room, both above and below, to make this possible. Once it had been shown to be feasible, balconies in similar style proliferated all over the building. In the following year a further small increase in laboratory space was obtained as a by-product of a move made primarily for different reasons. One of my students had built a fatigue machine which operated at 1000 Hertz, and was very noisy. It was banished from the laboratory proper to a "temporary" hut built on the flat roof of the ground-floor room now known as G.37. As will be explained shortly, the first-floor rooms at that stage did not extend beyond the present 1.30. The hut stayed there, in full use as a laboratory for 2-3 research students, until about 1963, when it was demolished as part of the operation of erecting the extension to the building - and re-erected in the grounds as a gardener's shed. It was around 1950 also that several of the theoreticians, both staff and students, occupied rooms in Royal Fort House, and in one of the houses later demolished to make room for the Main University Library.

A more substantial increase in floor area resulted from the erection of the present workshop. The first sketches were prepared in February 1949, the proposal being based on the case that the existing shop was too small to cope with the demands being put on it. However, there were numerous delays, due either to the unwillingness of the UGC to make available the necessary finance, or to the unwillingness of the University to give the job the necessary priority. During the delay, the proposed building grew steadily larger, until it was just about as big as could possibly be built on the available site. As with the main building, it was unnecessarily large for its immediate use (although we would not have admitted this at the time), but prudent forward planning suggested that we should ask for as much as possible. Even so, by 1954 it was being described as being too small. Building work was eventually started in 1955, and completed by April 1957 - only eight years after the original proposal. The space thus freed on the ground floor of the main building was adapted for use by the low-temperature group, not only for research apparatus but also to house compressors, liquefiers etc. It is now known as G.35. The glass-working shop on the second floor was also moved out to the new workshop, thus giving two extra laboratories for teaching and research. A comparable gain was achieved about the same time by dividing horizontally the very large room on the fourth floor of the building, in the tower. Because it was so tall, it produced rooms of reasonable proportions even when divided into two. A similar proposal had been made in 1946, but rejected at that stage because the resulting floor loading was said to be unacceptably high. The 1957 solution makes use of two large I-section girders spanning the room from east to west, with the upper rooms divided by extremely light-weight partitions. Even so, these rooms are suitable only for use as offices with no heavy equipment.

In 1958, another, even greater, gain resulted from the departure of the Department of Mathematics into the recently completed Engineering building. From the outset, they had occupied the whole of the third floor of the Royal Fort, except for the library, which they shared with physics. As a result, there were no services - gas, water, etc. - on the third floor, and when we took it over we decided to let it remain so. Part of the space was used for a much-needed extension to the library. One of the mathematics lecture rooms was retained as such, so as to help cope with the growing programme of undergraduate teaching. The rest of the space was used, with only minor modifications, to serve as the administrative centre of the Department, with offices for the Director, one or two senior staff members, and most of the secretarial staff. The original Director's office, a spacious room on the ground floor, calculated to impress official visitors, was converted into a laboratory, now G.26.

All these piecemeal modifications had resulted in an increase in the usable floor area available to the Physics Department of about 15,000 sq. ft. Of this total, the workshop was the biggest single item, (about 5000 sq. ft.), and the only one consisting of new building, as distinct from modifications and adaptations. The corresponding figure for the original 1927 building was about 42,000 sq. ft. of usable area, i.e. excluding corridors, stairways, cloakrooms etc. The increase was thus about one-third of the original area. However, in spite of these additions, it was becoming apparent that a more substantial increase was needed. In December 1955, the UGC had written to all Universities calling attention to the "bulge" in the population, resulting from the post-war baby boom. The cohort of students in question would reach university age about 1962. This peak would be superposed on the general increase in university provision which followed from the implementation of the Robbins report. Bristol replied by proposing an increase in its undergraduate population of 30 to 40%, rather more for science, and rather less for arts. There was much discussion, at both departmental and faculty level, aimed at translating these general intentions into numbers of students, and, in our case, into the increase in floor area that would be needed to accommodate them. A detailed study carried out early in 1957 suggested that an additional 22,500 sq. ft. would be needed in Physics, and this estimate was sent to the Developments Committee. The position was fully reconsidered in February 1959, and a closely similar result was obtained. At that time, the University's forward building programme envisaged an addition of 7,000 sq. ft. to the Physics Department, to be provided in 1962 by completing the two northern-most bays of the existing building.

This last phrase calls for some explanation, which will also serve to make clear several points in the following pages. When the laboratory was opened in 1927, the building represented only part of the architect's grand plan. What that plan was need not concern us here, since, like so many grand plans, it never came to fruition. What existed in 1927 was the tower block and lecture theatre unit facing south towards Royal Fort House, and most of the north-south arm of the building. This terminated in a manifestly unfinished, north-facing gable end, decorated by a fine display of English exterior plumbing and a singularly ugly iron fire escape. Doubtless this last was there to satisfy the fire regulations, but it also provided a very convenient secondary entrance to the building for those people who had keys to the doors leading from it. This eyesore remained unaltered until it was engulfed by the big extension, started in 1962. As can be seen in the plans and photographs which still survive, the east and west sides of this main arm consisted, on the outside, of a series of bays, separated by buttress-like structures. (They are still there, of course.) In 1927, there were nine of these bays that were (almost) complete at ground floor level, but only seven existed at first floor level and above. This produced a flat roof over those rooms now designated as G.37, G.38 and G.39, and it was on this roof that the hut mentioned on page 108 was built. The proposal mentioned at the end of the preceding paragraph would have continued the building upwards above the two existing ground floor bays, presumably in the same architectural style as the original, and presumably still leaving an unfinished gable end.

In 1959 a new complication arose, which proved to be of the greatest importance. The building of the new chemistry department and the new medical block, together with the growth of other departments, would produce a demand for water in volumes, and at a pressure, which the existing mains could not meet. The solution proposed was to build a high-level storage tank to serve all departments, and the obvious place for it was on top of the Physics Department, the highest point in the University precinct. The foundations of the existing, uncompleted, building were not adequate to support the extra load, and so an extension, even at ground level, would have to be built. This requirement was not only important, it was urgent: the target date for completion was 1962. When added to the need to complete the Physics extension in time to cope with the "bulge", it produced a very strong case in all the discussions which followed. The architects for the scheme were to be Oatley and Brentnall, the successors of the firm of Oatley and Lawrence, who had built the existing Department. Mr. Brentnall's first suggestion was to extend the existing building northwards towards Tyndall Avenue, and to put the tank on top of the new part. This would produce about 16,000 sq. ft. of new floor area for physics - a good deal more, and therefore more expensive, than the 7,000 sq. ft. provided for in the 1962 building programme.

However, Pryce pointed out in October 1959 that it was also a good deal less than the 22,500 sq. ft. that was the most recent estimate of our immediate needs. He suggested the addition of a short stub wing extending westwards from the proposed extension. This would bring the floor area to about the required value, and would also permit the inclusion of the urgently needed extra lecture theatres. Lecture theatres, which need unobstructed sight lines, could not be placed under the water tower, because of the need to have numerous (16) large columns to support the additional load. Future extensions could then be in another wing, extending eastwards along Tyndall Avenue. Three months later, Pryce again wrote to the Bursar, setting out in some detail our departmental proposals for expansion beyond the immediate future. He pointed out that the existing building, although overcrowded, had been partially empty when first opened 33 years previously, "However boldly we plan, we are unlikely to get a building that will satisfy our needs for more that 30 years ahead." To attempt forward planning for longer that this was unrealistic. (In passing, we may note that at the present time of writing, some 28 years later, the building is again beginning to be overcrowded.) Pryce gave a detailed list of requirements, which added up to 49,000 sq. ft., of which 40,000 were for named uses in the immediate future, and the remainder for unforeseen developments. A few weeks later I wrote another long letter to Mr. Brentnall, setting out the general principles which, we suggested, should guide the layout of the various rooms. These included the proposal that no attempt should be made to separate the "teaching" and "research" functions of the building, as was being done in some other places. It seemed to me that it would be good for both parties to be made aware of the other's existence.

At this point, progress became more formal, with the setting up of a Scheduling Committee, and correspondence with the UGC and with the architect's office, all resulting in more detailed proposals. The UGC had agreed with our suggestions in broad outline, and this more detailed work was felt to be justified. The results were discussed at length at a full meeting of the physics staff in May 1961 - already more than a year after serious discussions had begun. The architectural problems were not simple. The impossibility of fitting large rooms under the water tower has already been mentioned. The other major design problem concerned floor levels. As stated above, the ceiling heights in the existing building were unusually large, and it was desirable that, in the extension, they should be reduced to a more economical value. Unless we were to have two completely separate buildings - an idea that was strongly opposed by the whole Department - the only solution was to introduce a change of floor levels at the point where both sets could be served by the same staircase and lift. For economy, the amount of new building that would perpetuate the old floor levels should be kept as small as possible. There was also the additional complication that the axis of any new wing would be at right angles to that of the old building. Brentnall's solution to this group of problems, which was essentially that which was eventually built, was generally agreed to be ingenious and satisfactory. Another set of problems related to the lecture theatres. It was the intention from the outset that they should be suitable for, and available for, public use when not needed by the University. The numerous evening meetings held in the two older theatres showed that there was a demand for such a facility. The theatre wing was therefore designed as a self-contained unit, with cloak rooms, lavatories, a committee room, a kitchen etc., which could be shut off completely from the rest of the building if need be. Tyndall Avenue was then, as now, a bus route, and carried a good deal of other traffic as well. It was important that the resulting noise be kept out of the theatres; this is the reason for the balcony-like structure which gives access to the foyer of the large theatre, and for the foyer itself. It is also the reason why neither theatre has any windows.

The detailed specifications produced in November 1961 showed a total floor area of 59,000 sq. ft., excluding those parts which belonged to the water authority. Of this, about 20,000 sq. ft. was devoted to corridors, staircases, lifts, janitorial services, ventilation plant etc., There thus remained some 39,000 sq. ft. for lecture rooms, laboratories and offices, which was a good deal closer to our original request for 49,000 sq. ft. than to the starting point of 7,000 sq. ft. The general layout was essentially that which we had suggested, and subsequent modifications were not substantial. In particular, it retained the principle of having no laboratories on the third floor. Instead, in addition to professorial and administrative offices, it made provision for the increasing number of theoreticians on the staff. The building operations were conceptually split into two parts. Phase I included the completion of the northern extension of the old building and the erection of the water tower. Phase II comprised the short west wing, including the large lecture theatre, and the much longer eastward wing housing mainly research rooms. The primary reason for this split was to spread the cost over several financial years, but it had the incidental advantage that the two urgent elements, the water tower and one lecture theatre, would be available earlier than they would have been if the whole construction had proceeded as one operation.

At this point everything looked like plain sailing, but it transpired that there were several uncharted shoals ahead. When detailed planning had proceeded far enough to permit more precise estimates of costs to be made, the UGC commented that the total seemed to be excessive, and in particular, queried the need for another lecture theatre seating 300, in addition to the one already in existence, which seated 380. Our detailed justification was based mainly on grounds of general University usage, rather than purely departmental considerations. It is possible that these exchanges with the UGC were more part of a ritual rather than real opposition. The objection must have been withdrawn, because the planning went ahead. In July 1961 there was another hitch, when Bristol City planning authority objected to our proposals because they involved a tall building along the south side of Tyndall Avenue, which would obstruct the light to the houses on the north side. The argument that these houses would be removed within a very few years to make way for further University building seemed to carry no weight. (In fact they were demolished soon afterwards to make way for the University Main Library and the adjacent car park.) In the end, it was agreed in 1961 to set back the physics building from the road by a further distance equal to the width of one bay of the old building, i.e. 17 ft. This meant the loss of an appreciable amount of floor area in Phase I of the new construction, but, being in the architectural style of the old building, both inside and out, it was expensive area that was lost. In retrospect, the decision to set back the building further than was originally intended was probably a wise move on general amenity grounds.

There were also more financial worries. In June 1962, the UGC had still not given their final agreement to the proposals, and wrote pointing out that it would be cheaper by about £50,000 to provide a separate new building, rather than an extension of the existing structure. They may conceivably have been right. But anybody who remembers the external appearance of the unfinished end of the original building will be pleased that the arguments we were able to produce in favour of completing it prevailed in the end. Preliminary work of site clearance was already in hand. This involved the demolition of the first pair of houses along the south side of Tyndall Avenue, and the removal of various temporary buildings that occupied the rest of the site. This kind of work, and the construction of the foundations for Phase I, continued during 1963 at the same time as the detailed design of the superstructure.

The foundation works were complete by the end of 1963, and were formally handed over by the contractor to the University on January 9th. 1964. On the same day a letter was received from the UGC, saying that the estimated cost of Phase I was 30% too high (i.e. £125,000) and suggesting that the whole scheme should be scrapped, and that we should start again and produce a design for a separate, free-standing building. Not surprisingly, this caused some consternation. Part of the original reason for the extension of the laboratory was to cope with the increase in student numbers - "the bulge" - which had already begun in 1962. The estimated time to complete the building operations was not less than three years. Any further delay in starting would clearly delay the completion date, which was thus already well behind schedule. The reply from the Bursar to the UGC set out a number of convincing arguments why their limit on expenditure was unreasonably low. Perhaps the most telling was that it was unfair to consider Phase I in isolation, since it included all the service areas - staircase, lift, foyer, cloakrooms, stores etc. - that would serve the whole of the extension, and so inflated the cost of the remaining "useful" floor area enormously. The complications arising from the necessity of incorporating the water tank were also expensive, and all fell on Stage I, even though the cost of the installation itself would be borne by the water authority. Any further delay, caused by the suggested re-design, would bring in its train additional costs for temporary accommodation for the Physics Department, and temporary expedients to maintain the water supply. And finally, of course, the massive foundations for the tank itself had already been built. The Vice-Chancellor backed up the official University reply to the UGC with a long personal letter to its chairman, making more or less the same points in his own inimitable style.

In these ways, the UGC was persuaded to raise the limit of what it thought was a reasonable cost. At the same time, staff from the University, from the architect's office, and from the UGC discussed ways in which costs could be reduced. In February 1964, a list of 22 possible savings was produced. The biggest single item arose from the replacement of the interior teak doors, designed to match those in the existing building, by painted soft-wood. The second item, almost as large, arose from the replacement of the proposed Bath stone cladding of the exterior walls by concrete slabs faced with re-constituted Bath stone. From a utilitarian viewpoint, neither of these changes represented any hardship. A proposal to economise on the ventilating system was successfully resisted. In the old building, air is drawn in through grilles below the windows, and so passes by the hot water pipes which constitute the heating system. Each grille is fitted with a set of louvres which can, in principle, be shut if desired. Within my recollection, these had never worked, and any attempt to make them do so merely increased the amount of dirt which entered the building via this route. The new part, at our request, was to be ventilated by a "blow" system, rather than this "suck" arrangement. Air would be drawn into the building through a single large grille at high level, filtered, heated, and distributed to the rooms through ducting. This is a more expensive arrangement, but it does mean that dirt is blown out rather than sucked in. One of the economies suggested was to scrap this proposed system; but an orchestrated chorus of complaint from the staff doing experimental research proved sufficient to avert the calamity.

In the end, the gap between our estimate of what the building would cost, and the UGC statement of what they were prepared to pay, was reduced from the original £125,000 to £30,000. At this the UGC authorised us to proceed - provided that the University would find the £30,000, in addition to the £13,000, which was what the University had already proposed as its contribution to the savings. The Bursar agreed to this proposal, saying that he could find the money by cutting the budgets of other projects under consideration. Eventually, the contracts were placed, and work actually started on May 1st. 1964. Serious discussion of the plans had begun in the University as early as 1960, and our own departmental planning had started some three years before that again. The completion date for Phase I was set, hopefully, at February 1st. 1966, and as the work proceeded, it became clear that the contractors might be able to improve on this. They were very co-operative, and, after some discussion, they agreed to finish the second floor in advance of the rest. They actually succeeded to the extent that we could begin to move in on August 1st. 1965, and be ready to receive the students when they arrived at the end of September. The lecture theatre G.44 was completed in December 1965 as part of Phase I, and in the following month it, and some of the other new accommodation, received their baptism by being used for an outside conference.

In the course of the operations we managed to insert another item of "minor works" into the programme, which was, in fact, rather larger than the word minor might suggest. The original building had a massive stone balustrade along its whole length, at third floor level. This can be seen in the old photographs. It very effectively blocked all view from the third floor rooms, and made them rather gloomy and claustrophobic. I began to agitate for its removal in 1963, suggesting that it might be cheaper to remove it than to continue it across the top of the additional bays that were to be erected in the old style as part of the extension. We got the agreement of Mr. Brentnall, who described it as "not one of Sir George's happiest thoughts". (Sir George Oatley was a partner in the firm of Oatley and Lawrence, which built the original laboratory.) It eventually came down in 1964, and I believe that it was re-erected along the edge of a terrace in one of the Halls of Residence, in Stoke Bishop.

Detailed planning for Phase II was under way in December 1963, even before the building of Phase I had actually started. This time, everything went much more smoothly. We made a bid for an extension of the eastward wing, which formed the major part of Phase II, on the grounds of a continuing increase in the pressure of likely student numbers. No objection was raised, either by the University or by the UGC: all that was involved was the demolition of another house on Tyndall Avenue, and the extension of the wing by two more units. This was cheap accommodation; the expensive bits had already been provided in Phase I. Similarly, in 1964, the question of the "Student Discussion Room" was raised. This was the name adopted for tactical and diplomatic reasons for the space under the new large lecture theatre, which we envisaged (correctly), would be used as a student common room. It was originally planned to be half its present size. It was suggested (by the architect, I think) that it would be very cheap to dig away a bit more of the ground under the theatre, and so double its size. We were not averse to the suggestion, and this proposal too was accepted without demur. It has proved to be a wise move, since the large room is very valuable as an exhibition room in connection with conferences, open days, and similar events. The only problem that arose during the building of Phase II was that the date of starting was delayed by six months as a result of a government edict which imposed a moratorium of this length on all new building starts by the UGC in 1965. Work on the site actually began in July 1966. There were no major changes or difficulties thereafter. I was away for the year 1966/67, and all the detailed liaison with the architect and the builders was handled in my absence by Kevin Tindall. I can find no record of the formal "handing over" date, but it must have been around midsummer in 1968. This gives a total gestation period for the whole operation of building the extension which is rather more than ten years. We managed to get the workshop in rather less than ten years - but that was a much smaller job.

In the twenty years which have since elapsed, up to the time of writing, there have been no further additions to our premises. There have been many minor changes in the use to which some of the rooms are put, but these have been arranged without difficulty. The annual intake of undergraduates has risen to 100-110, and the Department is still coping. Complaints about overcrowding are reaching the point where some action is called for, but whether any action is taken will probably depend primarily on national policy in relation to education and research in physics. Apart from questions of size - and possibly of aesthetics - the only serious criticisms of the building which have arisen relate to heating and ventilation. These always seem to be difficult points in any architect-designed laboratory. In the early stages, we completely failed to convince our architects that heating a room from the top was not the best way to do it. Not only our architect, but the whole profession was ranked against us. The design of the building really involves too many windows. They do give adequate lighting, but the rooms on the north side of the new wing are cold in winter, while those on the south side are hot in summer. Again, our attempts to have double-glazing installed failed because of the argument - short-sighted as it seemed to me at the time - that the capital cost was large, while heat from the University's central boiler house was cheap. Cheap it may have been, but even so we soon found ourselves involved in a long campaign against the heating engineer to obtain an adequate supply. But on the whole, the architects provided us with a serviceable and flexible building: and Pryce has been justified in his policy of planning on as large a scale as circumstances permitted.

CHAPTER 7

STATISTICS

One feature characterising the history of the Department throughout its life has been a continuing increase in size. This has followed the general increase in the scale of tertiary education during the century, and the increasing importance attached to science in general and physics in particular within the tertiary sphere. The driving force for expansion usually came from outside the Department, and indeed, outside the University. However, the academic staff did not actively oppose the growth. It seems to be a general phenomenon that any academic, at any time, thinks that the optimum size for a university, / faculty, / department, / research group is about 10%

bigger than its present size, whatever that may be. In the immediately following paragraphs we illustrate, and comment on, this growth, first in terms of undergraduate numbers, and then in terms of graduate and staff numbers. Finance gets a chapter to itself.

The increase in size was accompanied by some major shifts in the balance of undergraduate teaching, and these are described in some detail. Tyndall's History has very little to say on these matters, and so this present account covers the whole period since the founding of the University. It is also carried up to the present time of writing (1988), instead of stopping at 1975, which was my original intention.

(a) Undergraduate numbers.

Information on student numbers comes from four different sources:

(1) From the date of foundation up to about 1949 there were Annual Reports from the Head of Department, and also from the Dean of the Faculty. Most of these have survived, and where the two sets overlap they agree fairly well.

(2) The annual Faculty Lists, which give the names of the undergraduate students in residence, and the courses that they are taking, are complete from 1928/29 onwards, and one or two earlier ones also exist. They do not always agree exactly with the data mentioned under (1), the major discrepancies probably arising from the fact that the two documents would be prepared at a different stage during the academic year.

(3) Tyndall's History gives two graphs showing numbers of students in some categories - but not all. Nor is it always clear who is counted in which category. And the numbers have to be read off from rather small graphs. Again, the agreement is not perfect, but is quite adequate for illustrating general trends.

(4) The above are all concerned with numbers of students in residence. The annual Calendar gives lists of the students who graduated each year, and these have been used to fill in one or two gaps in the early data.

Curve A in Fig.1 shows the number of students who were candidates for the honours degree in physics in each year. This serves as a convenient starting point, since the category is well-defined, and the set of data is complete. Fig.2 gives the total number of students in the Faculty of Science, in each year, who were registered for a course in physics, of some kind. Not surprisingly, the two graphs are of the same general shape. Note that in Fig.2, every student has been counted once no matter whether the course taken was an elementary one involving only three lectures per week, or a more advanced one involving ten. Since the balance between elementary and advanced teaching has changed considerably over the years, the graph is only an imperfect representation of the total teaching load. Note also that students taking the Intermediate course are included in the total, but not medical and dental students taking 1st. MB or BDS courses: of this, more later.

