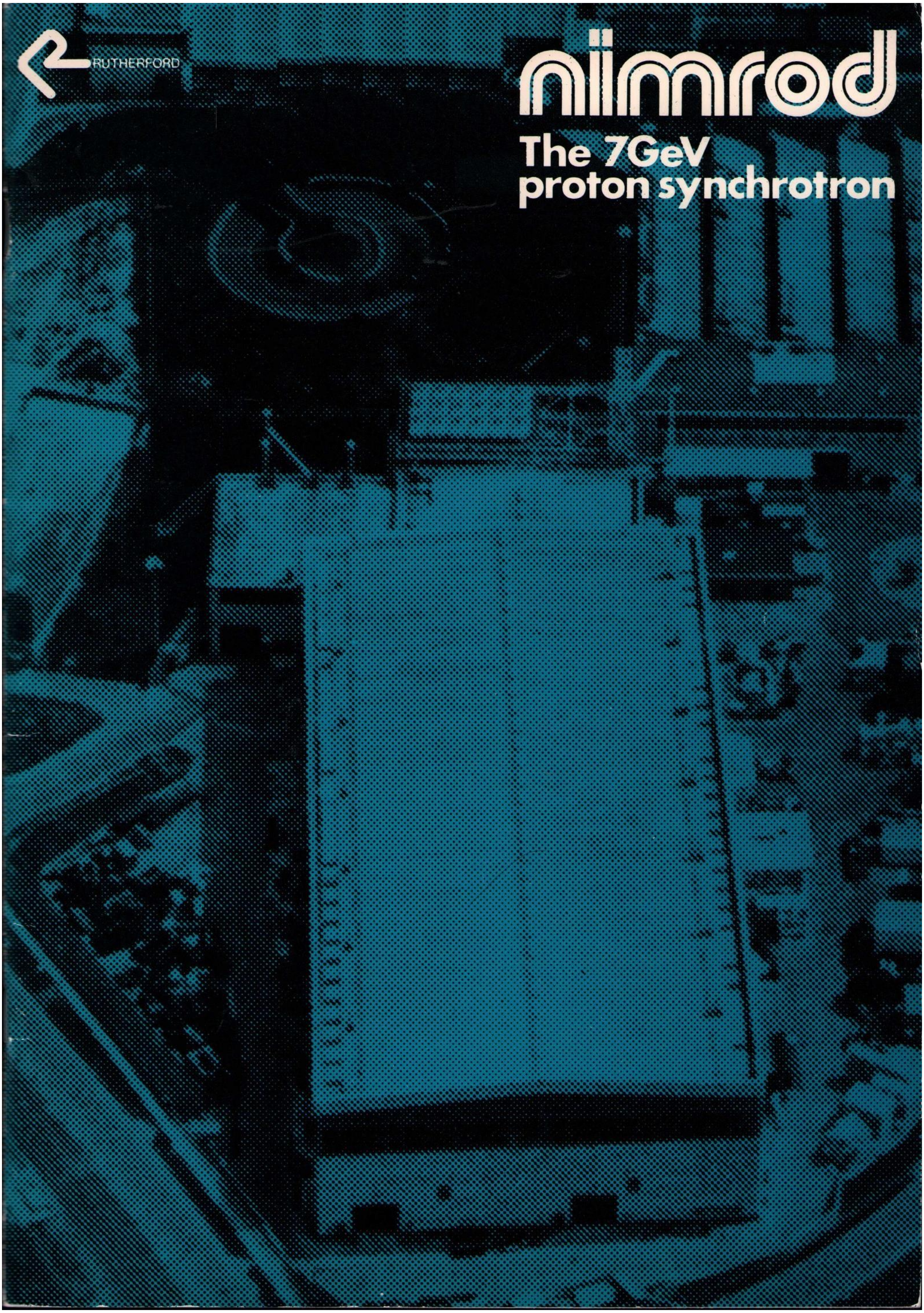
RUTHERFORD

nimrod

The 7GeV proton synchrotron



nimrod

The 7GeV proton synchrotron

Proceedings of a Nimrod
Commemoration Evening,
held at the Rutherford
Laboratory on 27 June 1978.

Edited by John Litt

Science Research Council
Rutherford Laboratory
Chilton, Didcot
Oxfordshire OX11 0QX
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DEDICATION

This volume is dedicated to
Dr T G Pickavance, CBE, FRS
first Director of the Rutherford Laboratory.

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FOREWORD

by Professor P.I.P. Kalmus

As a user of Nimrod for most of its lifetime, it is a pleasure for me to set the scene for this volume. In the early 1960s the number of physicists in the UK with experience of working at machine energies above 1 GeV had scarcely reached double figures. A few years later this situation was transformed completely, thanks to the successful operation of Nimrod. In the fourteen years from 1964 to 1978, several hundred physicists and postgraduate students from around twenty UK universities or laboratories and a dozen overseas institutions made use of the particles coming from Nimrod. In addition Nimrod was used by biologists, medical scientists and people in other disciplines. The results stemming from this accelerator have been published in hundreds of articles in scientific journals, presented at many conferences and formed the basis of over a hundred postgraduate theses at the participating universities. A whole generation of physicists received their training at Nimrod, and many of these are now at the forefront of particle physics research at CERN and elsewhere.

Following a decision of the Science Research Council to concentrate its high energy physics activities around the accelerators at the CERN Laboratory in Geneva and to support UK participation on the PETRA machine at Hamburg, Nimrod delivered its last particles at 17.00 hours on 6 June 1978. A brief closing ceremony in the main control room was followed by refreshments in the Laboratory restaurant. Three weeks later on 27 June a more formal commemoration took place. Under the chairmanship of Sir Harrie Massey, a number of talks were presented, and these are reproduced in this volume. Sir Denys Wilkinson talked about the events surrounding the construction of Nimrod. Dr. Leo Hobbis described the Nimrod project, and Professor Dick Dalitz reviewed the physics carried out at Nimrod. Finally, Dr. Godfrey Stafford made brief remarks on the future of the Rutherford Laboratory.

For the majority of its users who are still relatively young, the story of Nimrod, its history and pre-history as seen through the eyes of the distinguished speakers, gives a fascinating insight into the science and politics surrounding their former workhorse. For those of us old enough to have been involved in the experimental programme from the beginning, but too young to have had any responsibility in the initial decisions, the design of Nimrod always seemed rather conservative, and perhaps we now understand better the boundary conditions prevailing at the time. Of course, with the wisdom of hindsight, Nimrod should have been built as a strong focussing accelerator of higher energy. However, we were lucky. The Nimrod energy region proved to be an extraordinarily fruitful one, and much of our knowledge of hadron spectroscopy stems from this machine.