In the early years of the University, the customary undergraduate course led to an Ordinary degree: an Honours degree was rather exceptional. The degree course lasted for three years following "matriculation", and at the end of the first of these years one took the Intermediate examination. Students wishing to attempt Honours stayed for a fourth year, after taking the Final exams for the Ordinary degree. It was possible to obtain exemption from the Intermediate examination on the results of the Higher School Certificate examination, set by a University Examining Board, and taken at school. This practice became increasingly common with the passage of time, and the number of students taking Inter. correspondingly declined. This is very clear from Fig.3, in which curve A shows the number of students registered for Inter. There was also a group of medical and dental students taking courses of a similar standard as part of the MB, and BDS. curricula. This group was comparable in size with the Intermediate class. In view of the historical origins of the University in the Bristol Medical School, this is not surprising. If their numbers are added to those in the Intermediate class, we obtain the upper line, B, in Fig.3. It should perhaps be pointed out that Fig.1 and Fig.3 are plotted to the same vertical scale: this emphasises the importance of elementary teaching in the early years. There were also very small numbers taking physics as part of a degree in agriculture or domestic science or - occasionally - engineering. For our present statistical purposes these have been combined with the Inters. All of these groups were sometimes taught together, particularly for laboratory work. In the present account, the numbers of medicals, dentals, etc. are not included in the statistics unless specifically mentioned. By comparing the numbers represented in Figs. 2 and 3, we can obtain the fraction of all undergraduates taking physics courses who were studying at the equivalent of the present A-level. The following Table gives the value of this fraction, averaged over five-year blocks:-

Percentage of Physics Students taking Elementary Courses.

Period	Excluding Medics.	Including Medics.
1910 - 15	47	57
16 - 20	49	69
21 - 25	30	44
26 - 30	26	42
31 - 35	20	45
36 - 40	22	51
41 - 45	22	45
46 - 50	22	36
51 - 55	3	14
56 - 60	2	7

Two results are given, one for Science Faculty students only, and the other including also medical and dental students. It will be seen that, as late as 1945, about 40% of the students in the Department were taking "school" physics. However, the staff effort involved in this elementary teaching would be a good deal less than 40%, since the more advanced courses make a greater demand on staff time.

The decline of Inter. was a development that was not unwelcome to most of the academic staff, who always prefer advanced to elementary teaching. When I became involved in departmental administration, I felt that teaching at the elementary level could probably be better done in schools, where the staff have had tuition in the appropriate techniques and experience of using them, rather than in a university, where the lecturing is done by an inexperienced amateur, operating by the light of nature. I consequently maintained a steady pressure to abolish Intermediate classes altogether. I was not alone in the faculty in holding this view, and the step was formally taken about 1960. However, we had no control, and little influence, over the affairs of the medical faculty, and they maintained that there was a place in their world for university teaching at this level, in order to fill in one or two gaps in the school system. The result was that 1st. MB classes continued in physics and other science subjects, and still exist (in 1988). As a curious footnote, we find that, since they exist, the science faculty finds it expedient to make use of them from time to time, for the benefit of a student with an unconventional academic history. The course is dignified in the science faculty by the name "Physics E".

One might expect that the decline of the Intermediate course would have been accompanied by a decline in the number of Ordinary degrees, in view of the historical relation between the two. At a first glance this appears to be true, but in fact there is little direct connection between the two events. It is not possible to discuss this matter in relation to physics alone, for two reasons. (a) Unlike an Honours degree, an Ordinary degree is not awarded "in" a named subject: there are always at least two subjects of comparable importance that are involved. (b) The courses of instruction which were designed to lead to the Ordinary degree were used also for students who were taking physics as a subject ancillary to an Honours curriculum in another subject, often chemistry. Both of these difficulties can be avoided by considering the numbers relating to the faculty as a whole, and not just physics. In Fig.4, the line A shows the variation with time of the total number of students taking a Final examination, in any subject or group of subjects, for any kind of degree, in each year. In the same diagram, the line B shows the number of these who were candidates for the Ordinary degree. Combining the two numbers, we obtain Fig.5a, which shows the Ordinary degree candidates as a fraction of the total. The dramatic decline might recall the similar result for Intermediate candidates, (Fig.3, line A), but in fact the two are separated by about fifteen years. The line for Inter would, roughly, extrapolate to zero around 1965, while the Ordinary degree line would extrapolate to zero around 1980. There is thus no direct connection between the two.

In the post-war years, up to the early 1960's, the general pattern of student progress in the faculty was quite different from that which has become familiar more recently. From Fig.5a we see that in this period, about one-third of all degree candidates were sitting for the Ordinary degree. Most - but not quite all - of them would have originally been candidates for an Honours degree. The normal faculty practice was to require a considerable number of honours students to "revert to Ordinary" if they showed an indifferent examination performance at the end of their first year. A second group "reverted to Ordinary" at the end of their second year, and there was no method by which any member of either group could be re-instated in an honours school. The Physics Department, like everybody else, followed these practices. In the years 1945-60, for example, the total number of students taking physics "principal" as part of a Final Ordinary examination was about the same as the number taking Final Honours in physics.

There were two other features of the system in operation at that time which are worthy of comment. Firstly, it was quite common to require a student to "repeat the year's work" in a subject in which his examination results had not been satisfactory. For the session 1951/2, and several subsequent years, this was signalled in the faculty list by a letter "R" against the subject in question. No such mark appears in earlier years, but it is clear from other data that the practice had been in operation for some time. It was not uncommon for more than half of the students in an Ordinary degree class to be repeating at least one of the courses that they were taking, and there are cases where a student was repeating a course for the second time. This cannot have been good for student morale, but at the time it was accepted without comment as normal practice.

Secondly, there was a curious institution called a "supplementary third year", already mentioned on pp.83,85. It was possible for a student entering the University with exemption from Inter to satisfy the examination requirements for an Ordinary degree in two years. But there was also a sacrosanct regulation requiring three years of residence before a degree could be awarded. A course of study which would keep the student occupied for a year was therefore cobbled together ad hoc, and at the end of this third year he was awarded the Ordinary degree. No matter how well he did, there was no possibility of anything better. And no matter how badly he did, it was almost impossible to deprive him of the Ordinary degree to which he was entitled by the letter of the Regulations. This was the Supplementary Third Year. The arrangement seems to have existed, with slight variations in its name, at least as far back as 1923/4, and it appears regularly in the records except for the war years 1939-45. The number of students involved was never large: it rose from about 4 per year in the whole faculty in the late 1940's, to about 14 per year in the early 1960's, the increase running roughly parallel to the growth of the faculty. With the introduction of the revised regulations for the Ordinary degree in 1964, it became no longer possible to satisfy the examination requirements for the degree in two years, and after the session 1963/4 the entry does not appear in the faculty list. I doubt if anybody regretted the demise of an institution which, in retrospect, does seem to have been unsatisfactory all round.

After 1964, no student was admitted to the University with the intention of reading for an Ordinary degree. Nevertheless, more than a hundred such qualifications were awarded between then and 1980. The recipients were thus all rejects from honours schools. In this way, the continued existence of the Ordinary degree serves a very useful purpose. It provides a kind of safety net for students who find their chosen honours curriculum too demanding, and who choose, or are persuaded, to set their sights somewhat lower. Each year there are also one or two who complete the honours curriculum, but whose performance in the third year does not merit the award of an honours degree of any kind. At present (1988) there are about 20 Ordinary degrees awarded in the faculty each year. The number has been fairly stable for ten years, as is shown by the tail at the end of Fig.5a. The situation seems likely to continue into the foreseeable future.

Mention has been made more than once of the General Science degree and its successor, the Joint Honours degree. (see pp. 81 et seq.). Both of these institutions were designed to offer a course of study less specialised than the traditional Single Honours degree. The accepted wisdom was that there was a place for such a qualification within the university system. However, neither course has ever attracted large numbers of students. This can be seen from Fig.4, which shows the number of students taking the final examinations in each case, or from Fig.5b, which shows the same numbers expressed as a fraction of the total number of degree candidates. The numbers refer to the faculty as a whole. Because of the nature of the curriculum there is no reasonable way in which the situation relating to one single subject can be represented, but physics was not exceptional in this context. Clearly, the students showed no great enthusiasm for broadening their education. To what extent this was due to the fact that the teaching staff would have been, for the most part, dedicated specialists, is a matter on which one can only speculate.

Returning now to Figs.1 and 2, it is interesting to look into the origins of the more marked fluctuations. An obvious feature of Fig.2 is the pronounced minimum for the years 1914-19, followed by the equally pronounced maximum for 1920-24. Both are, of course, to be attributed to the disturbance of normal patterns by the war years. The changes are mainly due to variations in the size of Intermediate class, who constituted a considerable fraction, not only of physics numbers, but also of the total in the faculty. The effect on the numbers in the physics honours class, still fairly small at that time, is not distinguishable above the noise in Fig.1. Similarly, the steady fall in Fig.2 from 1921 to 1940 comes about almost entirely from the decline in the size of intermediate and ordinary degree classes, already discussed. The number of physics honours students, Fig.1, remained roughly constant.

The perturbation due to the second world war was more complicated. This time there was no minimum. Both the total numbers and, more strikingly, the physics numbers, actually increased. Our first-year honours class averaged 8 students in the period 1938-41, and rose to 34 in the period 1941-44. The second-year class showed a corresponding increase, from 6 to 24, one year later. But there was no corresponding impact on the third-year numbers in the following session. This was due to the introduction of special war-time regulations which allowed a student (in physics at least) to graduate with an Ordinary degree after only two years of study. The same regulations permitted such a student to return later, and to do an additional year's work for an Honours degree. Thus the corresponding jump in the size of the third-year honours class did not take place until 1946/47. For the three preceding years the class had averaged 5 students, and for the three following years the number rose to an average of 25. For the session 1941/42, two new subjects appeared in the syllabus, namely "Radio" and "Radio Maths". These two, taken in conjunction with Physics and Maths, qualified the successful student for an Ordinary degree. These special courses remained available until 1946/7. In the 1941/2 session only, there was also a group of students taking physics as part of what was called a "non-degree, state bursar course".

Thus, although the effect of the first world war was to reduce, temporarily, the number of undergraduates, including those taking physics, the second war actually increased the numbers in physics, as a result of the importance of radio to the armed services. In both cases there was a short-lived flood of ex-service students immediately afterwards. This shows up clearly in the numbers taking Inter., Fig.3. In the session 1946/7, about 85% of the physics intermediate class were ex-service. The process was assisted by the so-called Further Education and Training Scheme (F.E.T.S.) a national institution set up to help those whose education at tertiary level had been frustrated or curtailed by the war. The scheme was still helping to finance some students as late as 1951/2. It is interesting to note that the effect on the size of the Honours class was not a single large peak, as with Inter, but a more modest but sustained rise. (Compare Figs.1 and 3.) The sharp rise in Inter begins in 1943/4, before the end of hostilities, but since Fig.1a shows the numbers taking the final examination, the rise is not apparent until 1946/7. An interesting side light on these events appears if we look at the average age of the first-year Honours class. The following numbers were extracted from the Faculty lists.

Session	Average Age	
1948/49	22yr	0mo
49/50	20	2
50/51	19	9
51/52	19	9
52/53	18	11

Since 1945, the size of the Physics Department as measured by undergraduate numbers, has been affected by several deliberate decisions about the numbers to be admitted each year. Before the war, I suspect that the number of admissions was equal to the number of adequately qualified applicants, although it must be stated that I have no documentary evidence on which to base this opinion, nor was I personally involved. But in 1944, there appeared a formal, printed Statement by the University to the UGC, setting out hopes for future development. This is a detailed exposition, but it gives no clear indication of the time-scale envisaged. It suggests that the number of undergraduates studying "science" should increase from the then current total of 230 to 350, i.e. by 50%. (We may note in passing that this total had already been exceeded by the 1946/7 session.) The system of university finance then operative called for quinquennial proposals from each university, and this in turn required a similar review by each department. The papers from Physics show two general features:- (a) they are almost entirely concerned with research and the associated staffing and finance. and (b) when undergraduate teaching is mentioned, the general tenor of the comment is that "we have no great desire to expand, but are willing to do so by x% if called upon". In 1950. for example, we read (on a page added to the draft as an afterthought) "the number of undergraduates is ... 310 in the current session ... we recommend that the number be stabilised at 300". Similarly, proposals for the 1957-62 quinquennium, written in 1954, speak of "no large-scale expansion", and say " no major changes in the teaching activities of the department are foreseen" (In fact, the intake went up by about 20%.)

By 1956, however, the prospect of an increased demand for university places as a result of the post-war "bulge" in the birth rate was necessitating some forward planning. The enlarged age-cohort was due to begin applying for admission to universities about 1962. Bristol University as a whole was proposing to expand by about 40% to help meet the expected demand. For various reasons, a rather larger increase - about 50% - was thought to be appropriate for science departments in provincial universities. Our private view in the Physics Department was that, to allow for a foreseen increase in the demand for physicists, an even larger increase would not be out of place. But we were also mindful of two other facts. The Physics Department was already large when compared with other science departments at Bristol, and also when compared with other physics departments elsewhere. We were therefore reluctant to press for large increases. In addition it was felt, by Pryce in particular, that if we grew too big, the character of the Department would change - and change for the worse, of course. (We are now (1988) about twice as big as we were then (1960) - and who is to say that Pryce was not right?)

There were also some real practical problems to be considered, such as the provision for teaching ancillary physics to students of other disciplines, and the provision of ancillary teaching, in other subjects, for physics students. The most important constraint on expansion was the provision of adequate laboratory space, and thus the question of the growth of student numbers in physics was closely linked to the planning of the extension to the laboratory. In the draft quinquennial proposals for 1967-72, dated March 1965, we find, for example, :-"The extension (of the building) was originally planned for an increase in undergraduate population of about 50% ... There would appear to be no strong reason for any significant change in these long-term plans." And again, in January, 1970, in a letter to the Dean of the Faculty, after rehearsing the same arguments, "We therefore propose not to lay any great emphasis on plans for an increase in undergraduate numbers, and while we could fairly readily accommodate an increase of 10% within existing premises, and an increase of 20% without much difficulty, we do not propose to make such increases in the near future." (The intake into physics at this time was about 85 per year, to which must be added the smaller intake into Joint Honours schools involving physics.) Also, in October 1975, we find:- "... should the need arise, a 10% increase in intake could just be accommodated." What in fact happened was a fairly steady increase in the annual intake into the honours school. This is shown by line B on Fig.1. The operation of admitting students is particularly liable to random fluctuations in the resulting numbers, since it involves a good deal of guess-work on the part of the admissions tutor, about the decisions that will be taken, later, by the applicants. It is thus not surprising that the resulting graph shows no well-marked discontinuities that can be positively ascribed to a decision to increase numbers.

(b) Survival Rates.

The quantity which is plotted in the graph B of Fig.1 is the number in the first-year honours class; the number for the session 1960/1, for example, is plotted against 1961 on the time axis. The line A on the same diagram, showing the numbers in the third-year honours class, is plotted with the number for the 1962/3 session against 1963. Thus the two points, one plotted against 1961 on line B, and the other against 1963 on line A, refer to the same cohort of students - the 1960 intake. The two lines on Fig.1 might thus be expected to be the same shape, with a two-year shift along the x-axis, and a change of scale along the y-axis, to allow for "wastage". (In this context, the word "wastage" is used to include both those students who left the University for any one of many reasons, and also those transferred to any course other than honours physics.) It is clear at a glance that this simple-minded expectation is not fulfilled. But the fluctuations are so large that no pattern is discernible, except that the wastage seems to get less as the years go by. This question is of sufficient interest and importance to merit further investigation. The method adopted was to follow each admitted cohort, and look at the fraction which survived into the second and third year of the course. This procedure immediately reveals one complication. The numbers refer to students in the Special Honours course. But during the period in question there were also honours courses in General Science and, later, Joint Honours courses. It was always possible for a student to transfer between these and Special Honours, in either direction, provided that his academic record was suitable. Thus if the number of transfers into Special Honours was greater than the sum of transfers out and wastage, it is possible to obtain a survival rate of more than 100%. In a sense, therefore, the use of the term "survival" is not strictly appropriate, but the meaning will be quite clear.

The results of these calculations are shown in Fig.6. This shows the survival rate, as just defined, between Stage I and Stage III. Several points call for some comment. Up to and including 1940/41 the number of students in each year was so small that a change in the fate of one of them could produce a large change in the ratio, and the curve thus oscillates rather wildly. If, however, we add together the numbers in all 13 years from 1929 to 1941 inclusive, and then take the ratio, we get a meaningful average value of 0.73. Three-quarters of those who entered survived until their third year. There are then three low values of the ratio, for the sessions 1941-44. This is due to students embarking on the course and then leaving to join the services with a two-year degree. There follow three high values, for 1944-47, due to the same students returning after demobilisation to complete their course. Thereafter, the values fluctuate between 0.6 and 0.7 until 1965, and then rise to values between 0.8 and 1.0 for the rest of the time.

In order to throw more light on these variations, the survival ratio from Stage I to Stage III was split into two component parts, corresponding to the transitions from Stage I to Stage II and from Stage II to Stage III respectively. The results are shown in Fig.7a and Fig.7b. The early years are marked by the same large fluctuations. The mean, taken as before, is 0.85 for the I - II transition, and 0.87 for II - II. The enormous dip in 1941-44 clearly arises mainly from the II - III transition, as would be expected: the students stayed for two years before joining the forces. Both graphs show a tendency to rise with time, discernible behind the fluctuations, and in both there is a rather more pronounced rise around 1965, more clearly visible in 7b than in 7a. The points in 7b lying above 1.0 in the period 1975-85 arise from the practice, current at that time, of requiring a student with a rather unsatisfactory performance in Stage I to transfer, nominally, to the Ordinary Degree curriculum for Stage II. He would, in fact, take the full course of Honours lectures, and if his Stage II results were good enough he would be transferred back to Honours at the end of the year. Why this was done is not clear - to me - but the net effect was that, although the I - II and the II - III transfer ratios were distorted by the operation, the two transfers cancelled out when the numbers for the I - III survival rate were calculated.

Two features shown by all three graphs call for further comment. The first is the slow rise with time, which lies behind the fluctuations. Part of this increased survival rate in Honours is associated with the decreasing number of Ordinary degrees. If fewer students are relegated to the Ordinary degree, a higher proportion must survive in Honours. This change, and the decrease in the practice of requiring some students to repeat a year's work, both point in the direction of a more tolerant and generous attitude on the part of examiners and committees towards the weaker students, and a less rigid application of rules. Other possible explanations would be:-

(a) The quality of the students applying for admission, or the expertise of the staff responsible for selection, improved so much that fewer students were admitted in the first place who proved to be incapable of coping with the demands of the honours curriculum.

(b) The standard of undergraduate teaching, and perhaps also the level of pastoral care, improved, so that fewer of those admitted fell by the wayside.

(c) The standard of the examination fell, so that a higher proportion "passed".

All of these seem to me to be rather unlikely, and I therefore incline to the explanation suggested above. I can see no means of deciding the question.

The second feature of Fig.6 calling for comment is the rather marked increase in the survival rates around 1965. If one searches for some event at this time which might account for the discontinuity, two possibilities arise:

(1) The changed regulations about Ordinary Degrees came into operation about then. The general effect was to raise the standard of the degree by increasing the number of subjects in which a pass was required. It is conceivable, but unlikely that this might have had the indirect consequence of allowing some borderline candidates to stay in their honours school.

(2) In 1964, Pryce left, and was replaced by Powell as Head of Department. Amongst other duties, he would act as chairman of our internal board of examiners. Everybody who knew both men would agree that Pryce had very high standards, and tended to be strict about applying them, whereas Powell took a more generous view. Thus it is plausible that their influence as chairman of the examiners might have given rise to the step in the graph.

In this case it is possible to obtain further evidence bearing on the problem. If the former explanation, i.e. (1) above, is correct, then a similar effect should be present in all subjects, and not just in physics. This is most easily checked by calculating the survival rate for the whole faculty. The results have been shown by adding the appropriate points to Section A of Fig. 6. It is immediately clear that the faculty points reproduce the general shape of the line for physics, although the perturbation due to the war years is less pronounced: could it be that physicists were in greater demand by the services than other scientists? The rise from 1950 to 1970 is also present in both sets of data, but there is a tendency for the "faculty" points to lie above the "physics" line, i.e. the survival rate was somewhat smaller for physics. This is particularly noticeable in the period 1959-65. However, the sharp rise in the physics line in 1965 is NOT reproduced in the faculty data. This all fits with the suggestion that the period of Pryce's headship coincided with the period of low survival rate, and that the rate rose rather abruptly when he was succeeded by Powell.

We can obtain another pointer by looking at the results obtained by those students who sat the final Honours examination, and asking what fraction of the class obtained a "good" Honours degree - a term which is usually taken to mean either class I or class IIa. The ratio of (numbers of awards of class I or IIa) to (total number of Honours degrees) is shown in Fig.8. Before and during the period influenced by the war the numbers each year were so small that the ratio is fairly meaningless. In view of the discussion in the previous paragraph, marks have been added along the top of the diagram showing the terms of office of the Heads of Department. The low values under the Pryce regime, and the high values under Powell are clearly shown. The high values since 1985 are, as I understand, the result of a deliberate policy decision. The behaviour in the Mott era remains a mystery.

(c) Applications and Admissions.

Some mention must be made of the way in which undergraduates were admitted. I have been unable to find any information about the early days of the University. Since the numbers were so small, it would be a plausible guess that all qualified applicants were admitted. Indeed, Tyndall states in his History that this was true for the inter-war years, but he also occasionally complains about the inadequacy of the equipment for student laboratory work, so perhaps this factor also imposed some limitation on the intake. In the mid-1940's, when I first became involved in some aspects of departmental administration, I remember Piper preparing lists of applicants on large, double-foolscap sheets, which recorded against each name, information about the candidate (age, parent's occupation, etc.), the school and examination record, and notes on the Head's report. I suppose there must have been application forms from which this information was extracted, and I would guess that the early stages of this process would have been handled by the Registrar's office. From some scraps of remaining evidence it is clear that we placed considerable reliance on the school reports. These were graded, on our summary sheet, from alpha plus to gamma minus, and this was probably done by Piper on the basis of what the Head had written. We were clearly exercised about the reliability of this process, since there are occasional references to the degree of the correlation between these gradings and subsequent performance. In 1952, for example, I noted that the top 12 students in the first-year examinations had had a mean grading on entry of 7.1, while the bottom 12 had had a mean grading of 5.2. (The scale used ranged from 9 for alpha plus to 1 for gamma minus.) In 1960 we looked at the class of degree obtained by the 1957 entry in relation to their grading on admission. These entry grades were grouped under six headings, ranging from "alpha-plus", called Grade I, to "beta-minus and below" called Grade VI. The scale for class of degree ran from 5 for First Class Honours to 1 for an Ordinary Degree. The results were as follows:-

Entry Grade	I	II	III	IV	V	VI
Mean degree class	1.0	2.9	2.4	2.3	2.0	1.0
Number of students	1	10	13	12	13	3

Then, as now, the correlation was not strikingly good.

In addition to this not-very-reliable criterion, we also had school examination results to help the selection process, and there were interviews. Not all candidates were interviewed, but, even so, there must have been something of the order of 100 interviews each year. The only firm data that I have been able to find is that, in 1964 - i.e. after the introduction of the UCCA scheme - we interviewed 190 candidates out of a total of 796 applicants. Each interview occupied two members of staff for 15-20 minutes. The practice was later abandoned completely since - in my view at least - it occupied too much staff time in relation to the usefulness of the information that it produced.

We have complete records of the total number of applications since 1953. The numbers are shown on Fig.9, graph A. Around 1953, there was a widespread popular belief, encouraged by the press, that there existed intense competition for admission to a university, certainly in physics and, I think, in other subjects also. This was well before the introduction of the UCCA scheme. At Bristol we had more than 250 applications, and could admit only 50 students, i.e. an apparent success rate of only 20%. However, we were convinced that this apparent competition was not real. Accordingly, Mott and I carried out a survey by writing to all those Bristol applicants whom we had not been able to accept. The results showed that 73% of the original applicants were already students at some university. They also indicated that, after the next round of admissions, when some of the non-admitted said that they proposed to apply again, the figure would probably rise to about 87%. The exercise was repeated in 1956, and again in 1959, and gave exactly the same result - 73% of our original applicants found a place in some university. The "intense" competition was illusory, as we had suspected. From Fig.8 we see that, during the period 1953-61, the number of applications to Bristol rose steadily. Although there may have been some increase in the real demand, most of the increase in the number of applications must have been due to each candidate applying to more and more universities. Bristol was not peculiar in the matter, although there is some evidence that we were a popular Department, and may have been an extreme case. The graph shows that in 1961 we had nearly 1200 applications to deal with, and eventually admitted 68 students. It was this kind of situation, repeated all over the country, that provided much of the driving force for the setting up of the Universities Central Council on Admissions, which had its first full year of operation for students entering in 1963.

The graph A of Fig.9 shows that Bristol applications peaked in 1961, and continued to decline thereafter until about 1976, since when the number has oscillated. Much of this variation is not peculiar to Bristol, as can be seen by comparing graph A with B, which shows the total number of applications through UCCA from students who stated that physics was their first preference as a subject of study. During this period there were variations in the size of the 18-year-old cohort, due to birth-rate fluctuations. There would also have been changes in the popularity of university education in general, and in the popularity of physics relative to other subjects. All of these factors cancel out if one takes the ratio of the number of Bristol applicants to the national total. The value of this ratio is shown in graph C of Fig.9. This gives a measure of the relative popularity of Bristol as a place to study physics. The UCCA data for the first few years are not entirely reliable, since not all university institutions participated in the scheme from the outset. The anomalous values of the Bristol applications for the years 1964 and 1965 are something of a mystery. But it will be seen that, since 1966, most of the variability in the number of Bristol applications has been due to non-local causes. All that remains is a steady decline in our popularity, and some small and rather random fluctuations. The reasons for the decline could give rise to endless speculation.

Another aspect of the admissions exercise which is of interest is the provenance of the applicants. In the early days of the University these would be almost entirely local residents. Later developments are illustrated by the two maps reproduced at the end of this chapter. The first shows the domicile of the 230 students admitted to read physics in five years around 1950. It will be seen that 40 of them lived in Bristol, and about as many more lived in the surrounding counties. By 1965 the picture is quite different. (The second map shows applicants rather than admissions, but as far as geographical distribution is concerned, this difference is not significant.) Now only 4 out of 500 live in Bristol, and there is a very large group from London and its environs. Bristol has thus ceased to be a "provincial" university, at least for physics. To a first approximation, the distribution of applicants follows the distribution of population, but a closer look reveals some considerable and interesting differences. There appear to be two factors which are important. One is the distance between home and Bristol, and the other is the relative affluence of the population in the various regions. We can allow for the basic population density by making an analysis by counties, and counting the number of applicants per million of the population. We did an investigation of this kind in 1965, when the average for England and Wales was 20.8 physics applicants to Bristol per million of the population. But for the group consisting of London, the home counties and Hertfordshire, the average was 27.9/million. The district is fairly affluent, and Bristol is easily accessible. Within this group, the highest value was 59.8/million for Surrey, in the stockbroker belt, and the lowest was 8.8/million for Essex, a much more proletarian community. The group consisting of Lancashire, West Riding, Cheshire and Staffordshire, all fairly highly industrialised, gave 14.6/million, The highest within the group was Cheshire, with 21.5 - again a dormitory region for Manchester and Liverpool business men, and the lowest was Lancashire, with 12.8. Of the regions remote from London, the five western counties of England, together with Monmouth, averaged 14.6/million, while the four northern counties plus the North and East Ridings of Yorkshire averaged 9.8. The presence of several civic Universities in the north probably accounts for much of this difference. Clearly, in 1965, Bristol was primarily a middle-class University, drawing most of its students from the southern half of England. And now?

(d) Graduate Student Numbers.

The increase in the number of research students working in the Department has been less rapid than the growth in the number of undergraduates. The words "research student" are taken to mean a candidate for a higher degree who was not a member of staff: it thus excludes short-term "overseas visitors". A list giving information about those in residence each year has been produced annually since 1948/9, but the papers are missing for the period 1955-64. Data extracted from these records are shown in Fig.10, the candidates for the MSc course in the Physics of Materials being shown separately. The numbers should be fairly reliable: the main uncertainty arises from the fact that not all the students listed stayed for the full year in which their names appear. In particular, there were often one or two - rarely as many as five - who stayed for more than the statutory three years required for PhD candidature, but not for a full fourth year. These have all been included in the total, since it is not always clear from the records who did and who did not complete the year. The resulting total will thus be, if anything, an overestimate.

Over the past 20 years the average number of PhD students at any time has been about 50, which implies an annual intake of about 15. Fig.10 also shows that the MSc course attracted an average of about 11 students per year for the 17 years of its existence. The variations with time of both sets of data are largely unexplained. The question of how the students were financed is discussed in the next chapter. Since it appears there that a large fraction of the PhD candidates were financed by SERC (or its predecessors) we can look for enlightenment at the number of studentships awarded annually by that body. The total number of such studentships for physics (including, in the official nomenclature, Astronomy Space and Radio, Nuclear Physics, and Other Physics) is also plotted on Fig.10. There is a vague similarity between the shapes of the two graphs, but nothing more. Alternatively, we can make a comparison with the data collected by the Standing Conference of Professors of Physics, since 1978/9. One of their results gives the total number of graduate students in residence in all (or nearly all) university departments of physics in Great Britain. This will include MSc's, but prospective PhD's will predominate. Fig.11 shows a comparison between the Bristol figures for PhD's, and the national figures for post-graduate students. Both sets of data have been normalised so that the mean of each over the nine years is unity. Although the Bristol figures show a bigger range of variation than the national total, the two graphs are sufficiently similar to suggest strongly that the explanation of the variations is not to be sought in any local cause, peculiar to Bristol.

(e) Academic Staff

To complement the statistics on student numbers, it is of interest to look at numbers of academic staff. The apparently simple process of counting them has proved to be surprisingly difficult. There are two potentially useful sources of information. (a) Departmental records, of which the most valuable is the list of "Research Groups", prepared each year in October/November. These are complete only since 1969/70, and the records for earlier years are very fragmentary. When they exist, they should be quite reliable, except that they do not always indicate when somebody was here for less that a full year. Other lists of our "establishment" have been prepared *ad hoc* for meetings of the Steering Committee, but these are even more fragmentary, and less use. (b) The University Calendar includes Staff Lists. It was published annually up to the session 1980/81, and thereafter only every second year. The series is effectively complete from the founding of the University. However, a comparison with departmental records for the period when both are available (1969-81) shows that the Calendar is by no means a reliable source of information. One reason for this is that it must go to press some time before the start of the session to which it refers. During the intervening period, there is always considerable activity in the comings and goings of academic staff. But this is not the whole story, as one or two examples will show. Dr X was a Research Assistant for a period of five months in the 1970's, but his name continued to appear in the Calendar for eight years after he had left. Dr Y was appointed as a lecturer, but withdrew after appointment and before the start of the session in which he was due to take up his post. He nevertheless appears in the Calendar for several years. In 1979/80, there were 25 people working in the Department as Research Assistants or Associates whose names do not appear in the Calendar, and 7 people are listed who were not here. (This is an extreme case; there was a great flux of junior staff at that period).

In these circumstances I have thought it prudent to give the data from both sources, and both are shown in Figs. 12a and 12b. Each set is sub-divided into two groups, in a way that will be discussed shortly, but for the moment we will consider only the two totals, show by the topmost graphs in each case. By comparing 12a and 12b it is clear that in the years when the two sets of data both exist, the numbers from the departmental lists are greater than those from the Calendar. The long lead-time for the printing of the Calendar cannot, alone, explain this systematic difference. It is possible that there has been a failure on the part of the Department to keep the editor of the Calendar fully informed, and a lack of assiduity on the part of the editor in checking his information. But it is still not clear why the discrepancies should be always in the same direction. We can only hope that in the earlier years, when the rate of staff turn-over was less, the published data were more reliable.

In Fig.12a the total of staff is split into two parts as shown. This has been done on the basis of the appointment held, as recorded in the Calendar. One of the graphs shows what might be called "senior" appointments, i.e. Professors, Readers, Fellows and Lecturers, all posts which are usually intended to be permanent. The other graph shows the "junior" appointments, i.e. Research Associates and Research Assistants. The "senior" group will, in general, be older than the "juniors", but there are numerous exceptions. It is interesting to note that, up to about 1935, i.e. in the pre-war period, there were very few junior posts. From then until about 1965 they were about half as numerous as the seniors, and thereafter the two groups have been comparable in size. To some extent this is possibly an inevitable consequence of the increase in absolute size of the Department. But it probably also reflects the increasing emphasis on the research function of the University, relative to its teaching function. An increasing fraction of the staff have held finite-term appointments of short duration, often financed by research contracts. For this same reason, the graph for the senior posts is comparatively smooth, while the number of junior posts varies more erratically. The sudden rise in the latter around 1969, for example, is probably to be associated with the activities of Ziman, who managed to obtain from SERC a considerable sum of money with few restrictions on its spending. This enabled him to employ for short periods a succession of able young research workers who came to his notice.

In Fig.12b the total is again split into two parts, based this time on the method of classification used in the departmental staff lists, from which the data were derived. One graph gives the number of "established" staff, - which is almost synonymous with those paid from UGC funds. This will include all the "senior" staff of the previous classification, but also some "juniors" as well, whose appointments were effectively permanent. The second graph in Fig.12b is derived from a column in our staff list headed "others". Most of them were Research Associates and Research Assistants, usually paid from research contract funds. Some were self-financing, i.e. people from overseas paid from their country of origin. One or two were paid, temporarily, from UGC funds, when there was a temporary, short-term unfilled vacancy in the establishment list. The most interesting thing about Fig.12b is the way in which the number of established posts has declined steadily over the period covered by the available data, while the number of "others" has risen by more than enough to compensate - at least until 1985.

If we look again at Fig.12a, and at the graph in it giving the total numbers of staff, it is immediately obvious that the growth in staff numbers is very similar to the growth in student numbers, as shown, for example, in Fig.2. It is thus tempting to calculate the ratio of the two, i.e. the student-staff ratio. This is well known to be an exercise fraught with difficulty, arising from the problems of knowing who to include, in both categories. Fig.13, graph A shows the results of one such calculation of this kind. The number of students was taken from Fig.2, and is thus the total number taking a course in physics, of any kind. The number of staff was taken from Fig.12a, using the "total" number, thus including academic staff of all kinds.

(For the years 1969-81, an alternative calculation using the "total" curve of Fig.12b has also been done. This duplication gives rise to the two lines in Fig.13 for these years.) This is NOT the way in which the student-staff ratio is usually calculated, and to preclude any comparison with other results, the vertical scale in Fig. 13 is not specified. However, internal comparisons will still be valid. For example, the]arge values of the ratio in the years 1910-30 is due to the inclusion in the student total of the large Intermediate classes, and the large fluctuations are due to the fact that the staff numbers were small, so that the arrival or departure of one person has a large effect. The low values of the student-staff ratio from 1965 onwards arise partly from the inclusion in the staff total of those classified in Fig.12a as "junior" posts, which proliferated during this period. These people, mainly Research Associates and Research Assistants, usually had few teaching duties. It would therefore perhaps be more reasonable if they were not included in the staff total, in this context. If they are not to be counted, we must calculate the student-staff ratio from the number of "senior" posts in Fig.12a (or the number of "established" staff in Fig.12b). The results are shown by graph B of Fig.13. It will be seen that, even on this basis, the ratio falls during the period 1950 to 1970; after that it remains almost constant. Any comment would rapidly lead to a discussion of controversial issues that would be out of place.

By way of postscript, I give a quotation from Tyndall's Annual Report for 1917/18. He wrote:- "Since July, the academic staff has been further reduced, and only the Head of Department remains." The total undergraduate population at that time was about 50, of whom about 30 were taking the first-year, Intermediate course, while the remainder were spread over the later stages.

(f) Non-academic Staff.

Information on technical staff is even less satisfactory than on academic staff. The departmental list of Research Groups provides reliable data since 1969/70. Before that, there are occasional lists prepared for other purposes, and two or three group photographs, neither of which can be guaranteed to be complete. Even this source dries up before 1947/8, except for one isolated photograph, taken in 1921. Such information as is available is shown on Fig.14. The numbers do not include secretarial or library staff, but do include stores staff. There were also considerable, and variable, numbers of girls employed in Powell's research group, who were known as "observers" or "scanners". There was a fairly rapid turnover of personnel, and the total fluctuated a good deal, rising to a peak of the order of 20 in the 1950's. Where the number is known, or can be estimated, it is plotted as an additional point on Fig.14.

The absence of data before 1947 is particularly unfortunate, since the graph consequently fails to show the marked increase in the number of technicians attached to research groups after the war years. Before 1939 there were almost none. The technical staff were all employed in either the workshop or the undergraduate teaching laboratories, or else in the lecture theatres. The experience of the academic staff on war service in government departments alerted them to the advantages of having a personal research technician. On returning to the University they asked for, and gradually acquired, comparable assistance here. For this reason the guess-work interpolation between 1920 and 1946 is likely to be closer to the curve sketched in Fig.14 than to the straight line.

As for secretarial staff, there are no records at all. Tyndall mentions in his History the consternation that he caused when he first asked for a departmental secretary. The appointee, Miss Masters (later Mrs Terry) was still in the Department when I arrived in 1933, and was still the only secretary. In addition to doing all the typing for everybody, she also did all the clerical work relating to the stores. Mott soon acquired a secretary of his own, Mrs Langdon, who dealt also with the work of the theoretical group, and relieved Mrs Terry of some of her duties. When Salter was appointed as storekeeper, he did all the associated clerical work. The number of secretaries gradually increased to keep pace with the numbers of academic staff, but there are no records. As each new appointment carrying the title of "Professor" was made, it usually meant an additional secretary - sometimes, it appeared, more as a status symbol than through any increase in the work-load. The total must have reached seven or eight before financial stringency set in motion the reverse process.

CHAPTER 8

FINANCE

(a) General Survey

An account of the finances of the Department is another of the sections in which it seems to me that it would be profitable to extend the time-scale of the exercise, at both ends. This has not been without its problems, but the following paragraphs cover the period from 1910 to 1987 in a way which gives a fairly reliable account of the general trends, although individual pieces of data may not be entirely accurate.

For the early years, the minutes of the University Finance Committee show that it was the practice for the Head of a Department to deal direct with the committee by writing letters suggesting increases in the salaries of staff, proposing new appointments, or asking for authority to spend up to a specified maximum sum on "departmental expenses". The committee, which seems to have met every two or three weeks, dealt with all such matters in detail. A proposal to systematise these procedures by the submission of a regular annual budget in a form that would be standard for all departments was not made until 1914, and there was no Finance Officer until 1948.

The principal source of departmental income has always been the appropriate part of the grant which the University receives from the government of the day through the University Grants Committee. The administration of these funds is the responsibility of the Finance Officer, and it has been possible, with the assistance of the Finance Office staff, to obtain from their files details of the sums involved from 1952 onwards. This data is shown as graph A in Fig.15. To be strictly accurate, the numbers give the annual expenditure, and not the income, but the difference between the two in any one year will not be large. There are also some departmental papers available, but these relate only to "departmental expenses", and so do not give information on salaries and wages. They are thus of little use for our present purposes. Their main interest lies in the accompanying correspondence. Each year we had to submit our budget proposals for the coming session, and there was always an explanatory covering letter to the Vice-Chancellor. The files also include his reply, explaining gently but firmly why we could not have as much as we had asked for. This letter was always signed by, and probably written by, the VC in person, (by Sir Philip Morris in particular), but one suspects that the decisions recorded owe a lot to the Finance Officer.

It was not until 1946 that the Physics Department had any substantial income on a regular basis in addition to that from the UGC. Exceptionally, we find that in 1926 the Department of Scientific and Industrial Research - the forerunner of SERC- paid the salary of Miss Dent, a computing assistant who worked with Lennard-Jones; and in 1944/45 Mott had solicited a total of £4,500 from industrial sources to help finance the work of his research students. But in 1946 a new pattern really began, stimulated by the war-time experience of the academic staff, and encouraged by developments at national level. The Annual Reports of Council to Court from 1946/7 onwards regularly include lists of grants in aid of research from various bodies. These reports were issued only in abbreviated form during the war years, so that it is not impossible, although very unlikely, that such grants may have begun earlier. In 1946/7, Powell received £21,000 from D.S.I.R., to be expended over a period of five years. This was followed in 1950/1 by the first Royal Society grant, to Burch, and in 1952/3 by the first contribution from a Government Ministry, to Mott.

There is no reason to suspect that the lists of such grants in the Annual Reports are not complete, and they are used as the basis of what follows. There are departmental papers dealing with these matters also, but they are certainly not complete, since they were often prepared *ad hoc* as a basis for discussion at some meeting. The entries in the Annual Reports give the origin of the grant, its amount, and the year in which it was received, with - usually, but not always - a statement of the period that it was intended to cover, if this was more than one year. When this is done, I have split up the total and spread it evenly over the appropriate number of years. Clearly this is only a rough approximation, since the rate of spending would not, in fact, be uniform over the period: and in any case, the grant would probably not start from the beginning of an academic year. The totals of the "research grants" so obtained are plotted as graph B in Fig.15.

For the period before 1952 I have taken the figures for the departmental budget from a rather small-scale graph in Tyndall's History. The data are thus not very accurate, but at least they are probably self-consistent. It is not clear from the text whether Tyndall's figures represent only the UGC element for the years 1945/52, during which period there was also some income from research grants, as already mentioned. The same is true for the Finance Office data for 1952-75. There are thus two limiting possibilities for the whole period 1945-75. Either we assume that the figures represent income from all sources, or we assume that they represent UGC income only. In the latter event, we have to add the research grants to get the total budget. I suspect that the latter is probably correct. But in graph 16A, which shows the total budget over the whole period since 1910, I have drawn two lines to represent the two possibilities. The upper one is probably to be preferred. Since the range of values in Fig.16 is so large, the ordinate has been plotted on a logarithmic scale. The superposed straight line was not calculated to fit the points, but has just been drawn with a slope corresponding to a rate of increase of 10% per year. It is clear that the general trend of the data follows this line, although there are some notable deviations. The most pronounced of these is the dip in the years 1940-45. i.e. the war years. The corresponding dip in 1914-18 is present, but is less obvious.

Much of the apparent increase in funding shown in Fig.16 must, however, be attributed to inflation. We can make some allowance for this by dividing the expenditure for each year by some index number measuring the amount of inflation. *Faute de mieux*, I have used the Retail Price Index which, although not perhaps strictly appropriate, has the advantage of being available, on a consistent basis, since 1915. The results of these calculations are shown in graph B of Fig.16. The fluctuations are more obvious, but there is still clearly an underlying steady increase. The superposed straight line has now been given, arbitrarily, a slope of 6% per year. We have seen in the chapter on Statistics that, during the whole of the period concerned, the Department was growing fairly steadily. This alone would be expected to give rise to an increase in the annual budget. To allow for this in turn, we could divide the numbers of Fig 16B by a factor to represent the "size" of the Department. There is no unique way of defining this quantity, and the choice of a proper measure must be to some extent arbitrary. A reasonable suggestion would be to use the total number of research workers, i.e. the combined total of staff and research students. This takes account of the facts that (a) a large fraction of the total expenditure consists of the salaries of such people, and, (b) most of the remainder is spent on either equipment for research or the salaries of technical and secretarial staff. There is thus little point in trying to incorporate into the measure of "size" in this context any factor derived from the number of undergraduate students.

The result of making this allowance for size as well as the allowance for inflation is shown, on an arbitrary scale, in Fig.17. The pronounced minima during the two war periods are now very obvious, and the slow recovery during 1945-50 is also clear. The sharp peak in 1960/1 is caused by a particularly active phase in Powell's large and expensive balloon-flying experiments. The dip in 1929/30 may possibly be due to the economic "depression". It is now difficult to detect any general trend behind the fluctuations, but it could be claimed that, since 1965, any such trend has been downwards. If, as is sometime claimed, the costs of scientific research have risen faster than the general cost of living as measured by the Retail Price Index, this trend would be accentuated.

(b) Powell and Cosmic Ray Research

We can put a little flesh on the bare bones of these statistics by describing in more detail the finance of Powell's work on cosmic radiation and particle physics which, for a time, represented a considerable fraction of the total departmental research expenditure. In October 1945, as part of the process of getting research under way after the end of the war, Tyndall wrote to Tizard of the Nuffield Foundation, asking for money to develop Powell's photographic emulsion technique. Mott's group on solids, he said, is already reasonably well financed by grants from industry, and funds to support a school of nuclear research are needed to complement this. A grant of £2,000 per year for five years would be most helpful. The appeal was not successful, and there followed a lengthy correspondence involving the Nuffield Foundation, the

Royal Society, the Ministry of Supply ("Tube Alloys"), and D.S.I.R. After a great deal of buck-passing and administrative delay, a letter from D.S.I.R. in October 1946 gave formal notice of the award of a grant of £12,500. Within a few days, Tyndall was asking for the sum to be increased. Somewhat surprisingly, the request was granted, and by June 1947 the total grant was £21,000, to be spent over five years. In the following years a series of further applications were made and approved. In the ten years from 1955 to 1965, the total of these grants was about £195,000, of which all but £54,000 came from D.S.I.R., the other main sources being NATO and N.I.R.N.S. Note that this was about ten times the rate of support originally requested from Nuffield. It was also about equal to the total of the research grants received in the Department in support of ALL the other work in progress. The heavy reliance on D.S.I.R. was in marked contrast to the policy of Mott, who deliberately diversified his sources of funding.