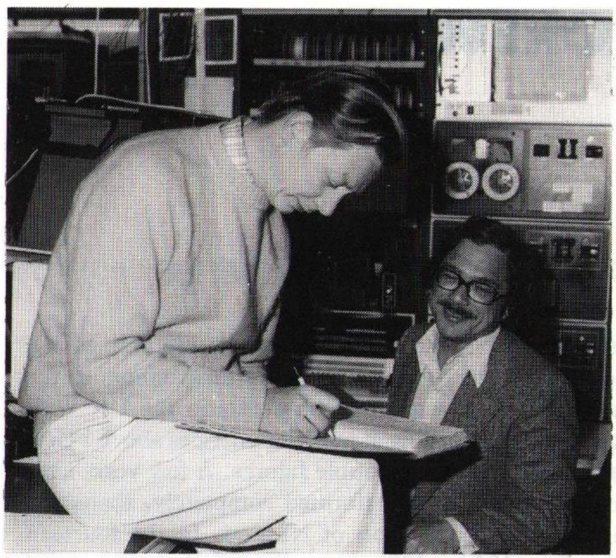
On behalf of the users I would like to thank the designers and constructors of Nimrod, the machine crews and all the Rutherford Laboratory staff who made it possible for us to base our research at this accelerator.

Drs. Godfrey Stafford and Gerry Pickavance at the Nimrod Closure Ceremony on 6 June 1978.

Nimrod's Last Shift

As 1700 hours approached on Tuesday 6 June 1978, four teams of particle physicists were hastily completing their final shift on Nimrod.

1. The main users on the K20 beamline were from Queen Mary College and the Rutherford Laboratory.
2. An Oxford University and Rutherford Laboratory team were checking out the ISIS detector, destined for use at the CERN SPS.
3. Neil Downie (Imperial College, London) was testing scintillators for the TASSO experiment.
4. A team from Bristol University and the Rutherford Laboratory were investigating a novel Cerenkov counter design.



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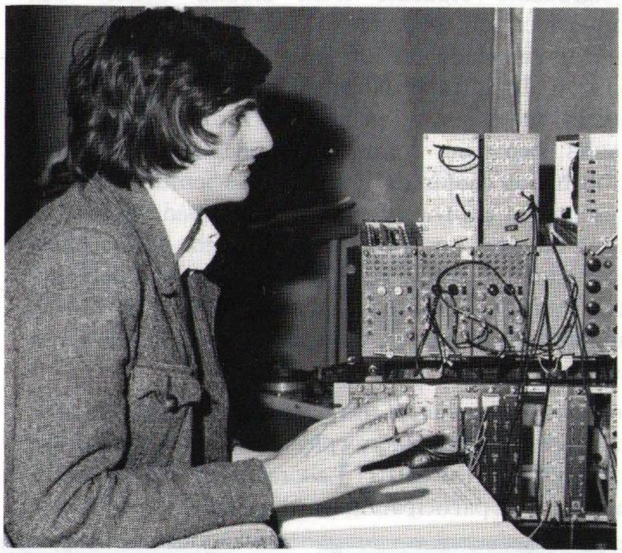

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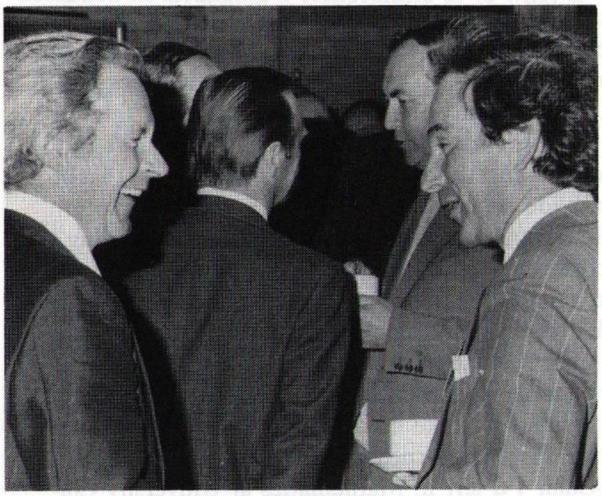
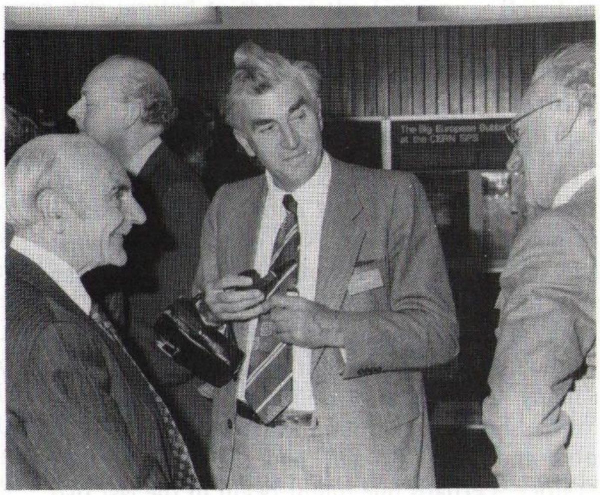
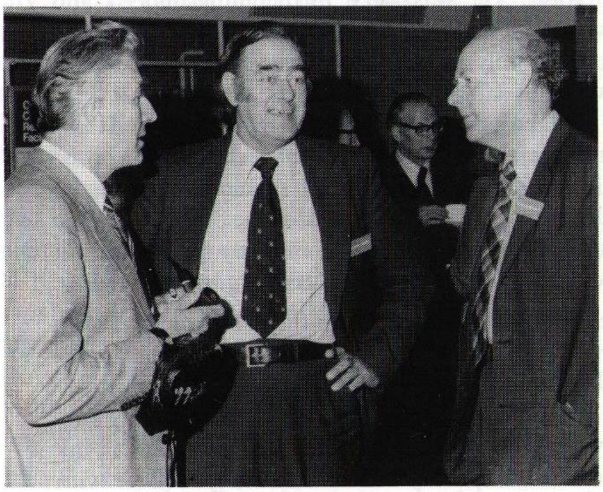
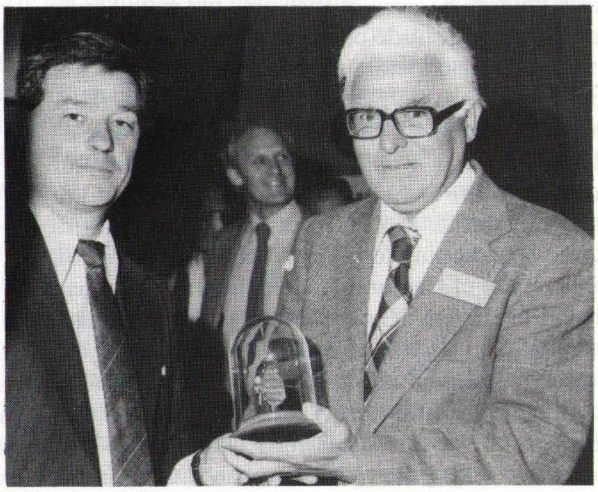
Nimrod Commemoration Evening

A formal commemoration took place on 27 June 1978 which included the presentation of several talks, reproduced in this volume. This page contains photographs of some of the distinguished guests attending the function. The bottom right view shows Dr Leo Hobbis presenting Dr Gerry Pickavance with the master key used to control the Nimrod beam throughout its long and useful life.