One incident during this period is worth recording in more detail. A draft of our quinquennial proposals, dated 1/12/50, includes the sentence:- "It is not certain that the work on nuclear physics and cosmic radiation will continue to be supported by D.S.I.R., and the subject is at present under negotiation between that body and the UGC." The general policy of D.S.I.R. was to give grants to help open up new fields of research in universities, with the expectation that, when the work became established, it would be paid for out of general funds, including in particular, the UGC grant. (Trueman, of the UGC, is on record about this time as saying that the UGC did not "necessarily" continue such a grant.) But in 1953, an exception was to be made in the case of those universities committed to the building and running of big accelerators; to these, their original D.S.I.R. grant would be extended for a further five years. In September 1951, Powell received a letter from D.S.I.R. saying that when his current grant ended in July 1952, they would not be able to extend it. Since this was normal practice, and since the possibility had already been foreseen, as quoted above, the decision might have been expected. In fact it came as a very unpleasant surprise. The explanation is to be found in a letter from Mott to Blackett (20/9/51) describing it as "the first intimation that we have had that his (Powell's) work would be treated on a different footing from larger nuclear physics projects." There is also an (undated) memo in Mott's handwriting which says "we had assumed that P.'s group would be treated like the Oxford and Cambridge groups". The decision had apparently been taken at an informal D.S.I.R. committee dealing with "Nuclear Physics Research in the Universities" in November 1950. Both Blackett and Thomson, the other people mainly affected, were members of this committee, and thus knew well in advance what was going to happen. Not so Powell; for once the grape-vine had failed. Later Mott notes "I blame myself for not knowing what was in the air."

The letter from D.S.I.R. had arrived only a few days before the University was due to submit to the UGC its estimates for the coming quinquennium. The Vice-Chancellor thus had no opportunity to do more than merely notify the UGC of the unexpected loss of income. In common with all other universities, the UGC grant to Bristol for 1952/53 was less than had been requested. More than once in the correspondence it is stated that, in arriving at their allocations, the UGC had "taken into account" the cessation of Powell's D.S.I.R. grant. But there is nothing more specific than this. Indeed, in the minutes of the D.S.I.R committee mentioned above it is stated that "it is a cardinal item of policy with the (University Grants) Committee that earmarked grants should be avoided, if in any way possible." Any such suggestion would, in any case, have met with a very cool reception from Bristol's Vice-Chancellor, who refers in a later letter to Mott (March 1953) to "the complete impropriety of allowing the UGC to influence the distribution of a general grant which is, by definition, at the disposition of the University". Accordingly, the difficult task of apportioning the limited resources fell on the shoulders of Sir Philip Morris himself. The result was that Powell was asked to reduce his proposed budget for 1952/3 to only 60% of the sum approved in the previous year. Efforts to coax more money out of D.S.I.R., the UGC and even the Royal Society proved of no avail. The Vice-Chancellor helped by agreeing to make John Davies' salary a charge on University general funds, and by March 1953 Powell was able to report that as a result of (unspecified) economies, he was operating within his budget. He also said that he thought he had been treated fairly, or even generously, by the University: but there remained a general feeling that Bristol was not getting that measure of support from national sources which it deserved.

In February 1953 Powell, prompted by Mott, wrote a long letter to Cockcroft, setting out his financial problems in some detail. Cockcroft was at that time at Harwell, and was an influential figure in government scientific circles. The formal acknowledgement was sympathetic, but the subsequent detailed reply - if any - is missing from the files. Whether the letter had any real effect is not clear, but, as already mentioned, there were numerous grants from D.S.I.R. to support Powell's work in the following years. (He also received £3,000 direct from A.E.R.E. in 1955, to buy emulsions for use in an experiment on the Berkeley cyclotron.) In October 1960, a new grant from D.S.I.R., to replace a group of smaller ones still current and to allow for an extension of some others that had come to the end of their term, was for a total sum of £96,000, - so large by the then current standards that D.S.I.R. had to obtain special Treasury approval.

Perhaps the most interesting point in the 1953 correspondence, however, is that Mott, writing to Cockcroft, says:- "I gather that, next quinquennium, the big machines will be the responsibility of the UGC. Does this mean allocated grants?". Papers available in Bristol give no indication where this rumour originated, or how it grew. But by 1963 the UGC made funds available to the University, for the quinquennium starting on 1st. August, expressly to permit staff in the Physics Department, currently paid by D.S.I.R., to be transferred to full membership of the academic staff. Those involved were Malos and Hillier, together with two Research Associates and two Research Assistants. The existence of an "ear-marked grant" had thus been established in principle. The phrase continues to appear - for example in lists of staff - and by 1965 it was accepted as an almost automatic procedure, with phrases such as "SRC research grants for 1967-72 (quinquennial take-over)" and "proportion to be transferred to UGC in 1967". So much for the "cardinal principle" of 1950.

(c) RTSG’s and the Funding of Graduate Students

In 1963, the Authorities, and the Press, were much concerned about the rate of emigration of British scientists, particularly to America - the so-called "brain drain". On July 15th. of that year an announcement made in the House of Commons stated that the SRC and similar bodies would make a grant of £250 per year in respect of each post-graduate and post-doctoral research worker financed by them, to be paid to the university department in which he/she was working. These payments were known as Research Training Support Grants, and were supposed to be an inducement to scientists to stay at home. Their purpose, as stated, was to help with the provision of equipment, and to cover other incidental expenses incurred by the department in the training of the student. The final statement went on:- "The Government confidently expects that the Universities will not adjust the sums made available from University funds to these science departments, which would result in this relief being reduced". For reasons of administrative convenience, the money was paid to the university, rather than to individual departments. As a result, it has always been handled at Bristol by the Finance Officer, and - in Bristol, though not everywhere - it has tended to get mixed up with the main departmental finance from UGC sources. The Physics Department has sometimes entertained a suspicion - perhaps unjustified - that, in spite of the Governments "confident expectations", when the University has been allocating its scarce resources between departments, its thinking may to some extent have been influenced by the knowledge of the existence of several thousand pounds of income to, say, physics, from the RTSG's. Because of similar doubts, or perhaps because of reports of other forms of mis-use, the SRC thought fit, in 1976, to issue a "reminder" to universities about the intention behind the grants. In fairness, it should also be recorded that the SRC's interpretation of what was a proper use of the funds was very flexible, and that the Physics Department found it most valuable to have these monies available to meet exceptional and unexpected demands. When introduced in 1963 the grants were described as a temporary measure, but their continuation was recommended in 1967, and by 1969 it was agreed that they should be regarded as a permanent feature of the system. Their value was increased to £300 pa in 1974 and again to £400 pa in 1984. At this figure it still (1989) remains; but it is now firmly established - in Bristol - that the spending of the money is under the full control of the department, and the accounts are kept separate.

The scholarships held by, and the grants received by research students can be regarded, in a sense, as being indirect income to the Department. Records are available about the sources of these grants for six years in the period 1948-55, and for most years since 1964. Table 8.1 at the end of this chapter gives a summary of this data in so far as it relates to students who were candidates for a higher degree. Post-doctoral and senior research workers sometimes described as "visitors", have not been included in the list. Some of them would have been paid from sources similar to those listed, and some might have been holding temporary University appointments. Students taking the MSc course on the Physics of Materials, which ran from 1965 to 1983 are listed separately. (Table 8.2) At one period, in the early 1970's, there were several overseas students taking a preliminary course of study before embarking on an MSc or PhD proper: these too are not included.

The sources of the funds are very numerous and very varied. For convenience they have therefore been grouped under six headings, as follows:-

(1) SERC and its predecessors, SRC and D.S.I.R.
This group includes both students on maintenance grants from these bodies and the, much smaller, number paid from research contracts held by their supervisors. It also includes, in the early years, ex-service students being paid under the Further Education and Training Scheme, and later, one or two Advanced Course Studentships and an occasional State Scholar.

(2) The University of Bristol.
This includes those holding Graduate Scholarships and also those paid from either the Fertel Fund or Mott's Solid State Fund. From time to time there was the odd one whose salary was set against a Research Assistantship which happened to be vacant.

(3) Overseas includes students paid either by their home University or by the Government of their home country. The list is not short, but Australia, Canada, India, Pakistan, U.S.A. and Ireland predominate. Holders of Commonwealth Scholarships and 1851 Exhibitions, visitors under the Colombo Plan, together with people supported by UNESCO or the British Council are included here. One or two overseas students whose source of funds is not given in the records have also been included.

(4) Government sources include various Ministries - of Aviation, Defence, Supply etc.. Most of these would have been paid from a research grant to the supervisor rather than an award to the individual student. The group also covers Government research establishments and nationalised industries, e.g. A.E.R.E., S.R.D.E., CEGB, and also departments like DTI and the Post Office.

(5) Industry includes both individual firms such as Kodak, I.C.C., BP, English Electric etc., and also industrial research organisations like B.I.S.R.A., E.R.A. and the Rayon Research Association.

(6) Others includes those students listed as "self-supporting". It also includes small numbers paid by L.E.A.'s and various charitable trusts, by other Research Councils. e.g. NERC and MRC, by the Royal Society and the inevitable few "not known".

Even with this condensation, the numbers in any one group in any one year are quite small, and fluctuate considerably from year to year. They have therefore been further grouped, in time, as follows:-

(a) Six years between 1948/9 and 1954/5. (Data for 1952/3 are missing).

Data for 1955/6 - 1963/4 are missing.

(b) The seven years between 1964/5 and 1970/1.

(c) The seven years between 1971/2 and 1977/8.

Data for 1978/9 - 1980/1 are missing.

(d) The seven years between 1981/2 and 1987/8.

Advanced Course students are listed separately for the whole duration of the course i.e. from 1965/6 to 1982/3. The results for both sets are shown in the following Tables, both as absolute numbers and as percentages rounded to the nearest integer.

The following points may be noted:-

(1) The fraction of the total bill paid by SERC and its predecessors has always been large, and has increased steadily during the period covered.

(2) The second largest group, on the classification used, is the overseas students. This is true for both PhD and MSc candidates.

(3) The fraction supported by Industry and Government taken together has been about 7% for PhD's and 2% for MSc's, and, for the former, has declined steadily.

(4) The contribution from University sources is now smaller than ever before: it was zero for the MSc candidates.

(5) The fraction headed "Others" is much larger for the MSc candidates than for the PhD's. The reason is that greater numbers of the former group are described as "self supporting". This is much more easily arranged for a one-year MSc course than for three years for a PhD.

There are two small complications which ought to be mentioned. Firstly, when counting the number of graduate students for inclusion in the chapter on Statistics, PhD candidates in their 4th., 5th., etc. year were included. But how they were supported for the extra time is not always accurately known. The lists tend to record the source of funds that was operative in the three initial years, and in some cases it is doubtful whether subsequent years were financed in the same way. The numbers in the table were obtained on the assumption that they were. To estimate the order of magnitude of the possible resulting error, the data for the years 1981-88 ("d" in the table) have been recalculated on the basis of the first three years of PhD candidature only, for which the information is more reliable. Table 8.3 gives a comparison of the two results. It will be seen that the changes in the percentages are very small, and the error can be safely ignored as being within the noise.

The second point concerns CASE studentships. These Co-operative Awards in Science and Engineering were introduced by SERC in 1972/3. They had existed as Co-operative Awards in Pure Science for a couple of years previously, and the name change reflects the direction of the prevailing political wind. The co-operation of the title is between a university department and an industrial organisation, and takes the form of joint supervision of the work of a research student who is engaged on a project that is of interest to both parties. The firm sometimes makes a payment to the student to supplement his grant from SERC, and always makes a payment to the university department, as a contribution towards the expenses of the research. These students have been entered in column 1 of the table, and the fact that some part of the money should really appear in column 5 has been neglected. In recent years we have had as many as ten such awards current at any one time. Since the financial contribution is typically of the order of several hundred pounds per year, this implies several thousand pounds additional income. This is a welcome addition to resources, but is small enough to be lost in the departmental budget as a whole.

CHAPTER 9

PERSONAL REMINISCENCES

As a light-hearted appendix, I thought it might be of interest to give some "personal reminiscences". These are really a collection of trivia which would ill fit into the general text, but may serve to give a flavour of the atmosphere in the Department, particularly in my earlier years here. I arrived in Bristol in September 1933, as a very junior member of the Department. Although I did not realise it at the time, this coincided with Mott's arrival as Professor. In that year Professor Tyndall was External Examiner for the BSc degree in Sheffield, where I was a post-graduate student. While visiting in June for the purpose of discharging these duties, he offered me a job in Bristol, which I accepted after only a few minutes consideration. There were no other formalities. The salary was to be £400 per year. The award of my PhD was not confirmed until November. In October I received a letter from the Registrar at Bristol saying that Council had decided that "the grants for research in Physics should be named as follows :- (a) George Wills Associateships and (b) a Chattock Research Studentship" and that "during the tenure of your present post you should hold the Chattock Research Studentship”. I have never been clear about the origin of the funds which paid for these appointments. By 1937/8 I appear in the Calendar as Lecturer in Physics, and all reference to Chattock has been dropped. He had been Head of the Department in its early years, an able scientist, but an exceedingly shy and retiring man, as appears from Tyndall's notes in his History. Much later, in 1971, I tried to revive the practice of naming an appointment in his honour. I persuaded the physics professors that it would be a proper move, and in due course the suggestion was approved by Senate and Council. (Senate minutes, July 1971). The name was to be attached to an existing post, rather than to a newly established position, which would have been more difficult. There was some discussion with the Registrar's office as to whether the title Research Associate or Junior Fellow would be the more appropriate. In the end, the Calendar for 1972/3 includes the name of M.J. Folkes as Chattock Junior Fellow. He had been a Research Assistant since 1968. When he left in 1974, the title was again allowed to lapse, and nothing has been heard of it since. This seems to me to be very unfortunate.

In 1933 the academic staff numbered about ten, with about the same number of technical staff. There were two or three technicians in the workshop, John Priest the electrician, whose main job was to keep the big 110V accumulators in good condition, two or three men looking after teaching labs and lecture theatres, and Mr Venn. Mr Venn was an Institution. He was the Laboratory Steward and everything else - all things to all staff. He looked after the stores, - now rooms 2.11 and 2.12, but then fitted from floor to ceiling with splendid pitch pine cupboards containing everything, and all locked. When you wanted anything, from a vacuum pump to a piece of wire, the first thing was to find Mr Venn. His room (now 1.12) was, with proper dignity, next to the Main Theatre and the Theatre Apparatus (1.13). If he was not there, which was usually, one stood in the main entrance hall at the bottom of the staircase and shouted "Mr Venn" as loud as you could. There was usually a faint "yes" from some corner of the building, and contact was established. You then had to persuade Mr Venn that you really needed what you thought you wanted: a piece of rubber tubing was particularly difficult to get. All very inefficient maybe, but it worked.

Among his other duties, Mr Venn acted as lantern operator (i.e. projectionist) whenever there was an important lecture, particularly one by an outside speaker. The "lantern" was a massive epidiascope, made, I think, by Zeiss, which stood on an equally massive, purpose-built wooden trolley, on castors, in front of the front bench of the main lecture theatre. It projected its picture on to the sloping screen across the corner of the room. The operator sat on the front bench. Half of his job was to insert and remove the slides, as required by the speaker. The instrument could cope with slides of all sorts of different shapes and sizes - except 35mm. The other half of his job was to tend lovingly to the arc which was the source of light. It ran off the 110 volt DC mains, and used big fat carbon rods. The relative position of these had to be continuously adjusted, or else the arc either went out, or made hissing noises which were very distracting to the speaker. There was also another, smaller projection system on an optical bench, and intended for use in demonstration experiments in optics. This also used a carbon arc, with much thinner rods. It was more sophisticated, and, when once set going, it maintained itself in proper adjustment by means of a clockwork motor controlled by relays energised by the current to the arc.

A second Institution was the departmental secretary, Alice Masters, who later married and became Mrs Terry. Besides dealing with Tyndall's not inconsiderable correspondence - except for the letters which he wrote himself, by hand,- she did all the departmental typing (lecture lists, exam papers and the like), typed all the papers that went out for publication, and kept the departmental accounts, including the Order Book. She probably did all the PhD theses too, but I can't be sure of this. After Mott had arrived and settled in, a second secretary, Mrs Langdon, was appointed to look after his correspondence and the work of the theoreticians. But Alice carried on with most of her duties.

The third Institution was Mrs Greed, the porter (portress?) a little old lady who was, I believe the widow of the gardener who had looked after the Royal Fort grounds before the laboratory was built. She had been appointed in 1927, with a "stipend" of £3/3/6 (£3.17), presumably per month, and lived in the little lodge just inside the main gate. This may have been rent-free accommodation, but I have been unable to verify this point. During working hours she sat in the little porter's lodge at what is now the back of the building, then the front, and answered The Telephone. (Tyndall had one in his office, as well.) When there was an incoming call, she went scurrying round the building looking for the recipient, who then had to come down to her lodge to answer it. She also distributed all incoming post, dealt with all the outgoing post, including maintaining a detailed record in the Post Book, and occupied any spare time that she had while sitting in her lodge by mending lab. overalls for the technicians and patching dusters. She also provided tea every day for the staff and research students. This consisted of tea *ad lib*. and little jam sandwiches, made on the spot, for which one paid twopence per day. It was surprising the number of people who "forgot" to pay, and it later transpired that she had been subsidising the institution out of her own pocket. She retired in 1950. When she died, in 1954 - an event said to have been accelerated by malnutrition - she left a legacy to the University, which was to be used to help any member of the departmental technical staff who was in temporary financial difficulty. Two such cases spring to my mind: a technician who had all his tools stolen from the workshop, and another who was away sick for a very protracted period. The Sarah Greed Fund still exists, and is used from time to time for its original purpose.

I must also mention John Burrow, also an Institution - in that he spent his whole working life in the Department - but in a quite different category. He took a degree in chemistry at Bristol in 1926, and when he expressed an interest in employment as a glass-blower Tyndall was delighted to take him on. He was sent on a course at the University of Leiden, and on his return became a mainstay of much of the research in the Department. He manufactured all of the many diffusion pumps used for producing the high vacua that were much in use. These were at first mercury vapour pumps, and later, oil pumps, using the newly developed Apiezon oils. It is perhaps worth mentioning that these low vapour pressure oils were developed by C.R. Burch, when he was at Metro Vickers before coming to Bristol. In the process he, Burrow, made many modifications to the detailed design, to improve performance. He manufactured all the large, special purpose Dewar flasks, without which most of the work of the Low Temperature group would not have been possible. During the war years he was seconded to the Admiralty Signals School, but continued working for them in his usual rooms on the second floor. He was responsible for devising and perfecting the technique of making a vacuum seal between glass and copper tubing, so making possible the production of the famous cavity magnetron. Much later, the introduction of commercial metal vacuum equipment changed the nature of his work, but he remained an invaluable asset to the Department. He was the founder, and for some time the President of the British Society of Scientific Glass-blowers, and many of the assistants that he trained are to be found in various laboratories. He was originally appointed as a Technician, but in 1945 a special staff post of Recognised Teacher in Laboratory Arts was created for him. Not that this made any real difference to his standing in the Department. He had always been on equal and friendly terms with everybody, and always willing to be helpful. Using an improvised arc-melting furnace, he grew crystals of compounds of interest to Pryce in his work on colour and spin-resonance. From 1967 until his nominal "retirement" in 1971 he was graded as a Research Fellow. After "retiring" he took a series of part-time jobs, preparing, and growing crystals of, various exotic compounds that were used by other members of staff in their researches. At the same time he was an invaluable informal "adviser" to the Stage III project students, putting at their disposal his life-time's experience of laboratory techniques.

When I arrived, the laboratory buildings had been in use for about six years, and were still rather spacious in relation to the number of occupants. I started working with another new arrival, a soft-spoken Australian by the name of Syd Williams, and the two of us occupied the room now designated 1.28. Our joint apparatus sat on one small laboratory bench in one corner of the room, and the rest was empty, permanently blacked out, and lit by one or two pathetic filament lamps hanging somewhere near the middle of its cavernous volume. It is perhaps worth mentioning that, in those days, no member of staff, except Tyndall and Piper, had an "office"; everybody else just had a desk in the corner of the room in which experiments were done. Williams and I, as post-doctoral research workers, didn't even qualify for a writing table between us. Perhaps because my PhD work had been in spectroscopy, we were set to work under the supervision of Dr Appleyard, investigating excitation potentials for some of the bands in the spectrum of nitrogen. I do not remember being given any choice in the matter, and I was much too young and inexperienced to have any views of my own. After we had produced some results, meriting no more than a short note in "Nature", I found myself transferred, and working with Dr Skinner on soft X-rays. I have no idea what happened to Williams. In retrospect, it is clear that the spectroscopic problem was fairly trivial, whereas the work on which Skinner was engaged was the start of a major contribution to the band theory of solids. These facts must have been clear to my seniors at the time, but I am fairly confident that I myself did not appreciate the point. My PhD course had lasted just two years: I had worked with very little supervision, in a small department, investigating a rather obscure point about band-spectra. There were no post-graduate lectures or colloquia, and only one other PhD student to talk to. I must have been quite unaware of any of the broader aspects of physics, and totally ignorant of the exciting developments taking place at the time. Yet I was said to be a good examination candidate, and obtained a good first-class degree. I sincerely hope that the newly-fledged PhD of today is better informed and less immature.

With Skinner, I found myself working as a rather superior grade of laboratory assistant. Almost all our equipment was home-made: that meant either the work-shop or the glass blower for the highly skilled jobs, and Skinner and myself for the rest. Laboratory technicians in research groups were unknown. I remember in particular a large high-voltage condenser, needed to produce a high-power spark that would give very broad lines in the extreme UV. These were used as a source of continuous radiation for measurements on other absorption lines. It was built from many sheets of window glass, many square yards of aluminium foil, and gallons of transformer oil, the whole thing being contained in a domestic water tank which must have been almost a metre cube. When one of the plates punctured and cracked - a not infrequent occurrence - the pieces had to be removed and a new plate inserted - a very messy job indeed. The home-made mercury diffusion pumps were driven by electrical heating elements, which frequently failed due, I suspect, to induced surges from the near-by spark system. The heating elements were also home-made, from nichrome wire and asbestos paper and string. Nobody contracted either mercury poisoning or asbestosis. When dealing with X-ray emission spectra the cathode was earthy and the anode at high potential. The anode had to be water-cooled, and this involved a rack holding yards and yards of glass tubing, along which the water ran from the tap to the anode and from the anode to the sink. This was suspended overhead, to get it out of the way, and on more than one occasion the rubber joints between the sections of the glass tubing failed, with unfortunate consequences. Nobody was electrocuted.

The room, now G.19, was always in a state of utter confusion and mess, and its occupants frequently nearly as messy. If need be, Skinner could operate machine tools and do glass blowing himself. The essential elements of his work were always perfect; any part that was less than essential received scant attention. Appearances counted for absolutely nothing, either in his apparatus or his person. He tended to keep rather unorthodox hours, and if he went home leaving me to do something, he would frequently telephone me as soon as he got in the house to make some suggestion that had occurred to him while driving. He was the first example that I had met of the dedicated and single-minded research worker. He was in fact a very competent physicist, combining theoretical insight with a flair for experimentation. In personal relationships he was less satisfactory. He seemed to expect everybody else to be as brilliant as himself, and made no attempt to disguise his low opinion of anybody who was not. Many a visiting colloquium speaker must have been disconcerted by his comment "but surely it is obvious that ..." delivered with an expression that managed to combine a smile and a sneer, and prefacing some devastating criticism - usually well-founded. When I began to work with him I knew almost nothing about the band-theory of metals, and when I finished, such understanding as I had gained owed very little to Skinner. It just never occurred to him that he knew far more about it than I did, and that some explanations would have been helpful and welcome. Nevertheless I did not feel aggrieved at this state of affairs, and thoroughly enjoyed the time I spent working with him.

In 1935 Tyndall wrote to D.S.I.R. asking for money to finance some experimental work on the optical properties of metals. There was a need for some measurements to supplement the theoretical work of Mott, Jones and Zener. It was proposed that I should carry out these measurements. I suppose I knew about this at the time, but I have no recollection of it: I knew nothing about measuring the optical properties of metals. A grant of £270 was approved for the year October 1935 to September 1936. What in fact happened was that I made a lot of measurements of the Hall effect and the magneto-resistive effect in bismuth alloys, a problem which it appears was suggested by Harry Jones. I wonder if D.S.I.R. knew about it. For this work I moved into the room occupied by Jackson, since low temperatures would be involved. This time I was on my own, designing and building the apparatus with some help, where appropriate, from Jackson. This help was freely given, but normally Jackson worked entirely on his own. He was an extremely quiet and reserved man, and had little contact with the other staff, and, as I remember, no contact at all socially. He owned neither car nor bicycle; his habits were as regular as clockwork and his desk-top always perfectly tidy. I found out later that he had a chip on his shoulder about the way in which Tyndall had treated him at some point. I never knew the cause of the trouble, and I doubt if many people - including Tyndall - knew of its existence. It was exceedingly rare for him to say anything about it, which made it all the more surprising that, when he did, the depth of feeling seemed to be quite intense.

Life in the field of low temperature research was more complicated in those early days. About once a week BOC would deliver a flask of liquid oxygen. Any other cryogenic materials you made yourself. Jackson had built a little device which stood on the bench, supported by a retort stand, into which you fed liquid oxygen, and nitrogen gas from a cylinder, and out of which emerged a trickle of liquid nitrogen. He also made a hydrogen liquefier - and when I say "made" I mean made it with his own hands, for example patiently threading one piece of metal capillary tubing through another to make a heat exchanger. This apparatus lived in the basement, and needed continuous attention from a skilled operator, who had to stand in front of it and keep a watchful eye on the pressure and temperature indicators, making minor adjustments from time to time. On the day when hydrogen was to be made, Jackson arrived at eight o-clock and disappeared into the basement with a packet of sandwiches for lunch. All being well, he would emerge again about three in the afternoon, triumphantly carrying a flask containing about a litre of liquid hydrogen. This would be transferred to his apparatus for measuring paramagnetic susceptibilities, based on the principle of the Sucksmith balance, and also built by himself. After about an hour, the temperature would have stabilised, and measurements could begin. He would then spend several hours standing and taking readings through a travelling microscope, alternating these with readings of an ammeter, to give the magnetic field, and a mercury manometer, to give the vapour pressure and thence the temperature. At about seven in the evening he would return home, happy in the knowledge that he now had material that would enable him to plot another couple of points on his graph. Occasionally he would make hydrogen for someone else to use, but one thought twice before asking this favour too often.

One day all did not go well, and there was a loud explosion from the basement. Subsequent investigation suggested that the cause had been the presence of some impurity in the hydrogen, which had solidified and blocked up one of the capillary tubes. This had caused a high pressure to build up in some part of the apparatus not designed to withstand it, and a piece of copper tubing had burst. Fortunately there had been no big explosion of the hydrogen gas, and nobody was seriously hurt. The amount of damage, apart from the liquefier itself, was very small. The ruptured piece of copper tubing, mounted on a little wooden plinth, stood on Jackson's mantelpiece at home for several years.

In September 1933 the Registrar received a letter from a 22-year-old German student, resident in Paris, asking for permission to study in Bristol, and giving details of his university work so far, in mathematics and physics. He had left Germany before reaching the stage of submitting his PhD thesis. As part of the programme of helping such refugees, he was given a maintenance grant, and was allowed to embark on a three year course that would lead to a Bristol PhD, without payment of fees. His name was Klaus Fuchs. He left in 1938 to take up a post in Edinburgh. I had no direct contact with him while he was in Bristol, but saw him around the Department, a very reserved and seemingly very shy and earnest young man. Alec Merrison, who worked closely with him later at Harwell for six months, told me afterwards that he, too, never really got to know him. He was quiet, gentle, earnest, with no sense of humour, and absolutely honest in his scientific work. He seems to have been a sincere idealist, but rather naive about his own affairs. When he eventually admitted to those things of which he was accused, he expected that all would be forgiven, and that he would be left to get on with his scientific work in peace. He was, said Merrison, "astonished" when two police officers appeared, and he was arrested.

In 1938 I was offered, and was happy to accept, a part-time appointment as Physicist in the Radiological Department of the Bristol General Hospital. This was a new post; there had never been any such position before, and I expect that Tyndall organised it. I continued with teaching duties in the University and (I think) with some research as well, and divided my time between the two institutions on an agreed basis. The totality of physical apparatus in the hospital comprised a commercial X-ray dose meter made by an American firm - Victoreen - which was used routinely to give some kind of check on the output of the X-ray therapy machines. I quickly doubled the stock of instruments by making a little gold-leaf electroscope which could be used for checking for leaky radium needles. The method was to keep a piece of cotton-wool for 24 hours in a glass tube along with the suspected needle, and then to check the cotton-wool for radioactivity by holding it near to the electroscope. The cotton wool would then be dropped in the waste paper basket, I expect. We later somehow acquired a "clucking hen", which was an ionisation chamber about as big as a cocoa tin, and probably made from one, connected up to some electrical circuitry and a loud speaker. This produced the sound implied by its name when there was radiation about. It came into its own one Sunday morning, when I was called out to the Infirmary to look for a radium needle that had possibly been put into the furnace along with some soiled dressings. This meant systematically checking all the clinker produced by the furnace during the previous day's working. We found the needle in the end. I later made myself unpopular with our workshop staff by building an X-ray phantom which could be used to measure the X-ray dose at any point inside a large mass of material, supposed to have scattering and absorption characteristics similar to those of animal tissue. It consisted of about a cubic foot of a witches brew of paraffin wax, carnauba wax and other things, which was alleged to be the correct mixture. It was divided into slabs an inch thick: one of the slabs was divided into bars an inch square, and one of the bars was divided into one-inch cubes. One of the cubes contained the ionisation chamber. Since the idea was that the pieces could be stacked together so as to place the detector at any point, the building bricks had to be fairly carefully made, to avoid leaving any substantial air cavities. This involved a lot of work with a milling machine. Although I did this myself, the resulting mess did not endear me to the technical staff. I suppose there was some reason why we didn't use a tank of water instead.

Mrs Greed's teas were ready at half past four every day. They were in the room next to the library, now part of the extended library, and corresponding to the place now occupied by the issue counter. On Mondays, she made a point of collecting the cups and saucers and clearing away the plates by five o-clock sharp. Then all the chairs were turned to face the blackboard on one of the walls, and the colloquium began. It was more often than not given by a member of the Department; outside speakers were quite rare. The Librarian, Miss Littleton, brought in before tea a pile of about a dozen current numbers of periodicals, and many people took this convenient way of keeping up with the literature. I clearly remember one day in 1939 when a paper in one of the journals provoked an animated discussion involving, principally, Skinner, Powell, Harper and, later, Tyndall. It was the first report of neutron multiplication by fission. The implications and potentialities were very clear, and it was generally agreed that "somebody" ought to alert "the Government" about what might happen. I think that Tyndall probably did write to "somebody" about it. Somewhere in his papers there is a reference to an incident that might well have been connected. Tyndall had written a letter to somebody about some such matter, which had been ignored. He was quite hurt about it. I suspect that he was not quite as intimate with the powers-that-be as he would have liked. But our archives do contain a letter to him from Lord Rothschild, dated 27/4/39, and reporting, in guarded language, a conversation with Fermi in New York, about a chain reaction, and the possibilities of isotope separation. It concludes:- "I have written to Goldney asking him if he has any objection to discussing it with Blackett, or even Dirac, but will wait till I hear from you before doing anything. Perhaps we could see them together." Another tantalising glimpse of behind-the-scenes activities.

The Department was big enough to contain an interesting mix of people, but not so big that it split up into smaller groups. Most people knew everybody else, and if there were any rivalries and jealousies, they did not obtrude into my knowledge. The atmosphere was friendly, and there was what could be called a "social life" running in parallel with the scientific work. At the date of my first arrival remnants of a rather formal structure still persisted. We junior members would occasionally be invited by Tyndall or Piper to come round in the evening "for coffee". The practice appeared to be to invite one or two close friends for dinner, and then to increase the size of the party by inviting others to arrive at a later time "for coffee". We all sat around making polite conversation, but at Tyndall's house in Henleaze Gardens the proceedings would be enlivened by table tennis. The table was in the attic, in a room that was just big enough, but which had a sloping ceiling with projecting beams above the table. Special rules had to be devised to cope with shots that rebounded from these hazards. At least this meant that a good player had no advantage, and the proceedings served very effectively to dispel any remnants of formality.

When, a year or two later, I married and we lived in a flat in Sneyd Park, the wives of the senior members of staff punctiliously "called" in the middle of the afternoon soon after we had moved in. On departing, they left three visiting cards on a dish on a little table in the hall, which every proper housewife was expected to provide for this purpose. There were two of their own cards, and one of their husband's - or was it the other way round? I am sure that it was all in accordance with the rules in the etiquette books, but I never did understand it myself.

But the old order was passing away, and, for the most part, a very friendly and informal atmosphere prevailed in the Department. A couple of examples will suffice. On most Sundays, there was a "lab walk". At ten o-clock in the morning, a group would gather at the Suspension Bridge. It might be three or four, or it might be ten or twelve. You just turned up if you felt like it, with a packet of sandwiches in your pocket. Fröhlich was almost always present, and he it was who had decided where we were to go. Nobody else knew, and nobody minded very much. We just walked and talked - often about physics, I would guess - until somebody suggested stopping for lunch. After eating, if Zener was a member of the party, he would climb up a convenient tree. When he came down again, we moved off and continued until we arrived somewhere that had a bus service back into Bristol - or else we just walked back.

Then there was the Christmas Party, which involved a larger group of academics and their wives, and graduate students, who didn't have wives in those days. Almost everybody turned up, and the routine varied little from year to year, although I find that most of my memories date from the post-war period. The organisers were volunteers, with a self-appointed "committee" to start things off and co-ordinate the efforts. The venue was the two adjacent Junior Laboratories (now 2.13 and 2.16) emptied of benches, and decorated with evergreen tree-prunings, courtesy of the gardener: the large lecture theatre, now the Tyndall theatre, was also used. There was often some kind of competition or guessing game, with the questions pinned up around the walls, to keep the customers occupied while the late-comers were arriving. There was always some kind of "entertainment" in the lecture theatre, often a version of a traditional pantomime, liberally sprinkled with in-jokes and topical allusions to departmental affairs. One year, when my research group consisted of one girl and four or five boys, we did Snow White; I of course, was cast as Doc. Among some of Tyndall's papers I came across a fragment of a script for such a production, in his hand-writing, which made it clear that he took the opportunity of making a few snide comments on the foibles of some of his staff, which might possibly have given offence in a less frivolous context. There was always food and drink in considerable variety and ample quantity, supplied by the participants. There was always carol-singing in the theatre, with John Bates vainly trying to keep the singers in time, having given up hope of keeping them in tune. At the end, there was dancing, and more drinks.

In the months preceding the outbreak of the war there was a scheme to familiarise the staff of university physics departments with the operation of the fledgling radar system. A group of half a dozen from Bristol, a mixture of academic staff, technical staff and research students, spent a week or so at an R.A.F. radar station near Poling in Sussex. We learned the language of the art, and had some experience of operating the rather simple equipment in use at that time. We were actually on site when war was officially declared and we must have returned there later for a more extended period, during which we had shifts "on duty". Nothing ever happened, but I remember driving to and from the station in my old car with minimal headlights. This stay was long enough to justify the renting of a furnished house - there were plenty around in that part of the world at that time- which was run as a kind of hostel for the party, by my wife and Mrs Harper. One day I was called for a interview in London by a Mr Brundrett, who later became quite well known. His job was to organise the recruitment of scientific personnel for temporary war work. He suggested that I should join the Admiralty to work on countermeasures to the enemy magnetic mines, which were receiving a lot of publicity in the press at the time. It may be that this was because I had just being doing some measurements on the magnetic properties of a nickel-manganese alloy which showed an interesting order-disorder transition. The connection between this and my proposed job was non-existent apart from the fact that the word magnetic appears in both. Perhaps that was sufficient for a harassed administrator: or perhaps my name was just picked with the proverbial pin. I was to report to HMS Vernon, at Portsmouth, at a specified date and time. I don't think I was given much option. HMS Vernon is what is known in the navy as a stone frigate, i.e. a shore establishment, and there I stayed until the end of the war.

Interesting though the experience was, it has no relevance to the history of the Wills Laboratory, except that it explains why I have nothing to say about what went on in Bristol during the period of hostilities. This is very unfortunate since it was during this period that the Department was invaded - if that is the right word - by both the staff and students of the physics department of King's College, London, and by a section of the Admiralty Signals Establishment from Portsmouth. I have come across not the slightest mention of either of these events in the departmental papers, nor, so far as I know, is there anything in Senate House. There is no information about how the arrangements were made, nor yet of their effect on the life and work of the Department - which must have been considerable. I think that this omission represents the only major gap in this history, but I am not in a position to do anything about it now. This is a pity.

When the end was in sight, I resumed contact with Tyndall - never completely broken, since I was officially on secondment, with my pension rights maintained - with a view to returning to Bristol. He was very willing to have me back, but took care to explain that I could go elsewhere if I wished. I didn't. The Royal Naval Scientific Service was being organised, and those of us holding temporary appointments were invited to join. I did not like what I had seen of this kind of work, and said so at some length in my reply. So I went back to Bristol. During my absence the Hospital work had been done by John Munson, who had been one of Tyndall's research students. The arrangement was working well, and since I was completely out of touch, it seemed pointless to try to pick up the threads again. So Munson stayed, and went on to a career in the National Radiation Protection Service. In the Department I was very busy preparing lectures, re-organising and equipping undergraduate teaching laboratories, clearing up the unholy mess that had accumulated in four years in the stores, and generally helping to get the place running again.

I contrived to arrange that two colleagues from Vernon should return with me. One was David Tanfield, who had come to Portsmouth immediately after graduating at Newcastle. He started on a PhD course at Bristol under my supervision, but after some time he left to take up a more congenial post in the physics department of St. Mary's Hospital, in Paddington. The other was Kevin Tindall, who had been a laboratory assistant in Vernon, and later a TEA III working with Tanfield. He joined us in a similar capacity a month or so later, being pleased to return to Bristol, where he had spent some months in 1940 working for the admiralty Signal Establishment when they were housed in the Physics Department. He stayed with us until his retirement in 1987, and is well known to everybody who had any contact with the Department at that time. His wide range of skills, and his willingness to have a go at anything, made him an excellent research technician. But it soon became clear that his character and personality, added to his other merits, made him an ideal candidate for the post of Laboratory Superintendent, and to this post he succeeded when Mr Venn retired in 1956. For his helpfulness and diplomacy during his tenure of that office, the Department owes him far more than most people realise. He has agreed to supplement my notes with his own account of life in the Department, and has permitted me to see a first draft of this document. It is only necessary to read it to see what I mean.

The Department to which we returned was - as is now clear to me in retrospect - notably different from the one that I had left. The passage of time would have brought about changes in any case, but the varied experiences during those years, both of those who stayed behind, and particularly of those who went away and returned, produced radical alterations which soon began to be manifest. The Department grew rapidly in size, and changed just as rapidly in character. A lecturer (with some exceptions) became no longer content to engage in research on a subject that interested him. He liked to have a personal research technician, and a "group" of research students. He liked to have an "office" as well as the laboratory in which he (sometimes) and his students (usually) worked. The group developed the habit of having a "coffee break" in the middle of the morning. This was indeed a valuable institution at which the day's problems were informally discussed with the supervisor. On the debit side, it sometimes meant that attendance at the departmental afternoon "tea break" declined, which was a pity, since that provided a much more broadly-based forum for the discussion of ideas. The group technician, abetted by the rest of them, wanted to have his own little mini-workshop, which had to include a lathe as a kind of status symbol. This I came to regard (later) as wasteful of space and resources, but the pressures were too great to resist. Apparatus grew in size and complexity, and an increasing fraction was purchased. In the changing circumstances, the fact that the student thus had to spend less time making his own was probably, on balance, a good thing. The departmental workshops continued to make some of the large and difficult items that were not available commercially, but such a central organisation is not very appropriate for doing the little job, that only takes half an hour, but is wanted NOW. Hence the mini-workshops. The Department has never taken kindly to centralised facilities. We had a departmental photographer at various times, but even so there were also little private dark rooms attached to research groups. There has never been anything akin to a typing pool: the very thought was horrifying. The result was that one of the more thankless tasks of that staff member who dealt with routine administration has been to re-shuffle the duties of the secretaries in post from time to time, to ensure a match between their skills and their work-load on the one hand, and the reasonable, or unreasonable, demands of the academic staff and the inexorable tide of administrative chores on the other.

The tradition of the Christmas party was revived, and I am assured that it is only from this date that the technical staff took part. I am surprised that this happened so late. But the character of the event gradually changed. The "entertainment" first became more elaborate, and lost some of its impromptu air. A stage was built in one of the lecture theatres, using laboratory benches. The moving spirit was Kevin Tindall, ably assisted by three of the older technical staff responsible for the undergraduate teaching laboratories, Ken Goble, Stan Edwards, and Maurice Rundle. Kevin, among his many other accomplishments, was an excellent raconteur, and not one to spoil a good story for the lack of a little corroborative detail, calculated to lend an air of verisimilitude to an otherwise bald and unconvincing narrative. He is the real origin of more than one of the supposedly humorous anecdotes in the folk-lore of the Department, in which I play a part. At the parties, he usually acted as compere. Some of the graduate students were quite active in the early post-war years. The secretarial staff were responsible for organising the decorations, and with the aid of staff wives, produced magnificent food. Most of the academic staff took little active part in the organisation. They turned up on the night, and stood around with their wives and families, chatting to one another. This rather detached attitude grew; graduate students played a continually decreasing role, and the organisation was left more and more to the technicians and secretaries. The decline in interest continued slowly, and, at a time after the period covered by these notes, the event was abandoned. Kevin's eminently readable story describes all this in more detail, and makes rather sad reading.

On the research side, I followed the usual pattern and built up a research "group". For some reason which I cannot remember, I first decided to interest myself in the mechanical properties of glass. I did a lot of reading of published papers, and spent a long time building a piece of equipment which was supposed to permit a measurement of tensile strength of a glass fibre that had never been out of a vacuum since drawing from the melt. It never worked, and I happily switched to some measurements of the creep behaviour of copper-silver alloys, to provide some data which could be compared with the predictions of a Mott theory of creep in precipitation hardened alloys. My first student, who worked on this problem, was Michael Davis, an ex-naval officer, who later went on to occupy a responsible post in the European organisation dealing with the control of coal and power. As usual, we started by building his apparatus. We made an extensometer involving a couple of galvanometer mirrors attached to the tensile specimen in such a way as to work as an optical lever. To measure the strains, the image of an illuminated slit, after reflection from the two mirrors, was observed through a travelling microscope. (Shades of L.C. Jackson!) We soon found that, in the first stages after applying the load, the movement was so fast that even two of us working together could not take readings fast enough. A solution was found by using an ex-R.A.F. aircraft cine camera, which would take single frames when the operator pressed a button. The camera photographed the scales of the travelling microscope, and also the dial of a stop-watch. All that the operator had to do was to try and keep his cross wire on the moving image of the slit, and to press the button whenever he succeeded in doing so.

It worked quite well. I have given the details as a good example of the research techniques in use at the time.

Davis was followed over the years by a succession of about a dozen others, usually Bristol graduates, but including two Australians. The pattern was to take on one new student each year, so that at any time the group consisted of three or four people at various stages of their work. In this way a certain amount of continuity was maintained. The subject of the researches was always some aspect of the mechanical properties of metals. However, interest moved from the original topic of creep under a tensile stress, to behaviour under alternating tension and compression, and thence to fatigue.

Through contacts made at conferences on such matters, I contrived, in 1966, to get myself invited to spend a year at the University of Illinois, in Urbana. This effectively put an end to my research group in Bristol, and it was not practicable to engage personally in research in America. I was to be there for only one year, and in any case, I was based in an engineering department, with a different kind of approach to problems of materials. But in 1964 our MSc course had started. I had been much involved in the planning and inauguration of this venture, and had successfully pressed that, although the title was The Physics of Materials, the emphasis should be on mechanical properties. There were already MSc courses running at other universities dealing with electrical, magnetic, and optical properties, and in particular with semi-conductors, but when we started there was no taught post-graduate course similar to the one that we planned. Staff recruitment was carried on with this development in mind, and resulted in the addition of Arridge and Ashbee to the staff, interested respectively in fibre composites and ceramics. Nye, Frank and Keller contributed to the teaching, and later Dingley joined the group. Thus the continuing interest in mechanical properties and their relation to structure grew from the seed originally planted by Mott, and continued to flourish.

As for myself, I found it increasingly difficult to keep pace with new developments, even in those topics in which I had been most interested. By way of excuse, I could say that I was much involved with administration and committees, Faculty Board and Senate in the University, and UCCA, SCUE, and the Institute of Physics on a wider front. Much of the work was concerned with physics education. I eventually took the unprecedented step of resigning my Professorship a year before I was due to retire, and arranging to be appointed as a Special Lecturer for the last year. The Vice-Chancellor and the Finance Officer were most co-operative about making the rather complicated arrangements. But I was told later that this creation of an unusual precedent caused quite a flutter in some Professorial dove-cotes. Being in this way gradually relieved of responsibilities, I had the opportunity to hand over to others all the numerous admin. jobs that I had acquired over the years, while still being around to see to it that the transition went smoothly. I was thus able to depart without causing any disturbance, and to leave the Department to carry on as before. I hope that somebody will be forthcoming to record all the changes of the next forty years.