INTRODUCTORY REMARKS

by Sir Harrie Massey FRS

Well, ladies and gentlemen, I'm not quite sure exactly what I feel like, being here today; perhaps many of you may almost be surprised that I am not a ghost and to realise how substantial I am. Now on thinking of what I ought to say and how I should behave today, I can promise the speakers that I won't trespass unduly on what they are proposing to say. In order to avoid doing that, I thought perhaps I might go a little further back than many are capable of doing and give you some idea of the sort of background which led ultimately to Nimrod and NIRNS, and hence the Rutherford Laboratory. Perhaps one might begin in the last two years of the 1940-45 war when there was a British party in North America concerned with various aspects of nuclear fission. By that time it was quite apparent that there was a considerable future involved in these activities and that one should begin to think about what to do in Britain in these matters after the war. Chadwick, who was in charge of all the British activities in North America at the time, called two meetings to discuss just this question. Besides Chadwick himself, these meetings were attended by Cockcroft, Oliphant, Skinner, Peierls and myself.

We were entirely agreed that the work in nuclear physics after the war would be on such a scale as to require collaboration and we felt that this involved the establishment of a central institute which would provide facilities for users — and we were thinking particularly of university users at the time. The only confusion in our minds was a very considerable one. We hadn't distinguished between pure and applied nuclear physics because at that time we were hardly aware of the magnitude of the applied side. The first meeting was held before the first atom bomb was dropped, or even the first one tested. On one thing we were agreed (with one exception): there was no doubt at all who would have to be the director of this institute when it was established — we wanted Cockcroft. He said nothing because he, in fact, had been hoping for a very comfortable career in the University of Cambridge after the war. If we had asked him, with his sense of duty he would not have been able to refuse; that of course is what actually did ultimately happen. But it is interesting to note that in the confusion we didn't really distinguish between an establishment like the Rutherford Laboratory which is concerned with pure science and an establishment which was concerned with the applications to industry and to nuclear fission.

After the war things didn't go quite along the lines we had imagined. Cockcroft was the only one who more or less followed the line which we had thought he must; he became Director of the Atomic Energy Establishment at Harwell rather than an institute for research in pure nuclear physics, and he was joined there by Skinner. As far as the others were concerned, and the possibility of university collaboration, this was put in the background somewhat by the fact that Chadwick succeeded in persuading the powers that be to provide enough funds to build a synchrocyclotron for Liverpool. Oliphant at Birmingham was building a machine there and this again broke precedence, it was even a major breakthrough, because he applied to the DSIR for a sum of money greater than anything that had ever been applied for before — £35,000! I know this because I had the job of assessing the feasibility of the design — I take no further responsibility in that matter. Peierls returned to theoretical physics and I to the Chair I'd left in Mathematics at University College. Although I was able to introduce quite a number of features in experimental physics into the Mathematics Department, it didn't run so far as collaboration in expensive nuclear physics. So, for one reason or another, most of the people who were concerned in the meetings in the USA became busy in other ways, and nothing very much happened in the way of collaboration in pure nuclear physics.

However, following through entirely my own experience in these events, in 1949 I had the opportunity to change from Mathematics to Physics at University College. The Physics Department virtually didn't exist in space at that time. It had no room for anything much and so any effective research work had to be done mainly elsewhere. Immediately I thought of the possibility of collaboration with Harwell and, when the possibility arose of shifting to Physics, the first thing I did was to visit Cockcroft and see whether he was still of a mind to build up university collaboration. He was very forthcoming indeed and provided us with a 30 MeV synchrotron which, although it only worked effectively for about a day, provided useful experience. I made my contribution to the collaboration by extracting from him Harry Tomlinson to become head of the technical side of the work at University College. I remember writing to Cockcroft saying how sorry that at the beginning of the collaboration I should steal somebody from him; but he replied gallantly that that was what he expected.

The collaboration began with the use of the cyclotron at Harwell, which was the biggest cyclotron in the world which couldn't produce mesons. The problem was to decide what kind of experiments we would carry out. I can remember a climactic meeting in my room in a temporary hut at University College with

Pickavance and Cassels to discuss these experiments. We made the suggestion first as to what experiment we wanted to do, and of course it was the same one that they intended to do. It says a great deal for their goodwill that we didn't immediately break up. We succeeded in arranging a compromise as to what experiments could be carried out involving the use of a high pressure cloud chamber which we had available and which could be introduced into the scheme.

That was the beginning of a very fruitful collaboration which went on and on, and already it was apparent that Gerry Pickavance was just the man to deal with when it came to these sorts of activities. From that time onwards there was an increase in the comings and goings between the University of London and Harwell, because it was not long afterwards that Devons was appointed to a Chair at Imperial College where they did not have any major facilities either. We then found ourselves discussing the possibility of the collaborative use of a proton linear accelerator, originally supposed to be of 600 MeV.

This kept things going until Cockcroft began to call his famous accelerator meetings where one discussed whether one was more likely to get more Nobel Prizes from the use of a high *intensity* machine, or a high *energy* machine. In fact these meetings were the ones which were the forerunners of the choice of Nimrod. Clearly Cockcroft must have been thinking about the possibilities of somehow or other restoring the original idea we had in Washington for a collaboration in the design and use of an accelerator. He was quite interested to hear of the other kind of collaboration which we were just toying with via the Rocket Panel in the United States. He asked for quite a lot of detail on the way that the Panel actually worked and he was thinking then of how one could devise a suitable collaborative scheme.

Many of you here who are used to collaborating with ten or fifteen university and laboratory teams probably find it hard to believe that, at the time, many university physics departments felt it was not possible to take part in a collaboration of this kind. They were sceptical indeed and it was against that kind of background that Cockcroft went forward with the idea of NIRNS. As soon as this became a reasonable possibility, we were naturally very excited in the universities about who would be the Director of NIRNS. At that time of course it was only Cockcroft among the high level management in AEA who really thought of NIRNS as something which should be fostered for its own sake. It was because of this attitude that we were fortunate enough to have appointed, as the first Director, Gerry Pickavance. He was a key man within the Authority's organisation on the accelerator design and development. Cockcroft was prepared to put the universities and the community-at-large as the prime users and perform this vital service for us (one of the very many things for which we have been very grateful to Cockcroft).

The first meeting of the Governing Board of NIRNS was held in 1957. I can well remember this meeting. Since my only experience of a Governing Board at the time was limited to that of the Governing Board of Rugby School, where its main activity was to discuss and describe the situation regarding injuries acquired in the pursuit of rugby football and hardly anything else, I must say that I had some fears of this particular meeting. I also remember one or two other features of that meeting. One was a purely personal thing which sticks in my memory very much because I cut myself while shaving that morning rather more severely than usual and I was very worried about appearing at the meeting in such a mutilated form. I remember when we went to the meeting we were welcomed at the door by a very boyish-looking man indeed, hardly believing that this was Lord Bridges — feared by the rest of the Treasury (in fact they said it was wholly unfair to have appointed him Chairman of the Governing Board, because they were in fact a little frightened of him themselves). This was a tremendous initial advantage possessed by NIRNS.

The background at that time was one which again was different to the present day in very many respects. There were a lot of doubts, suspicions and uncertainties. Many of the universities felt that the Atomic Energy Authority was about to take over nuclear physics, and that they wouldn't have a chance. Believe it or not, when Cockcroft offered the cylotron to become part of the property of the Board it was turned down because it was felt by some members of the university community to be a machiavellian device of the AEA to infiltrate university nuclear physics. I was in the most ridiculous position of being a representative of the universities on the Board, and had to put their case!