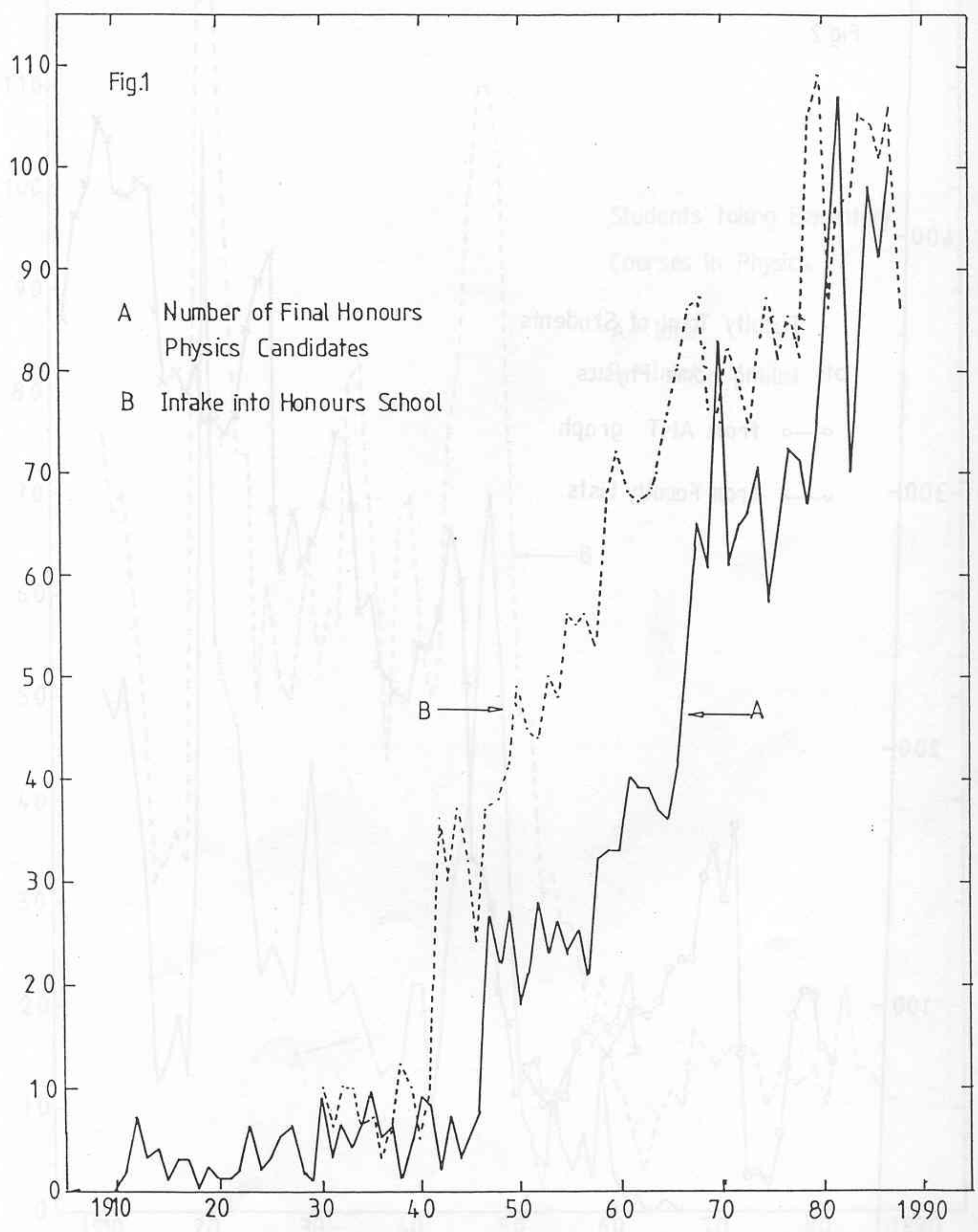


Fig.1
A Number of Final Honours Physics Candidates
B Intake into Honours School
B
A
0
10
20
30
40
50
60
70
80
90
100
110
1910
20
30
40
50
60
70
80
1990

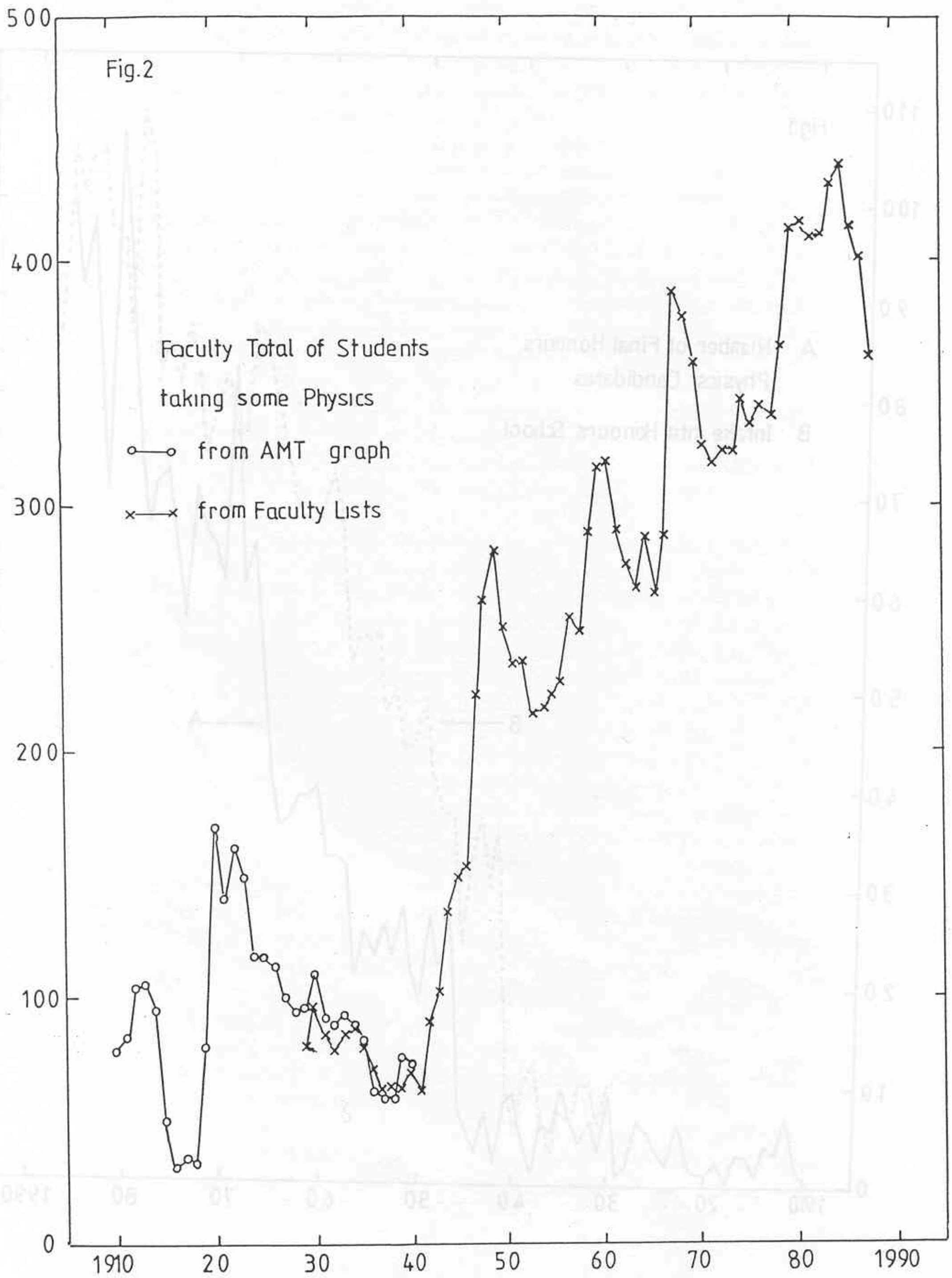


Fig.2
Faculty Total of Students
taking some Physics
from AMT graph
from Faculty Lists
500
400
300
200
100
0
1910
20
30
40
50
60
70
80
1990

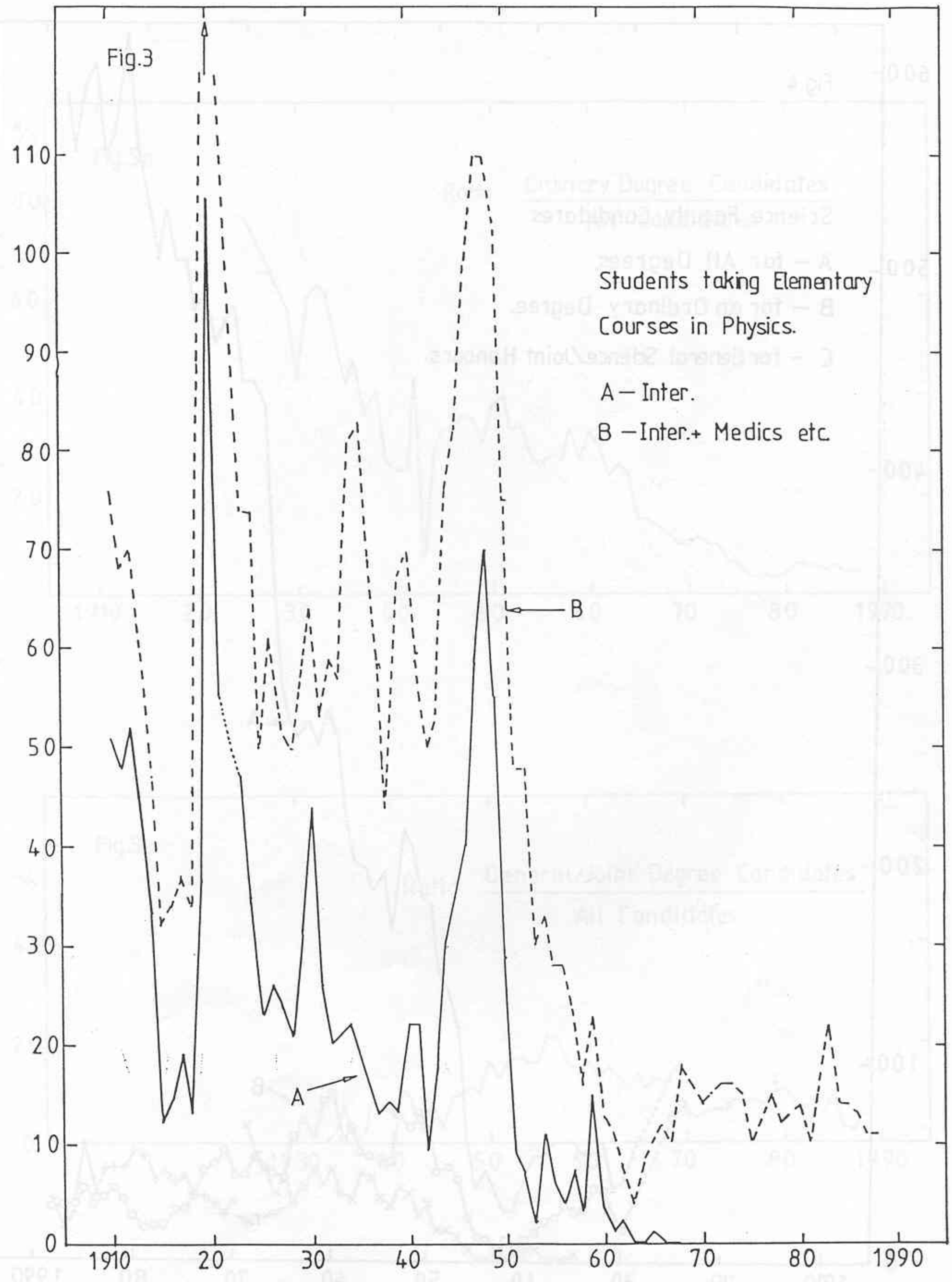


Fig.3
Students taking Elementary
Courses in Physics.
A — Inter.
B — Inter.+ Medics etc.
A
B
110
100
90
80
70
60
50
40
30
20
10
0
1910
20
30
40
50
60
70
80
1990

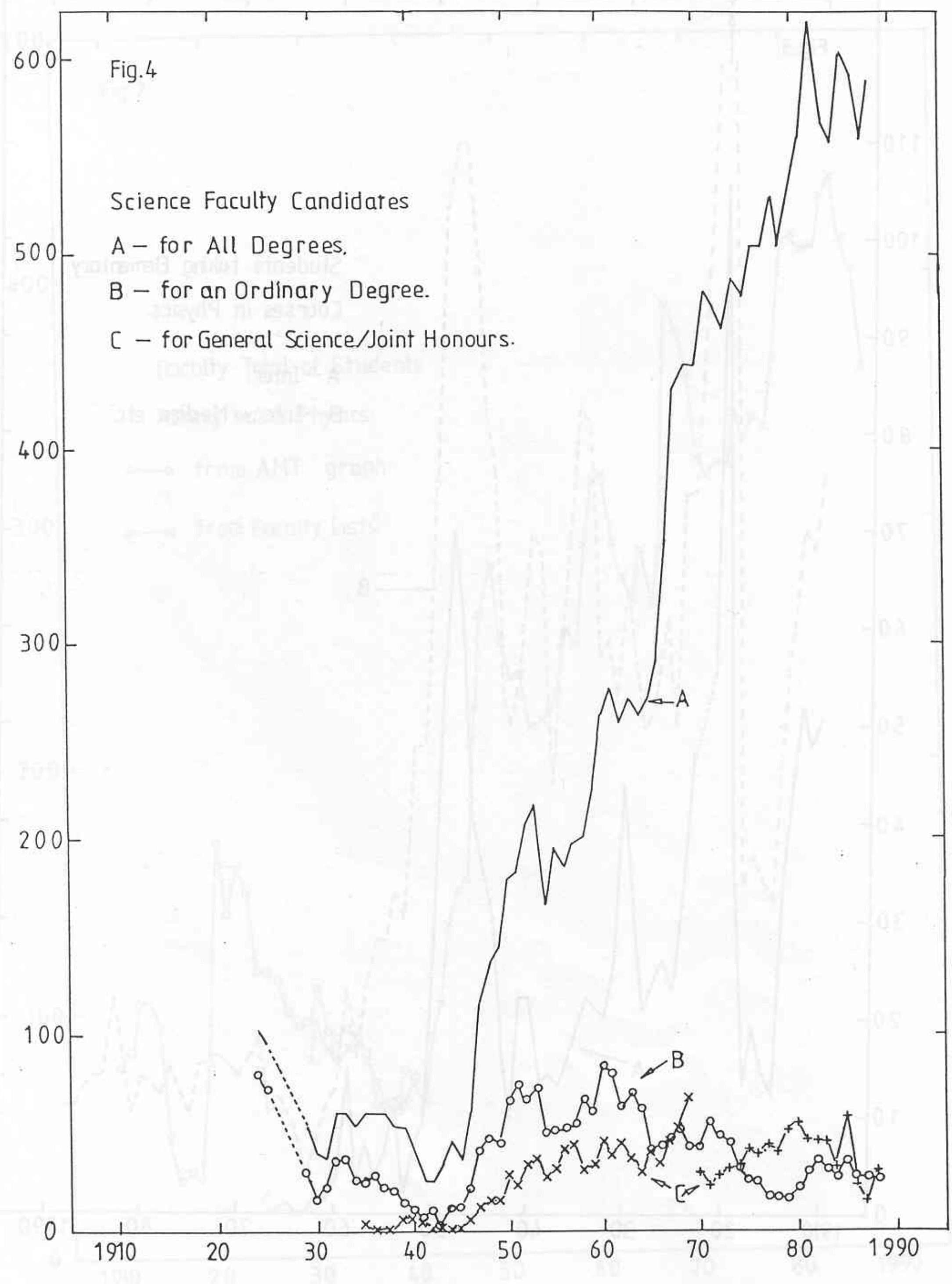


Fig.4
Science Faculty Candidates
A – for All Degrees.
B – for an Ordinary Degree.
C – for General Science/Joint Honours.
A
B
C
0
100
200
300
400
500
600
1910
20
30
40
50
60
70
80
1990

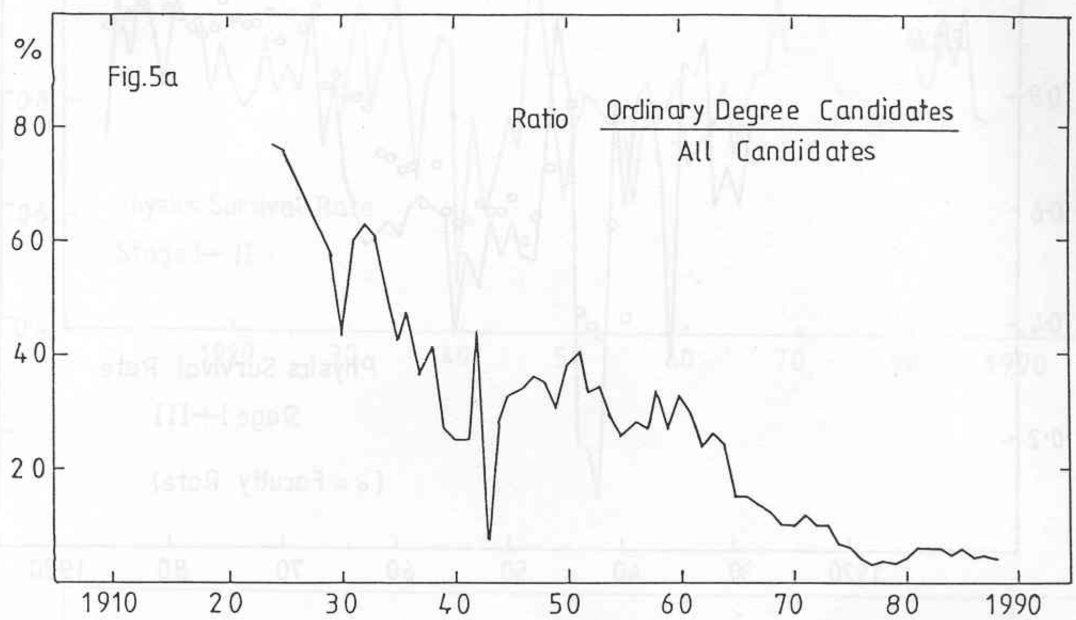


Fig.5a
Ratio Ordinary Degree Candidates / All Candidates
%
80
60
40
20
1910
20
30
40
50
60
70
80
1990

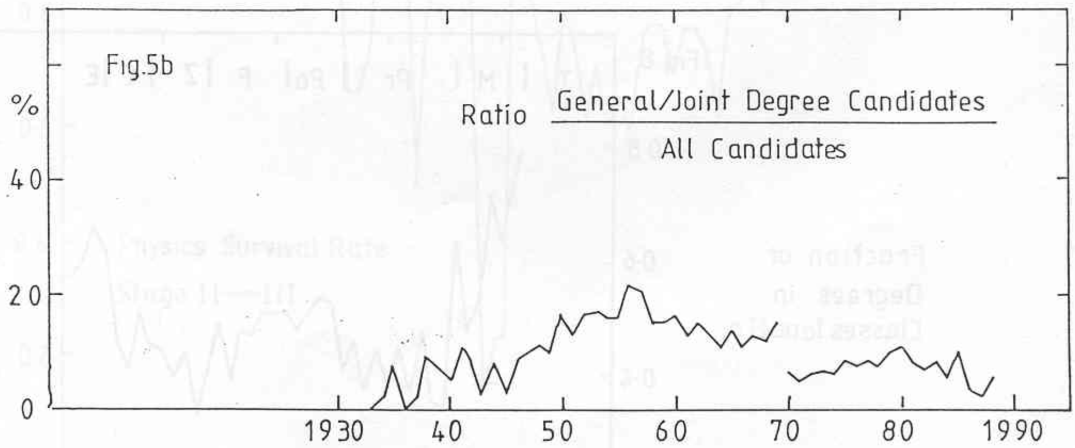


Fig.5b
Ratio General/Joint Degree Candidates / All Candidates
%
40
20
0
1930
40
50
60
70
80
1990

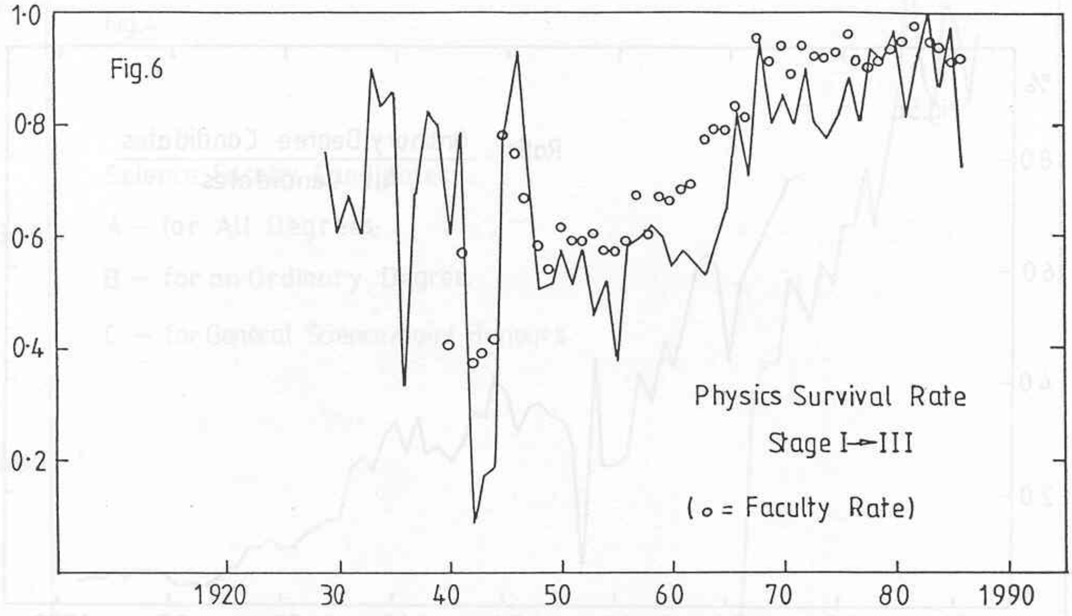


Fig.6
1·0
0·8
0·6
0·4
0·2
1920
30
40
50
60
70
80
1990
Physics Survival Rate
Stage I→III
(o = Faculty Rate)

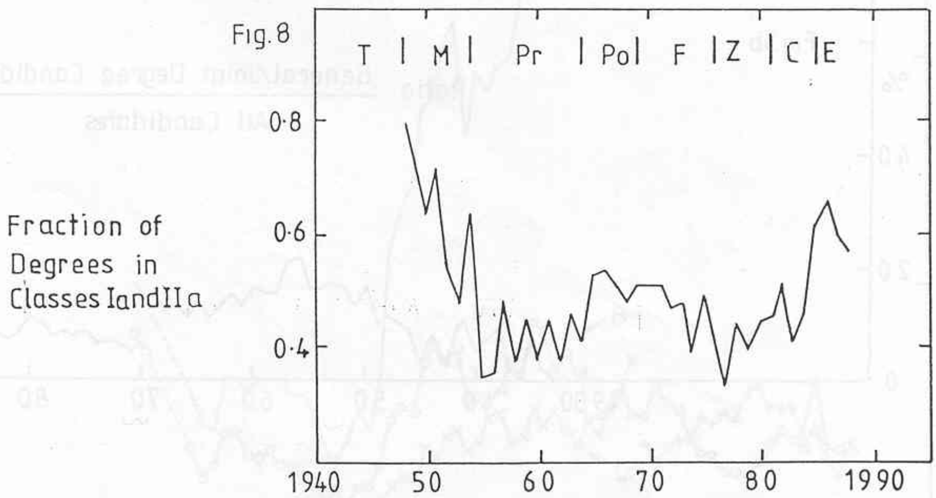


Fig.8
T | M | Pr | Po| F |Z |C |E
0·8
0·6
0·4
1940
50
60
70
80
1990
Fraction of
Degrees in
Classes I and II a