We were also in a complicated position on the Board in relation to the DSIR and a method had to be worked out for defining the demarcation lines with both the University Grants Committee and the DSIR. I remember negotiating on behalf of the Board with the Chairman of the UGC and the Secretary of DSIR. The one thing they just would not tolerate was the idea that the Board would pay for any Research Assistant appointments of an academic character; they reserved that entirely for themselves — everything else could be done by NIRNS, but not that.

These suspicions were gradually overcome and here again an enormous amount of the credit goes to the first Director, Gerry Pickavance, for the co-operation which built up so that before long it was hard to believe that the suspicions had ever existed. Now we are in the position of regarding the Laboratory as one which is a model for large-scale collaboration between external users and in-house teams. The resident personnel must be of high quality, must be able to carry out their own research but are willing to do so very largely in collaboration with the outside users; and this is a great feature of it. You may say that I have mentioned Nimrod hardly at all, but in fact the whole idea of NIRNS was to provide a large research facility for the nuclear physics community and Nimrod was the core of it. Any success Nimrod has had, so far as producing science is concerned, is amplified by the success it has had in bringing the community together. There is no doubt at all that the high energy physics community is the envy of all science communities; it's the most closely knit, and one which really can talk with one voice in a way that many others do not. There is no doubt that a very great deal of credit is due to the existence of the Laboratory and its Nimrod activities.

It was quite a sad occasion when NIRNS had to be wound up in 1965 following the decision to transfer this activity together with other large activities, including space research, to the newly created Science Research Council. There was essentially a "wake" of NIRNS in the form of an excellent lunch at the House of Lords given by Viscount Bridges and I think that many of us felt rather sorry that it was now concluded. However, the work of the Institute is still going on under the SRC, and has acquired so much momentum and the Laboratory is so effective that there is no doubt at all it will have a long and fruitful future.

Sir John Cockcroft cuts the first turf at the Laboratory.

EVENTS SURROUNDING THE CONSTRUCTION OF NIMROD

by Sir Denys Wilkinson FRS

Three or four hundred yards away from here, sometime in the late 1940s, a remark was passed that, for complicated reasons that I shall now tell you about, led to the epoch of high energy physics in the United Kingdom that terminated in the switching off of Nimrod just three weeks ago. The remark was passed by Jim Tuck who came back to this country from Los Alamos for a short time after the war and was associated with discussions that led to the construction of the AERE synchro-cyclotron which operated at 160 MeV in 1949. Jim Tuck said that particles going round in synchro-cyclotron orbits had radial oscillations and if only you could induce those radial oscillations to become larger, maybe the protons could be made to pass into a magnetic channel and be extracted very copiously from the machine. A few years later, in 1950, Tuck and Teng, in Chicago, actually tried this out: it didn't work; they got vertical instability and no protons emerged. However, in 1951 at the Liverpool cyclotron, Herbert Skinner asked Ken Lecouteur if he would have a look at this to see what had gone wrong in Chicago. Over the years the problem was solved and in 1955 Ken Lecouteur and Albert Crewe achieved very high efficiency of beam extraction from the Liverpool cyclotron. I'll stop at that point, which is momentous for the origins of Nimrod, although why it is so is by no means obvious, and will return to it later on as a critical point of my story.

Now what I will do is to tell you about some of the events leading up to the foundation of the National Institute for Research in Nuclear Science in 1957 — I will look at the situation from the same point of view as Sir Harrie, namely that of the alternative attitudes to physics in the UK, particularly expensive physics, obtaining in the 1950s in the universities on the one hand and in the Atomic Energy Authority on the other.

I must remind you that, during the time in question, CERN was getting going. There were two meetings of UNESCO delegates, following the approval of a general scheme for tackling the problem by UNESCO's General Conference in July 1951, the first in Paris in December 1951 and the second in Geneva in February 1952. The "Geneva Agreement", following these two meetings, set up a "Council of Representatives of European States for planning an international Laboratory and organizing other forms of co-operation in Nuclear Research", later called CERN for short, and was signed on February 15th 1952, coming into force on May 2nd 1952. By October 1952 the following countries had formally joined CERN: Denmark, France, German Federal Republic, Netherlands, Sweden, Switzerland and Yugoslavia. In addition the following countries had signed subject to ratification: Belgium, Greece, Italy, Norway. You will notice that the UK is not in either list although a strong British delegation had taken part in the early formative discussions in Paris and Geneva and had offered the Liverpool cyclotron, then under construction, to CERN as an interim facility until its own could get under way. By the end of 1952 the UK had also made a contribution of 120,900 Swiss francs, being more than a tenth of the total monies received by CERN to that date. I have gone into all this in some detail to show that, although the UK appears to have dragged its feet at the beginning of CERN, it was nevertheless very much involved. Many of us felt very strongly that the UK should have made a full commitment to CERN at the very beginning — I well remember Jim Cassels' passionate advocacy for example — and I will not go into the reasons why it did not. There are as many versions as there are memories, but the reasons for our remaining in the wings are certainly not unconnected with the attitudes and forces that led to NIRNS and Nimrod, which is what I am chiefly telling you about today. In the event, the UK was a party to the Final Convention of CERN when it was signed on July 1st 1953.

Today CERN is, of course, absolutely central to our thinking about high energy physics in this country. There are other places to which we also look, SLAC, Fermilab, DESY, but CERN is still the centre-piece of our high energy work. And so in leading you towards the establishment of the National Institute in 1957 I'ld like you to bear in mind that this country had already joined CERN some years before with the objective of CERN being the top of the pyramid where our high energy physics would be done. We had, in 1952 and 1953, told ourselves and told our government that CERN would revitalize physics in the UK and in Europe, reverse the brain-drain, and that this was the only way.

In the years immediately following our accession to CERN to "save Europe" and, presumably, ourselves several accelerators in the meson-producing class came into operation in the UK: the 400 MeV proton synchro-cyclotron in Liverpool which operated in 1954; the 1 GeV protron synchrotron in Birmingham which operated in 1953; the 340 MeV electron synchrotron in Glasgow which operated in 1954. Expenditure in UK universities on these and other, more modest, nuclear machines although small by the standards of true high energy accelerators such as the Brookhaven 3 GeV Cosmotron which operated at that energy in 1954, the 6.3 GeV Berkeley Bevatron that also operated in 1954 and, particularly, the Brookhaven and CERN 25-30 GeV proton synchrotrons then under advanced design (they operated in 1960 and 1959 respectively) was a large fraction of the total resources available for university science in the UK. This situation caused considerable disquiet among university non-nuclear scientists as I can illustrate from a memorandum written in 1954 by Sir Francis Simon to the DSIR Nuclear Physics Sub-Committee referring to an earlier meeting of the NPSC:

"(1) Too much money is being spent on nuclear physics in comparison with other branches of physics. Since the total amount of money available for physics is restricted, every increase in expenditure for nuclear physics means a decrease for the other branches. This is certainly not in the national interest.

"(2) It seems that we dissipate funds and manpower by dispersing the big machines all over the country. Some of the Universities have not the scientific personnel to run even the existing machines at full capacity — not to mention the new ones for which they are asking now.

"(3) In addition to this our technological manpower is insufficient for building up the machines on a reasonable time scale; there is the danger that we will never catch up with the Americans, who organize the setting up of their machines in a much more rational and different manner.

"(4) Disquiet was expressed about the consequences of attaching the big machines to the Chairs of Physics. Once that has happened, the successor of the retiring professor has to be one who fits in with a particular machine and the man chosen will often no longer be the most promising scientist.

"(5) Finally, the point was raised whether we should not follow the American example and create a kind of Brookhaven to remove at least some of the disadvantages of the present position.

"If new machines are desirable — and there seems no doubt about that — then concentration of effort is inevitable. Both economies of funds and manpower point to the creation of a Brookhaven type institution, to which even some of the existing machines may be transferred. During our discussions we might tentatively consider the location for such an institution, in particular the possibility of placing it outside the fence of Harwell.

"It is much easier — at least in the field of nuclear physics — to get money for apparatus or machines than for proper positions for the people who are going to build and run them. The DSIR should reconsider its policy in these matters and provide funds for a sufficient number of senior positions, with a reasonable security of tenure, to staff those projects for which it finances the equipment.

"It is not necessary to go as far as the Americans, but four or five professors for the bigger laboratories would be about the right number. One great advantage of having a number of full professors apart from the head of department would be that one of them could be responsible for the big machines of nuclear physics in the department; the general policy of the laboratory would then no longer be ruled by the machine.

"The increase in professorships would progress much too slowly if pursued by conventional methods. It would probably be necessary for the Treasury to earmark funds for this purpose which the UGC would then distribute to the Universities".

This memorandum is remarkable in several respects, one being that it explicitly considers, for the first time to my knowledge and recollection, a *"Brookhaven-type"* of institution (and Sir Francis was obviously thinking of just the high-energy dimension of Brookhaven because he was only concerned with containing expenditure on *"big machines"*) *"outside the fence of Harwell"*; these two factors almost specify the Rutherford Laboratory of several years later. The other remarkable feature that I wish to stress is that although Sir Francis is exercised to provide for expensive high energy nuclear physics as cheaply as possible he does not even mention CERN and this a scant year after the UK had joined.

It is important to realize that, in these days of the early and middle 1950s, universities were really quite badly pinched for money but there was plenty in the AEA. Nuclear energy looked a rosy prospect and plans were being promulgated, indeed as late as 1957, for the whole of the UK's incremental need for electricity to be supplied by nuclear power. But the AEA had no mandate to go into high energy physics — nor, for that matter, to pursue purely academic nuclear structure physics for its own sake although dark university murmurings of scandals in this respect were heard from time to time. "Getting money out of Harwell" was a great university pastime but although John Cockcroft always had been, and remained, the best friend of university science there was a (rather modest) limit to which AERE's help could run and it certainly did not extend to paying for the building of accelerators in or for universities. The universities could, and wanted to, build accelerators on the grander scale but had no money; AEA had the money but could not build accelerators except those of a defensible project orientation such as AERE's proton synchro-cyclotron with which I opened my story which was justified on the grounds of neutron spectroscopy and cross-section measurements.

But wait! By 1954 Harwell had indeed embarked on the construction of a 600 MeV proton linear accelerator which would have been a superb meson source and academic nuclear physics tool. How had it persuaded itself and the government to do this? The reason was certainly not in order to stimulate the admiration of, and to give help to, the universities.

The operative reason was that there was at that time a report from Berkeley that if you bombarded heavy elements with protons of 600 MeV something like 30 neutrons came out. This, of course, later turned out to be wrong but if it had been true then here was a very realistic rival to reactors for making certain isotopes on a large scale. (Let me remark in passing that even with as few neutrons as in fact do come out under GeV bombardments the conversion of fertile to fissile elements by proton-linac-generated neutrons is a matter for very serious current study.) So there was a valid, or apparently valid, AEA "project" reason for building a high energy proton linear accelerator with a current of a few microamps.

At that time, 1954, the current that could be extracted from synchro-cyclotrons of a few hundred MeV as an external beam was only a few hundredths of a microamp, down by a factor of a hundred or so on what could be achieved with a proton linear accelerator. However, and back to my opening remarks about the success, in 1955, of the extraction of a good fraction of the circulating beam from the Liverpool cyclotron, as soon as cyclotrons could produce as much usable beam as proton linacs the *raison d'être* for the AERE proton linac completely disappeared. (Things are different nowadays, of course, and with the tremendous advances in design linacs are again dominant over cyclotrons but that is beside the 1955 point). Since the AERE proton linac was going to cost £2 million (even in 1955) clearly it could not be sustained and it was stopped. However, the contract for the first 50 MeV had already been let and the firm to which it had been let refused to allow it to be cancelled. Thus a 50 MeV proton linear accelerator, later the Rutherford Laboratory's PLA, was built, and served this Laboratory well for several years.

But in 1955 the AEA was in a pickle because it had committed a lot of effort and money to the proton linac project and something had to be done about it. So people in the AEA began to think and to write extensive memoranda. I must not over-simplify the situation nor invest it with too many Machiavellian overtones but there were people in the AEA, and John Cockcroft was certainly one of them, who really had the interest of collaborative research with the universities at heart and this was their opportunity as we shall see.

Another influential member of AERE at that time, senior beyond his years, was Brian Flowers. He and I talked endlessly in the latter months of 1955 about the ways in which nuclear research in the UK might be organized. In October 1955 Brian wrote an internal AERE memorandum, which was given some small private circulation outside AEA, entitled "National Laboratory of Nuclear Science" which came right out into the open in a far–sighted way that clearly forsaw the establishment of NIRNS but that went well beyond what NIRNS eventually became — although not beyond what some of us hoped for. I will give you some extracts:

"There can be no doubt that physics is becoming phenomenally expensive. It is going to remain expensive as long as we continue to probe the sub-atomic world, and that presumably means for evermore. Even if we think of the high energy field alone, it is unrealistic to consider for this country an accelerator programme consisting of only one or two bevatron machines. It is essential for us, as a matter of national policy, to be prepared to start work on a new machine every few years. The universities of this country are not equipped to spend money at this rate (about £3 million per annum) and it is arguable whether they should become so even should they so wish. Moreover the universities are not able to supply permanent full-time staff at a high level to run such projects. In any case it is clear that revolutionary changes have to take place in the scientific life of this country if we are to maintain our position in international science.

"It is essential to realise that if a high energy centre is set up, it cannot exist in a vacuum. Even merely to support and fertilize the bevatrons one must have nuclear physics, machine design, theoretical physics, electronics, and all the paraphernalia of engineering. But such a centre would soon be sterile if left to itself. The centre must be either next door to a strong university science department (and in that case preferably integrated with it), or it must be part of a large technological, or at least more general, establishment.

"Let us therefore consider a single centre for the country as a whole and try to imagine what it would be like when fully equipped. First of all, it would contain all the bevatron machines plus their design groups and maintenance groups as well as the users of the machines and those who supply their facilities. Secondly, it would have perhaps two high flux thermal reactors, and possibly a small fast reactor. The first thermal reactor would be equipped with choppers and all the other devices which make use of neutron beams; the other would be amply supplied with metallurgical and other irradiation facilities. These reactors would also need all the usual engineering support as well as users.

"To make full use of these facilities there would have to be as many research groups as possible: metallurgy, chemistry, solid state, engineering, medical, physics, theoretical physics. There would have to be full-scale service groups, engineering, electronics, drawing offices, workshops and a computing centre. Although one would hope that many of the scientific employees would be in fact university staff, temporarily employed by some appropriate means, there would clearly have to be a very much larger permanent staff ultimately running into several thousands.