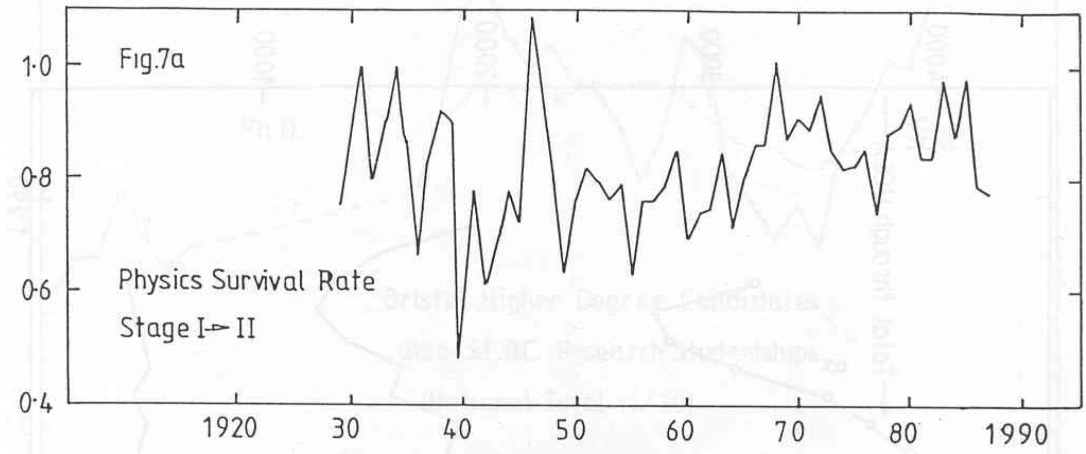


Fig.7a
1·0
0·8
0·6
0·4
Physics Survival Rate
Stage I→II
1920
30
40
50
60
70
80
1990

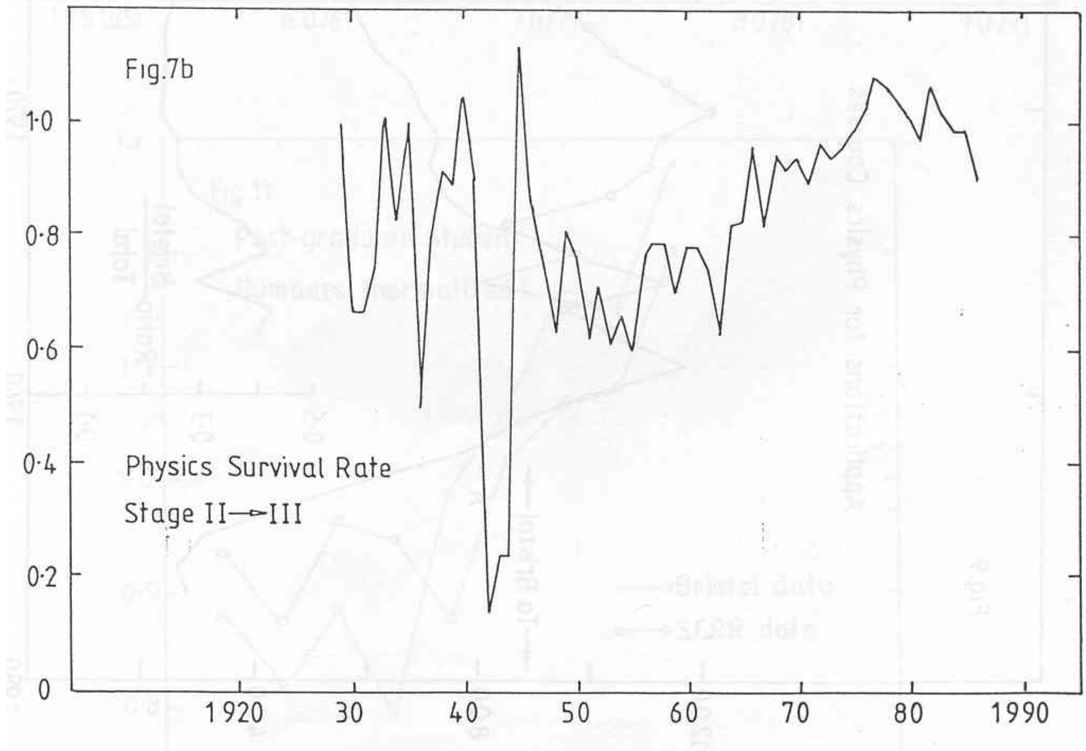


Fig.7b
1·0
0·8
0·6
0·4
0·2
0
Physics Survival Rate
Stage II→III
1920
30
40
50
60
70
80
1990

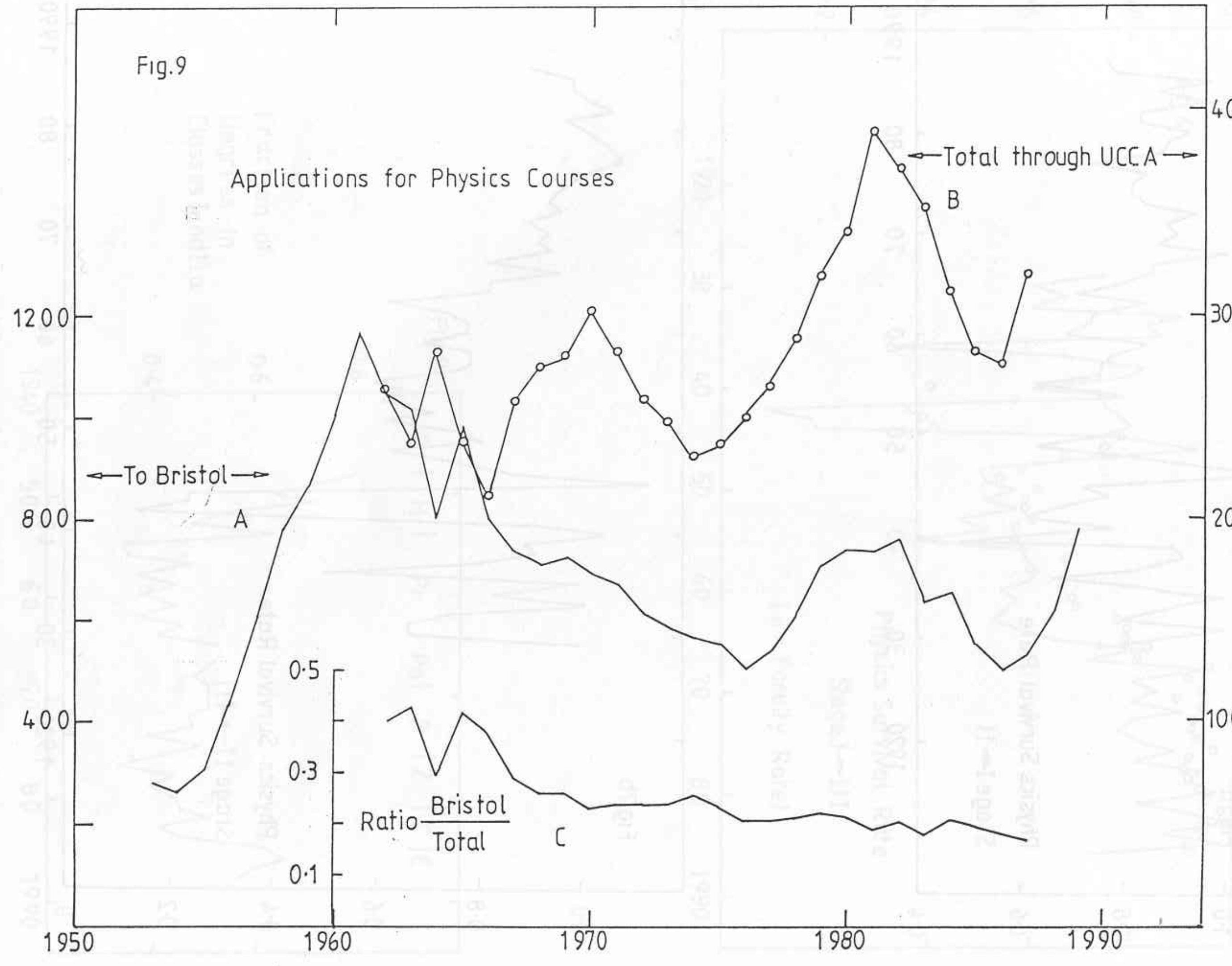


Fig.9
Applications for Physics Courses
←To Bristol→
A
←Total through UCCA→
B
Ratio Bristol/Total
C
1200
800
400
4000
3000
2000
1000
0·5
0·3
0·1
1950
1960
1970
1980
1990

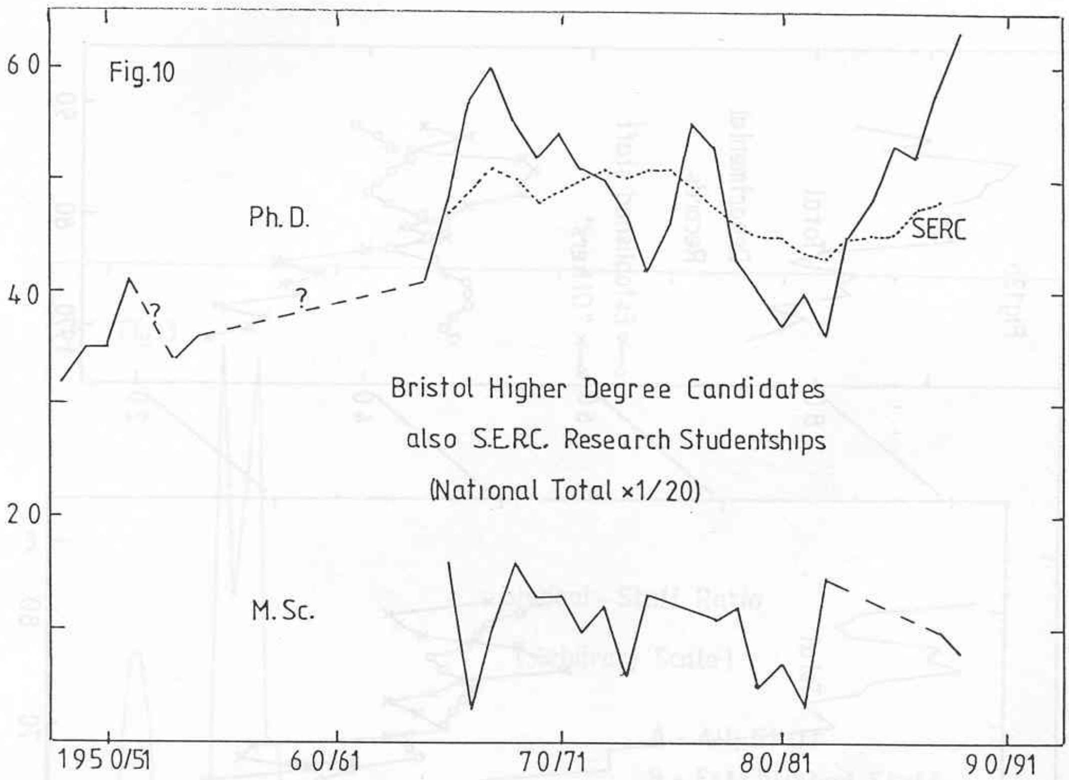


Fig.10
60
40
20
Ph. D.
SERC
?
?
Bristol Higher Degree Candidates
also S.E.R.C. Research Studentships
(National Total ×1/20)
M. Sc.
1950/51
60/61
70/71
80/81
90/91

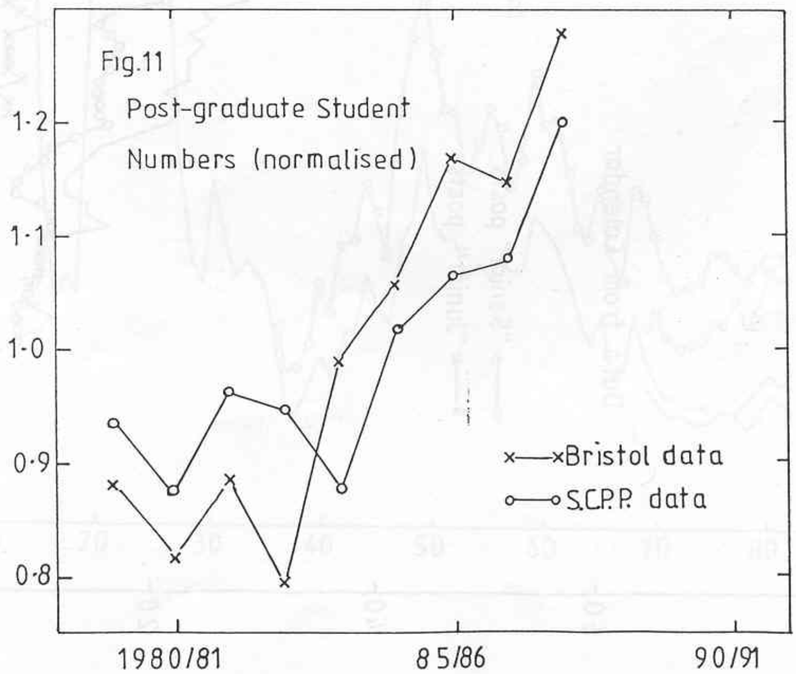
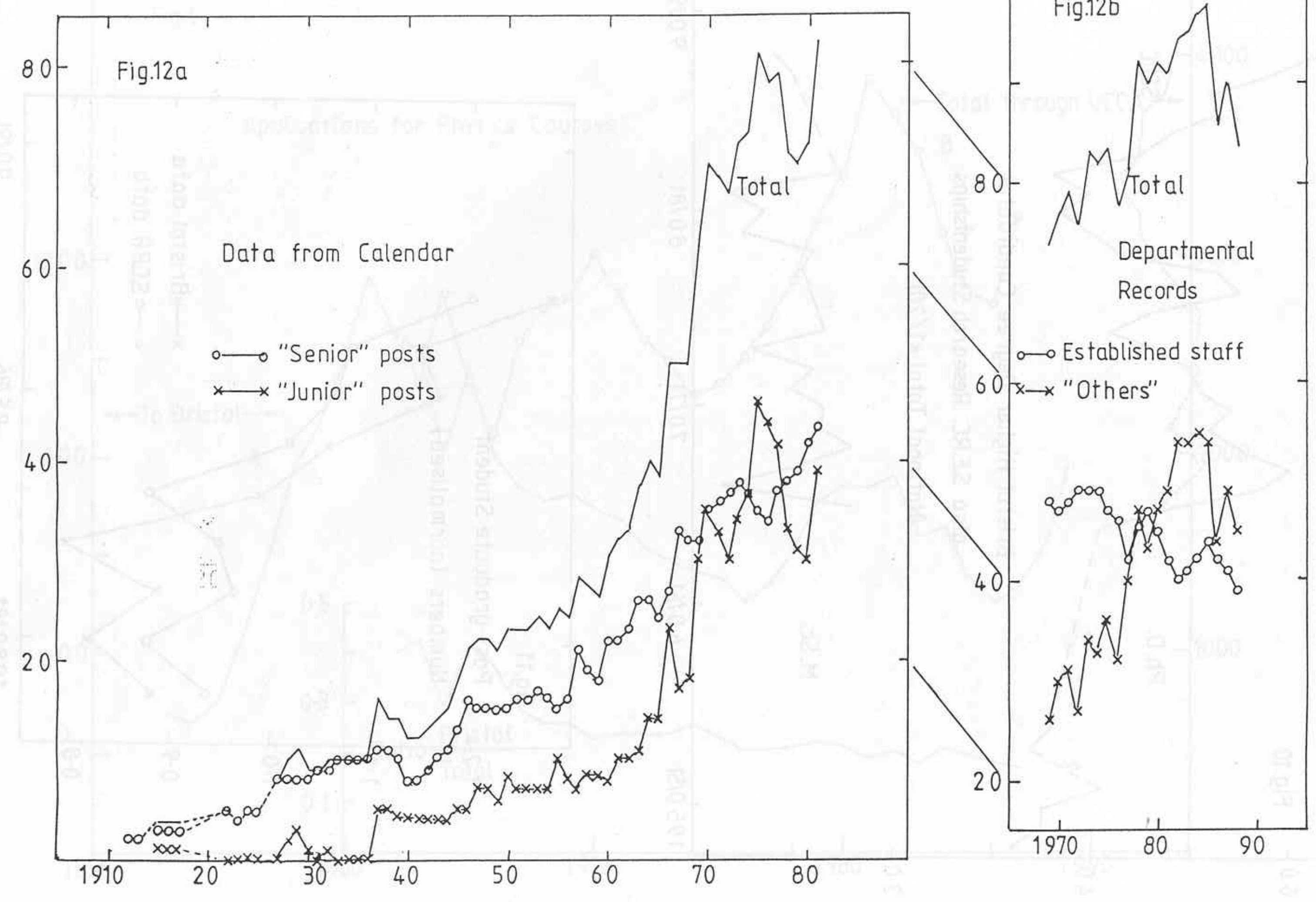


Fig.11
Post-graduate Student
Numbers (normalised)
1·2
1·1
1·0
0·9
0·8
×—×Bristol data
o—o S.C.P.P. data
1980/81
85/86
90/91

ACADEMIC STAFF NUMBERS



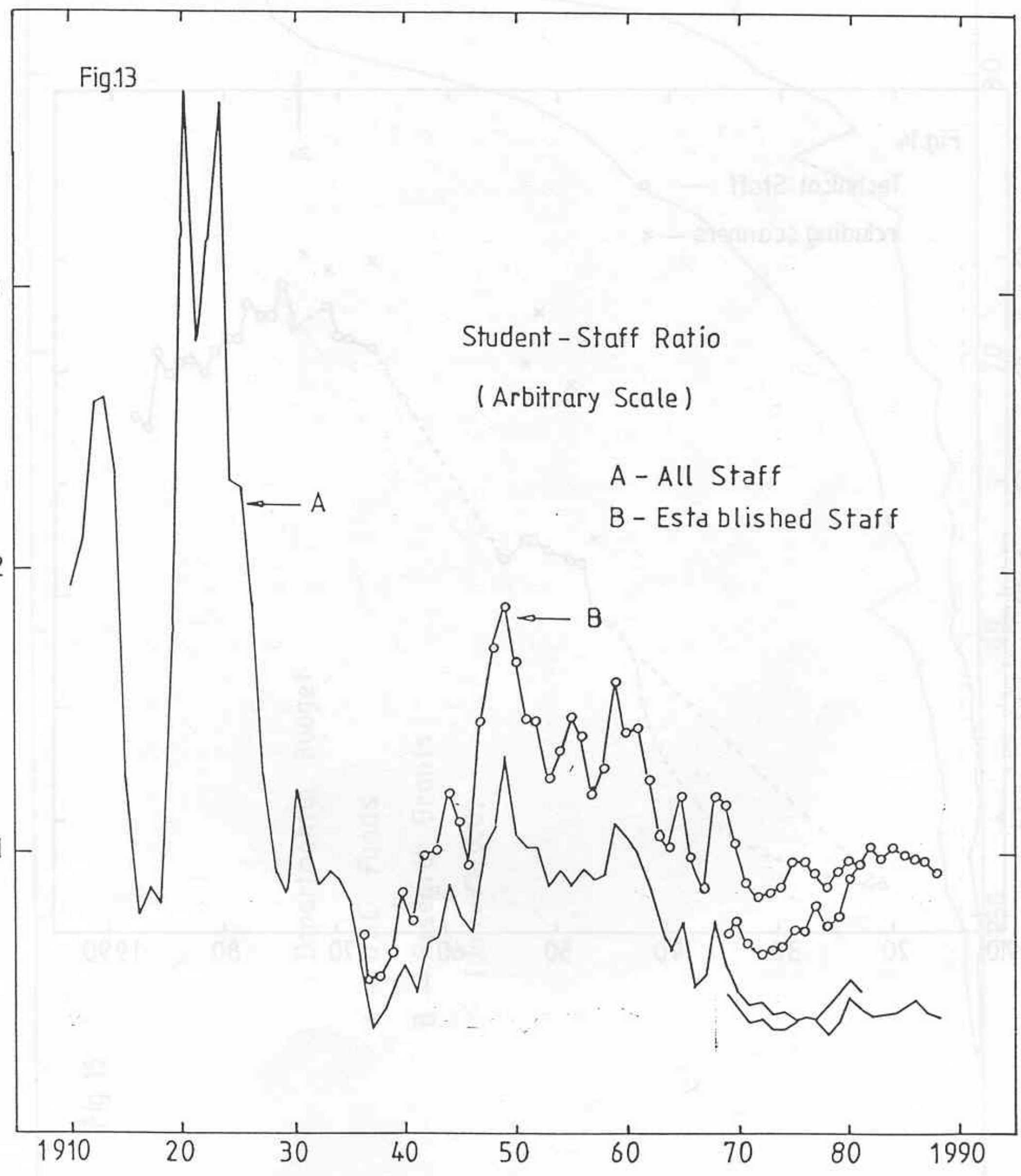


Fig.13
Student - Staff Ratio
(Arbitrary Scale)
A - All Staff
B - Established Staff
A
B
3
2
1
1910
20
30
40
50
60
70
80
1990

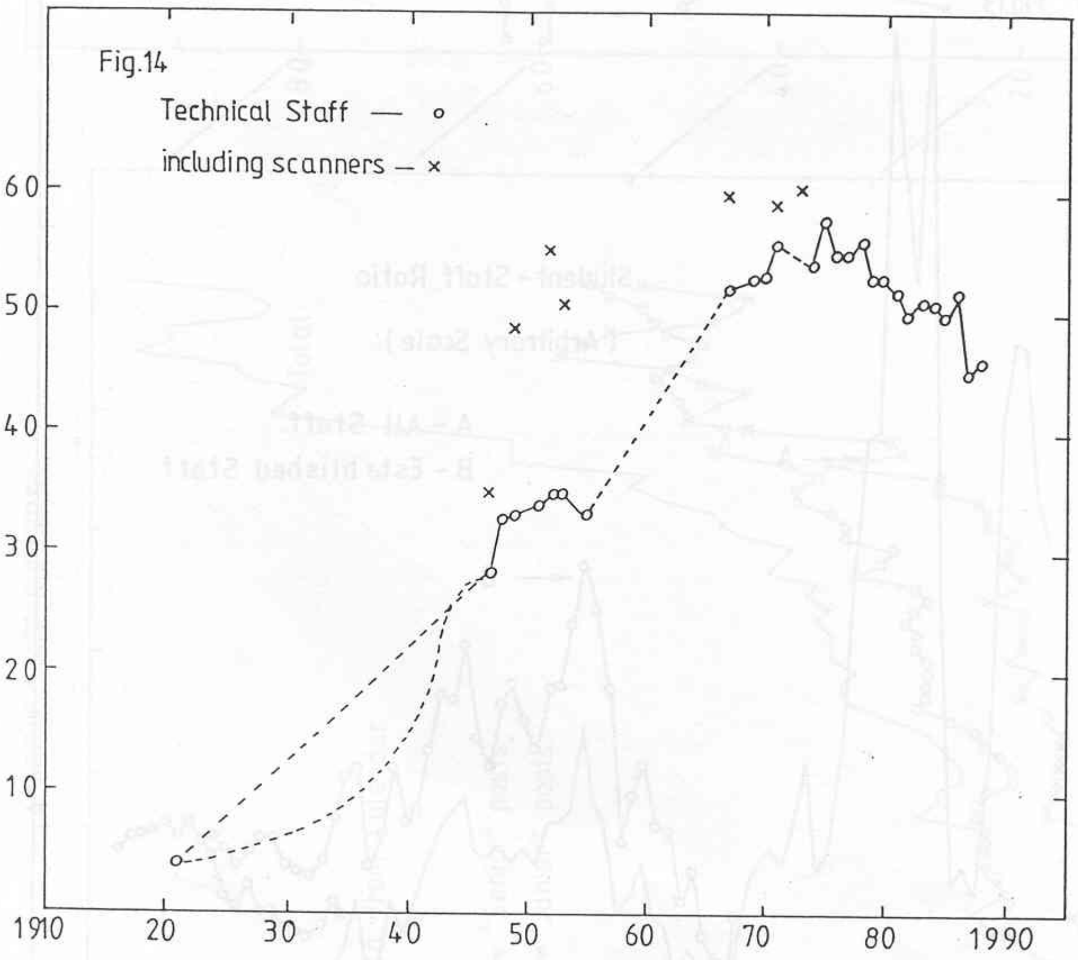


Fig.14
Technical Staff — o
including scanners — ×
60
50
40
30
20
10
1910
20
30
40
50
60
70
80
1990

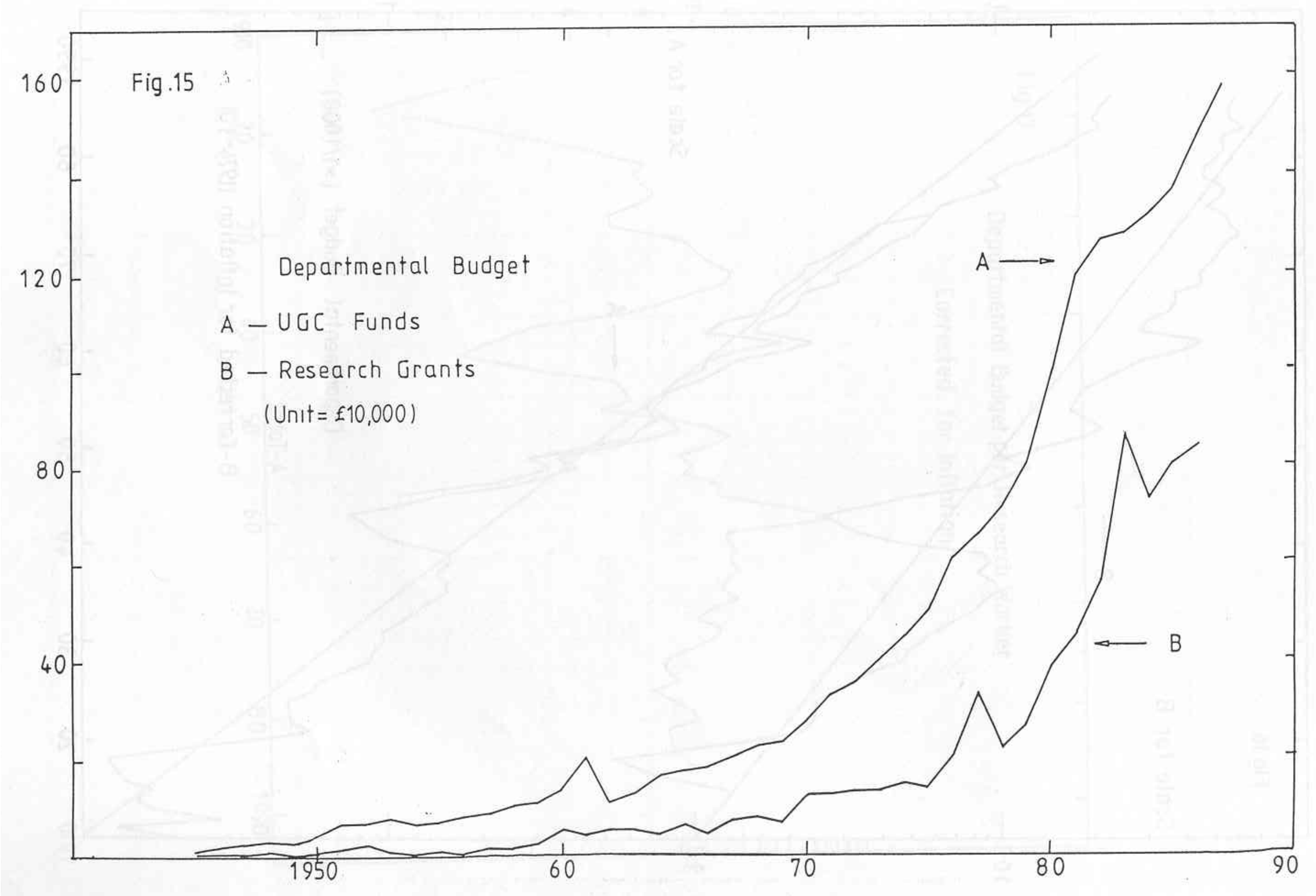


Fig.15
Departmental Budget
A — UGC Funds
B — Research Grants
(Unit= £10,000)
160
120
80
40
1950
60
70
80
90
A
B

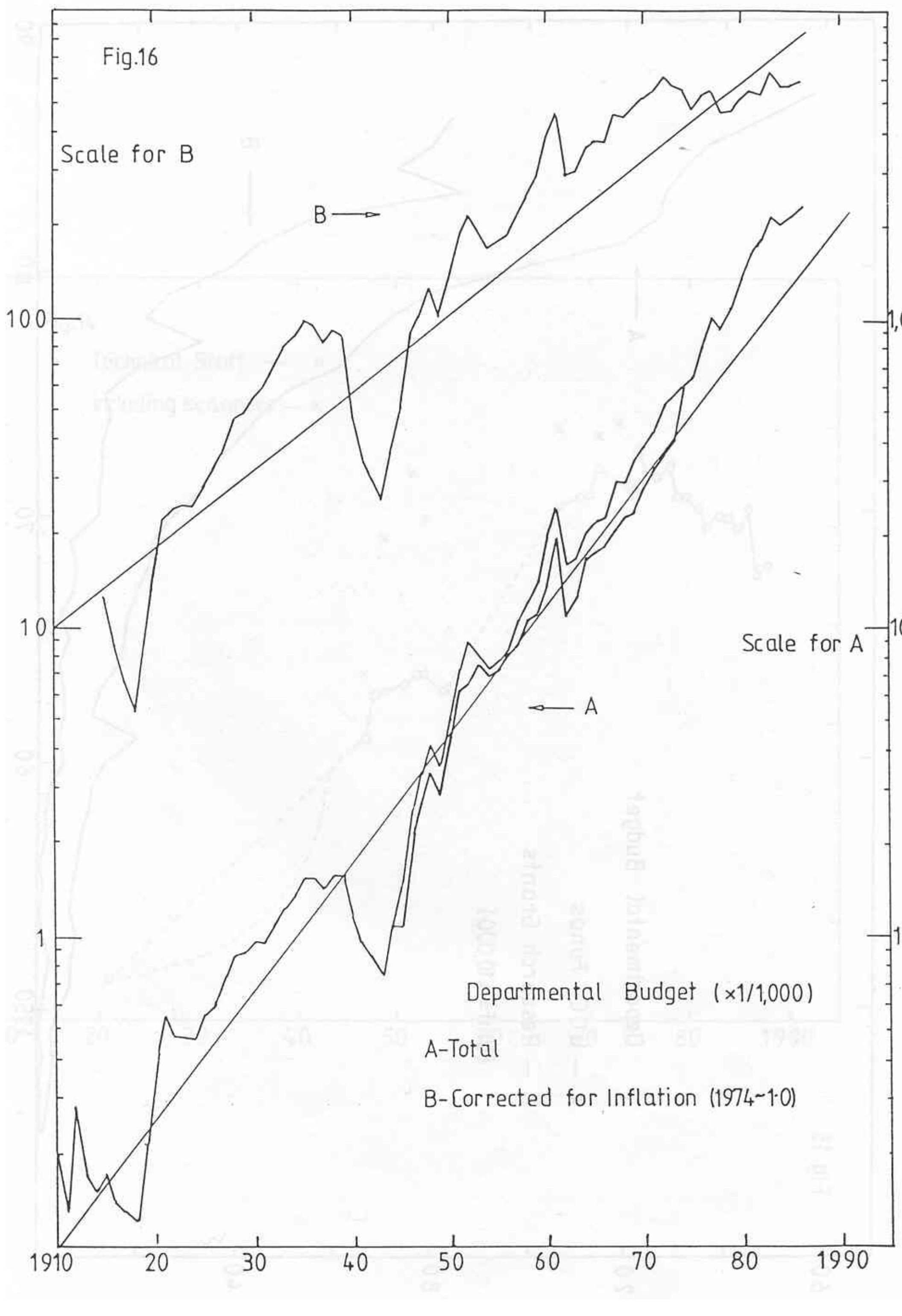


Fig.16
Scale for B
B
100
10
1
1,000
100
10
Scale for A
A
Departmental Budget (×1/1,000)
A-Total
B-Corrected for Inflation (1974~1·0)
1910
20
30
40
50
60
70
80
1990

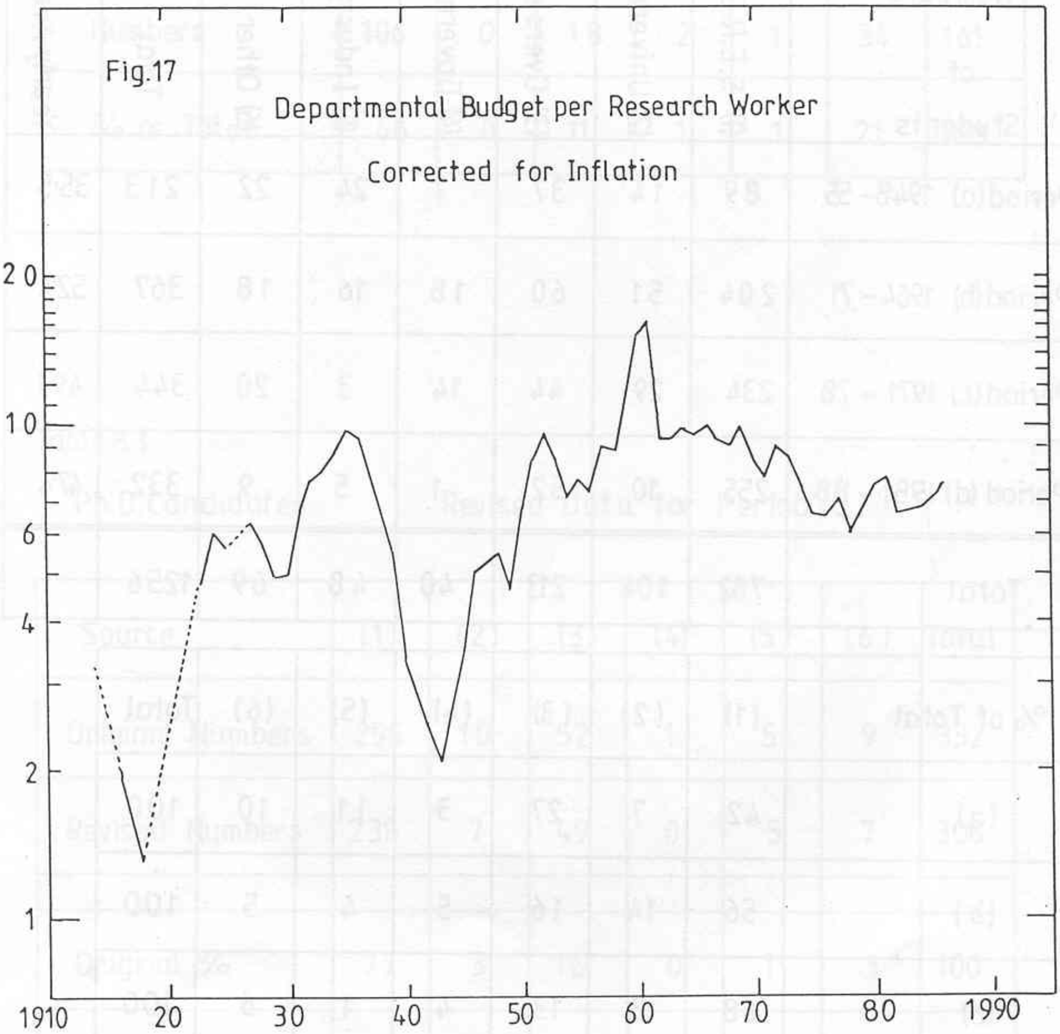


Fig.17
Departmental Budget per Research Worker
Corrected for Inflation
20
10
6
4
2
1
1910
20
30
40
50
60
70
80
1990

Table 8.1

SOURCES of FUNDS for GRADUATE STUDENTS

Ph. D. Candidates

Numbers of Students	(1) S.E.R.C.	(2) University	(3) Overseas	(4) Government	(5) Industy	(6) Other	Total	Yearly Average
Period (a) 1948-55	89	14	37	7	24	22	213	35·5
Period (b) 1964-71	204	51	60	18	16	18	367	52·4
Period (c) 1971-78	234	29	44	14	3	20	344	49·1
Period (d) 1981-88	255	10	52	1	5	9	332	47·4
Total	782	104	213	40	48	69	1256	

% of Total	(1)	(2)	(3)	(4)	(5)	(6)	Total
(a)	42	7	27	3	11	10	100
(b)	56	14	16	5	4	5	100
(c)	68	8	13	4	1	6	100
(d)	77	3	16	0	1	3	100
Total	62·1	8·3	17·0	3·2	3·8	5·5	100

Table 8.2

SOURCES of FUNDS for GRADUATE STUDENTS

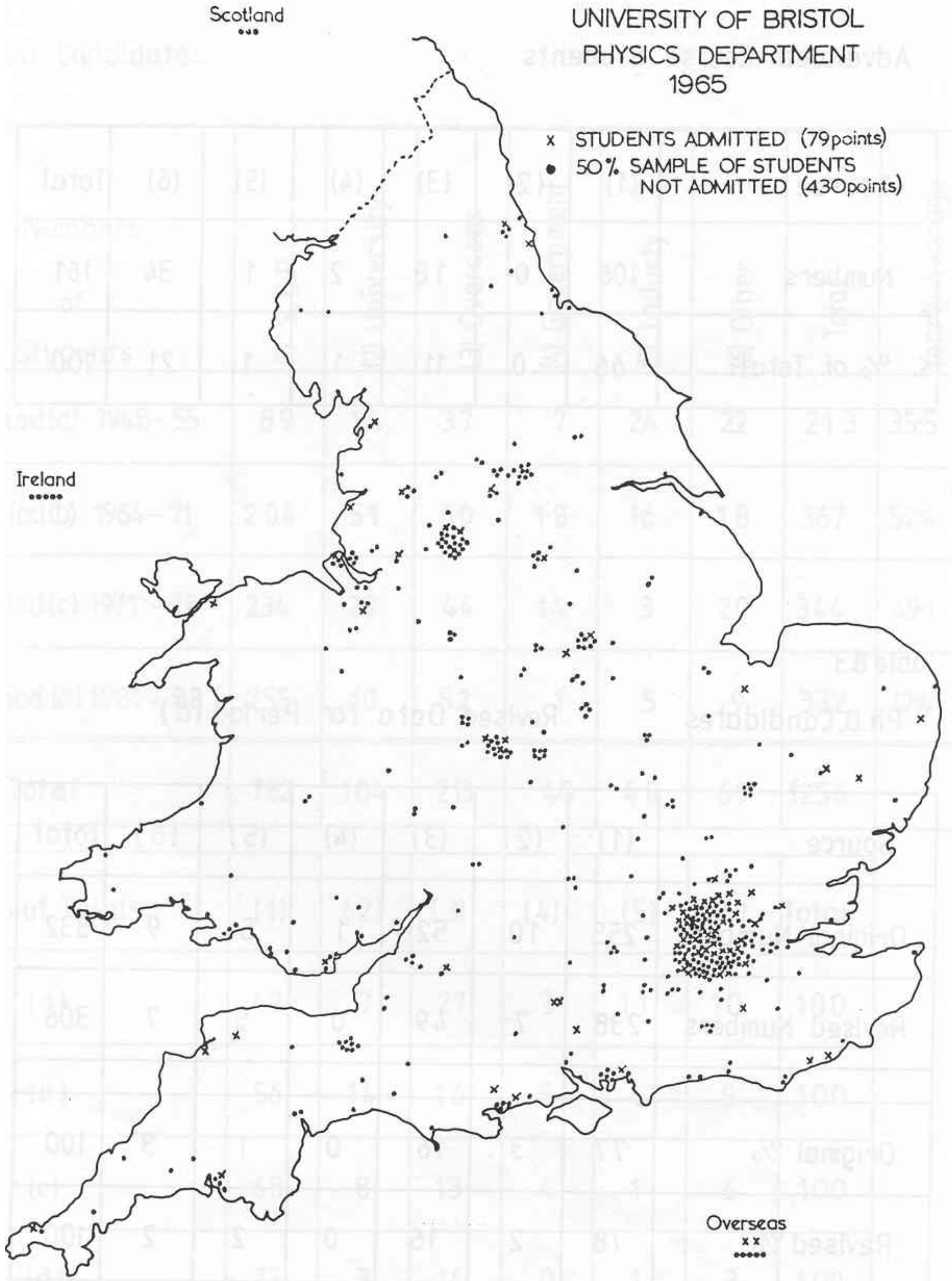
Advanced Course Students

Source	(1)	(2)	(3)	(4)	(5)	(6)	Total
Numbers	106	0	18	2	1	34	161
% of Total	66	0	11	1	1	21	100

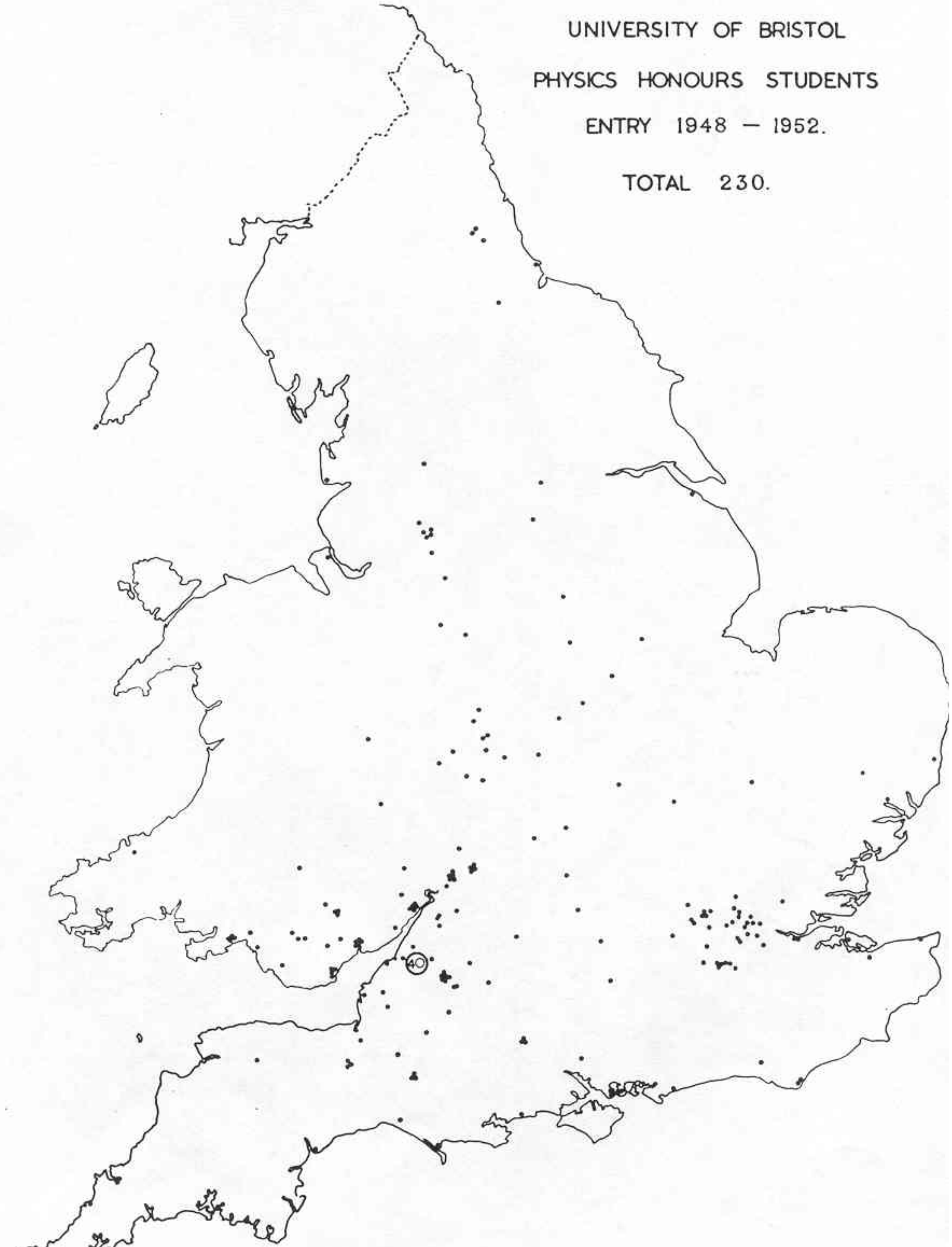
Table 8.3

Ph.D. Candidates Revised Data for Period (d)

Source	(1)	(2)	(3)	(4)	(5)	(6)	Total
Original Numbers	255	10	52	1	5	9	332
Revised Numbers	238	7	49	0	5	7	306
Original %	77	3	16	0	1	3	100
Revised %	78	2	16	0	2	2	100



UNIVERSITY OF BRISTOL
PHYSICS DEPARTMENT
1965
x STUDENTS ADMITTED (79 points)
• 50% SAMPLE OF STUDENTS NOT ADMITTED (430 points)
Scotland
Ireland
Overseas


UNIVERSITY OF BRISTOL
PHYSICS HONOURS STUDENTS
ENTRY 1948 — 1952.
TOTAL 230.
40