"The strange thing is that this dream-world closely resembles Harwell, with its security fence largely demolished and without some of its most technological groups".

He then argues for the removal of the more technological work to the AEA Second Site, then being planned and that became Winfrith Heath, with the gradual conversion of Harwell to a "Brookhaven":

"It goes almost without saying that this conception of the future role of Harwell could not be put into effect through a single act. It would probably take several years to effect the change; and indeed it is better so since the accelerators have to be built and the Second Site developed. It is also obvious that wherever the first accelerator is built, there will be the centre. If it is put at Harwell, the possibility remains of gradually absorbing Harwell into the centre; if it is put anywhere else Harwell will gradually cease to exist as a research centre of international importance. We must not get off on a wrong foot.

"Let us therefore suppose that Harwell is to become our national centre in due course, and inquire what it is to do . . . As the transformation gradually takes place, fuller integration with the universities would also come about. In addition to its research work, therefore, there seems to be every reason why it should collaborate fully with universities in teaching duties, and that it should be in the position to grant PhD degrees and technical diplomas.

"The universities have always jealously guarded their right to freedom of action and thought. Although some sacrifice in freedom is inevitable if they are to participate fully in a national laboratory of the sort described here, they must clearly be granted a very large measure of control over the whole organisation. This does not seem to be possible so long as Harwell remains merely one part of the Atomic Energy Authority. It is therefore proposed that Harwell, in due course, should become an organisation outside the AEA, run jointly by the universities, by the AEA, and by the permanent staff of Harwell."

(In the September 1955 draft Brian had not been quite so certain as to the location of the new laboratory and had written *"this more or less limits it to the neighbourhood of Harwell or the wilds of the North".)*

I think that it is quite important to note that Brian Flowers and one or two of us did, at that time, have a rather extended view of what the national laboratory, eventually the Rutherford Laboratory, should be; namely rather more like Brookhaven in the large range and scope of its activities. We had this vision of pulling the more applied work out of Harwell, putting it on the *"second site"*, pulling the AERE fence down and turning the whole area here into what we referred to as *"the finest research laboratory in the world"*. That was a vision, of course, that did not come off; the reason was, at least in part, powerful opposition from within the universities themselves, as you will see, although quite a bit of the sentiment behind the vision motivates the new Rutherford Laboratory as we are now (in 1978) beginning to see it emerge.

Brian's document was an important one for getting the university community to face up to the facts of life which were clearly that only through a central, national laboratory could the universities of the UK have access, in the UK, to the means for significant high-energy physics.

My repetition of "UK" in the last sentence is deliberate and to emphasize the fact that although Brian's memorandum is chiefly directed to the question of the UK's doing of high energy physics the word CERN does not occur in it any more than in Sir Francis Simon's memorandum of the year before, also proposing a national centre for high energy physics.

I have mentioned that Brian Flowers' memorandum had some external circulation which was chiefly to individuals and institutions concerned for the development of the country's nuclear, particularly high energy, physics. It produced some significant reactions of which only a fraction can have chanced to become available to me.

Sam Devons, then at Manchester, got the memorandum early in November and replied on November 8th:

"I am a little surprised to find that anyone from Harwell is writing to justify the choice of Harwell as a site for a British 'Brookhaven'. I recall having some discussion about the site of the PLA a couple of years ago when this was first launched, and was thought of as the beginnings of a National High Energy Laboratory, and at that time the arguments in favour of Harwell were claimed to be so overwhelming that no alternative was very seriously considered, at least for very long. I do not think that Massey and I were convinced at that time that this choice was irrevocable, but we accepted it as reasonable, particularly as at that stage the 'National' Laboratory only involved Harwell, London and possibly Oxford directly. The mere fact that the issue is being discussed again is, I think, most welcome, and all the more so since from my personal point of view Harwell is no longer as convenient as it was.

"Lastly, I would like to point out that all proposals for central research laboratories inevitably involve removal from the University of the influence of physicists and research in physics. This is surely to be regretted even if it is a necessary price one has to pay in order to do research at all. Both the Universities and physics in this country will lose if the separation between them becomes too great, so that in considering any central research laboratory one of the primary features to be considered is how academic research can be fostered without divorcing it from the academic background."

Two things are noteworthy in Sam Devons' letter: the first is the implication that the 600 MeV AERE proton linac had been represented to the universities as a tool, at least in part, for their research. This, I am sure, is John Cockcroft, partly from his personal conviction that a national laboratory was the right way to go for the UK's high energy physics but equally to enlist the aid of the universities, or at least neutralize their possible opposition, in his winning of a powerful accelerator from the government for AERE. The second noteworthy point is the explicit recognition that a national laboratory, even though it may be established for, and even partly managed by, universities, must inevitably entail a certain loss of university autonomy. This reservation, expressed by Devons in an uncharacteristically moderate manner, was later to be echoed and substantially amplified by a number of senior university physicists to whom the management of the great war-time laboratories remained a sharp and threatening memory.

To continue one of the main burdens in my attempt to recapture some of the background atmosphere of the UK's high energy thinking in the middle 1950s: Sam Devons' letter does not mention CERN.

Nevill Mott, writing from Cambridge on January 5th 1956 in a piece entitled "Note on the organisation of the proposed national centre for research in high energy nuclear physics":

"As regards permanent staff on the pay roll of the Atomic Energy Authority, scientific officers will certainly be required to develop and maintain the machine. I think that the committee should discuss whether, apart from visitors from universities and research fellows, nuclear physicists are required on the permanent staff. The experience of Brookhaven should be valuable here. If the centre is near enough to the Atomic Energy Research Establishment, it is possible that the nuclear physicists there could play the same role as the permanent staff at Brookhaven."

The title of Mott's note is significant in showing the way in which the idea of a national centre was becoming set in people's minds. The last sentence from the abstract shows that the choice of a Harwell site for the national laboratory was certainly not, at that time, regarded as inevitable. Need I add that Mott's note does not mention CERN?

In all the documents that remain with me from this epoch the first even to mention CERN, and that only casually and in passing, is my own "Some Notes on the National High-Energy Centre" written from Cambridge and dated January 9th 1956:

"Also the modes of experimentation immediately best-suited to University work at a distance — the visual techniques — are rather fitful in their demands on machine time. Because of this and because of the changing popularity of any type of research with time and because of the alternative availability of CERN the joint University demand for machine time is liable to very wide statistical fluctuation."

This note was concerned largely with organizational matters and contains the first quantitative argument for the establishment of strong resident groups in the Centre, standing in their time-allocation on the accelerator in the ratio 1:2 in relation to university groups. (The idea of mixed groups and the resident/university symbiosis, commonplace today, had not yet taken hold).

I was also concerned about tenure for resident research staff:

"But it is not good enough to say that the Centre will take on all resident research staff on a basis of, say, 3-5 year contracts, because then the really good man seeking an assured position will leave at the age when he could begin to be of most use. The solution is that resident group leaders may have 'permanent' posts at some parent centre of applied work such as AEA or the Universities to which they have complete freedom to return when inspiration fails or they need a change but that while they are with it their responsibilities are solely towards the Centre. It is to be understood from the beginning that such movement is quite normal and that it involves no loss of face. This insures against hardening of the Centre's arteries. Ordinary members of the resident groups would have short-term contracts or fellowships as a rule."

My paper also contains the first detailed suggestion that the national laboratory should contain strong resident theoretical groups:

"Theoretical groups. The close association of experimental and theoretical workers has proved to be of great value to both parties. It is particularly true that the experimenter benefits considerably by fairly continuous exposure to theoretical notions and viewpoints even though he does not himself go into them in great detail. This only happens when the experimenter and the theoretician are close together and very immediately available one to the other. It is therefore felt that the Centre should contain provision for resident theoretical groups as well as for resident experimental groups and that the theoreticians should be accommodated in the same building as the machine and resident experimental groups."

In early 1956 Oxford was considering expanding work in nuclear physics and I was in discussion with them about the possibility of my going there (which I eventually did in September 1957). However, despite the active talk of a national high energy laboratory with a possible siting near Harwell, Oxford had made no commitment to high energy physics and did not see my possible going there as constituting or implying such a commitment. These points, together with the openness of the siting question, are both illustrated in a letter to me dated July 2nd 1956 from Sir Francis Simon, then head of the Clarendon Laboratory, which contains:

"..... depend on the siting of the new National Laboratory. There is a certain chance that it may be built somewhere between Oxford and Harwell, but these matters are still under discussion ..... I would certainly not stand in your way if you wish to go into high energy physics ....."

The other notable, or perhaps, by now, not so notable, point is that Sir Francis' letter to me does not mention CERN.

Maurice Pryce wrote, from Bristol on July 2nd 1956, a note entitled "A National Institute for Nuclear Research" supporting the idea of an institute for big accelerators and high flux reactors but expressing what were becoming familiar university reservations although firmly rejecting them in the greater good:

"In so far as such an Institute will exert a strong attraction on the best creative physicists from the universities, it will constitute a threat to the universities. This must be realized at the outset, and faced. The proper development of physics in Britain must not be allowed to be frustrated because the universities are unwilling to adapt themselves to changing circumstances. The universities have to accept a curtailment in the part they play in fundamental research, ***in a limited field,*** *and make the best of it. They should not try to resist the change. To do so would prove harmful to British science, and in consequence to the universities themselves."*

Indeed, in his logical acceptance of the national laboratory as, in effect, an integral extension of the university system, Maurice goes so far as to suggest, as had also been envisaged as a possibility in Brian Flowers' note, that the Institute might actually award research degrees:

"Supervision of the student's research would be, in the kind of Institute I envisage, fully as good as, and probably better than, in many university departments. The standards could and should be maintained as high as at the best universities. Already at the present moment the quality of most of the pure research going on at AERE Harwell is very high and compares favourably with university research. There is there a sense of solid achievement and self-confidence which breeds good research and good ideas. In an Institute devoted more explicitly to fundamental research, this would naturally be even more pronounced. I therefore believe that academic standards would be fully maintained. I am aware that the proposal to let a non-university Institute award a research degree is unorthodox. But the situation calls for unorthodox solutions."

Maurice Pryce's four page memorandum contains no mention of CERN.

During the latter half of 1956, corridors hummed and memoranda flew, culminating in a rather formal and formidable eleven page document from Sam Devons dated November 3rd 1956 entitled "National Institute for Advanced Nuclear Research" and written firmly from the point of view of the universities and their anxieties:

"During the past century or so academic research in physics, that is research not primarily stimulated by conscious or foreseeable application, has been based mainly on the universities of this country. A new policy is being propounded which advocates departure from this practice. A major branch of physics, that is concerned with advances in nuclear and high-energy physics, is to be transferred to an environment much less intimately related to university activity, to a newly created national centre devoted to 'advanced' nuclear physics. The propounders and supporters of this policy range from those who regard its acceptance as an unfortunate necessity to those who welcome it as an independent and wise development. This note discusses both the necessity and desirability of the proposed policy."

Sam continues with a major critique of the 2-tier sheep/goat system as he saw it, contrasting the gold-plated AEA with the shoe-string universities. However, he accepts, albeit reluctantly, the idea of a national institute:

"There should be a national commission ('committee', 'authority') for encouraging, supporting and co-ordinating the whole of 'pure' nuclear physics research, covering both the 'big' and 'not-so-big'. It should be in a position to survey all the work, progress and plans for development (whether in the universities, AEA, CERN, etc) that are concerned with 'academic' nuclear physics and which draw on the same national resources and dip into the common purse. Its primary function should be to ensure that the ***best and most economical*** *use is made of the total resources, manpower, equipment, etc. The scientific initiative for research should stem from the scientific institutions themselves, either individually or collectively.*

"Such a commission should be responsible for providing full financial support for the large university nuclear physics laboratories and should have sufficient funds to ensure that it does not encourage research activity which it cannot adequately foster. It should be responsible for large nuclear-physics projects treated ***as a whole,*** *ie including capital equipment, special buildings, maintenance and operation."*

Note, at this point, Sam's purely passing reference to CERN; but later in the document he returns to CERN in an explicit recognition of the point that I have been inferring all along from the sheer lack of reference to CERN in the documents of the day from which I have been quoting. Sam is writing of the usual arguments for the centralization of large experimental facilities on the grounds of cost:

"All these factors are indeed present, but I consider grossly overrated (similar arguments were advanced not many years ago in support of centralising ***European*** *research in CERN, arguments which now appear to be relegated to the limbo as far as this country is concerned)."*

Devons' document led to a meeting of, chiefly, university physicists in Manchester on December 11th 1956. Sam Devons produced notes following the meeting entitled "Research in Nuclear Physics in the United Kingdom". It is interesting to note the attendance:

"The meeting was attended by:

Professor W E Burcham (Birmingham)
Dr E H S Burhop (University College, London)
Dr C C Butler (Imperial College, London)

Professor J M Cassels (Liverpool)
Professor S Devons (Manchester)
Dr R J Eden (Manchester)
Professor O R Frisch (Cambridge)
Dr B D Hyams (Manchester)
Dr A L Hodson (Leeds)
Professor P B Moon (Birmingham)
Dr T G Pickavance (AERE, Harwell)
Professor L Rosenfeld (Manchester)
Professor G D Rochester (Durham)
Professor H W B Skinner (Liverpool)
Dr D H Wilkinson (Cambridge)

Invitations were also sent to Bristol (Professor C F Powell, Professor M H L Pryce), Glasgow (Professor P I Dee, Professor J C Gunn), and Edinburgh (Professor N Feather, Professor N Kemmer). Some written statements of views have been received."

Why Brian Flowers, who had shown himself so concerned in the matter, was not invited I cannot say; perhaps he was thought to represent The Enemy; if so, all the more honour to Gerry Pickavance who apparently did not come under that stricture and who, indeed, in his subsequent directorship of the Rutherford Laboratory showed himself to be the best friend the universities could possibly have had and who did everything imaginable to make the universities feel that NIRNS and the Rutherford Laboratory were **their** show.

The notes of the meeting begin with the by-now ritual acceptance:

"General agreement was expressed on the need for the creation of a National Institute for the construction and operation of a high-energy accelerator of energy 7 GeV. The probable need for a second accelerator and possibility of more than one site for the National Institute were also noted."

Reference to 7 GeV reflects the thinking and design work that had been going on in AERE, with some consultation with universities, following the cancellation of the big proton linac project.

But the familiar anxieties were also expressed:

"It was felt that it should be made clear who would be responsible for major policy on future developments of the Institute beyond its initial programme. It was thought that any major developments should be considered in relation to further developments within the Universities.

"It was felt most strongly that the creation of the National Institute would not eliminate the need to initiate new developments in the universities of a size comparable with those now there, apart from the need to continue to provide support for the medium-sized projects already in existence."

It is important to realize that much of the universities' worries at this time were due to their fear that if AEA "took over" the universities' high energy physics on the grounds that only it could construct vast machines and operate the national laboratory it would also begin to control nuclear physics **inside** the individual universities. This would inhibit the construction, inside universities, of accelerators of a scale appropriate to individual universities either by direct influence on DSIR or, more subtly, by providing small (university scale) as well as big machines inside the central national laboratory—then arguing that universities did not need their own accelerators because such machines were already available centrally. These fears became dramatically explicit immediately after the founding of NIRNS as I shall shortly relate. At the same time the universities had to recognize that all nuclear physics had somehow to be drawn together:

"With the establishment of the new National Institute there will be four distinct organisations sponsoring nuclear research (Universities and DSIR, AEA, CERN, and the National Institute). It was felt that some co-ordination of policies and resources would therefore be most desirable."

The meeting also recorded some murmurs along the familiar gold plate versus shoe-string line:

"Several of those present at the discussion felt that at some future date some simplifications in the machinery for financing large-scale nuclear research might be possible. They felt that in this way material support might be given more equally to projects requiring comparable resources which were developed in different types of institution.

It was felt that some decisive action would be needed to reduce the discrepancy in salaries between University and AEA employees, particularly since they would be working side by side in the National Institute."

This meeting at least showed itself conscious of CERN, but there was no attempt to have any discussion of the right balance of the UK's high energy programme as between the new national institute and CERN, the only detailed reference to CERN being:

"It was noted that a number of the above points were equally relevant to British relations with CERN. In particular (i) CERN staffing arrangements should allow for the maximum possible proportion of 'visitors', and (ii) there should be facilities for 'truck teams' to use CERN, as it was probable that much of the contact between British Universities and CERN would be through these teams."

I have now taken the story up to the end of 1956, obviously in a very incomplete and fragmentary form, relying only on memory and records that happened to come my way and that I chanced to keep. As you have seen, the universities were doing their own thinking, hoping and fearing but with essentially no contact with AEA, except some at the technical level, and still less with government. Yet all this time active and detailed planning had been going on inside AERE and AEA, with highest-level governmental consultation, for the creation of this very national institute for high energy physics about which the universities were so concerned and yet, as it was inevitably felt to be, "behind their backs". It was the revelation of the extent of this planning, ostensibly for the universities but without administrative and organizational consultation with them, that, to many, seemed to confirm the worst suspicions.

I do not know when the other, AEA/"governmental", side of the story began in detail but, as will immediately become apparent, things had gone far enough already in February 1956 for a submission to government for a national high energy institute then to be made by AEA, for that submission to receive approval in principle subject to timing and finance and for the governmental instruction to be issued:

"That detailed proposals with estimates of expenditure for the construction of the first accelerator and the auxiliary facilities of the Institute be worked out for submission to Treasury by the end of 1956."

As you can see, the universities had their anxieties: how are we going to do high energy physics? Are we going to be swallowed up by a big institute run by the AEA? The AEA had its own problems, if only arising from the proton linac debacle; but it, or at least some of its most senior members, was also motivated sincerely by the wish to do something in the very best national interest and in the best possible collaboration with universities. However, there was a sort of ambivalence about the matter and it was particularly unfortunate that universities as such were not brought into the organisational discussions and planning during 1955 and 1956, to a greater degree than they were, by the AEA.

The crucial document for the founding of what was to become NIRNS, the National Institute for Research in Nuclear Science, is what, for the few of us in the trade at the time, became known simply as "the paper for ministers" or, more properly, "Proposals for a National Institute for Research in Advanced Nuclear Science". This was the final document prepared in response to the provisional approval given by Treasury Ministers in February 1956 to the AEA submission. It went forward sometime, I judge, in the second half of 1956 although I do not know the exact timing. It is fascinating, reading it twenty-odd years on, to see in what detail a governmental agency, with no remit whatever for the care of universities, was arranging with government for the taking of that care of the universities in a field vital for the intellectual and academic development of the universities. This, you might think, would be cause for high moral indignation as it was for the suspicion that it engendered and of which I have already spoken. That is not, however, my personal view; I believe that the way it was done was the only way in which it could, in practice, have been done and that had the universities as such got involved they would have made a muck of it. That John Cockcroft's benevolent paternalism, knowing best, was the only way is not a popular line to take in these days of open democratic everything but that does not affect the truth.

But to the paper; I will not give it in full but just the critical elements beginning with its first clause which is particularly important in showing how the whole deal was presented to government by AEA on behalf of the unconscious universities:

"Various problems have arisen in the financing of nuclear physics research in the Universities during recent years, resulting in the main from the need for large accelerators, exceeding one or two GeV. These problems arise from two causes: firstly, the great costliness of the machines and their maintenance which is disproportionate to other university expenditure on Physics and expenditure in other departments of the universities; secondly, some of the expenditure — and very large sums are involved in this — is difficult to predict and therefore this work does not fit into the normal quinquennial university budgeting."

The next clause addresses itself to scale and cost estimates in a way that, even now, brings a blush to the cheek; it is a superb example of Cockcroft's technique of "getting it off the ground":

"Preliminary estimates show that the cost of the next generation of big machines is likely to be considerable, and it will probably be necessary during the next 7-10 years to build two large machines of different types together costing of the order of £8 million. Other equipment needed might well bring the total capital cost to £10 million, and it is thought that running expenses might rise to a peak of about £400,000 a year. Given the cost and complexity of the necessary machines and the limited resources available, the needs of the universities for the larger machines can hardly be met in the future on the basis of separate provision for each university or even each major university. The setting up of a new research institute for the use of universities and other organizations working in the field would appear to be the best alternative solution. The Institute might in the course of time have more than one laboratory".

The next clause exposes, completely correctly, the background:

"In the United States similar difficulties have arisen and a new Institution has been set up at Brookhaven owned by the Atomic Energy Commission and operated by a committee representative of a number of the universities concerned, where work by these universities involving the use of such large machines can be concentrated.

"Discussions have been taking place between the Atomic Energy Authority, the Department of Scientific and Industrial Research, officers of the University Grants Committee and the Treasury on the possibility of such a development, and proposals were submitted in preliminary form to the Lord President and Treasury Ministers in

February 1956. Treasury Ministers approved the submission in principle subject to timing and examination of the financial arrangements. The Lord President agreed to the examination of this possibility."

I will not dwell upon the differences between the procedures that had given rise to Brookhaven and those that the second paragraph of this clause reveals as having operated in our own case except to note that the nearest that the reported discussions had got to the universities was to the officers of the University Grants Committee and not even to the Committee itself even though, had they done so, that Committee, impenetrably Byzantine even then, would have had no mechanism for consulting the primarily-interested parties. This is the critical clause if you want to understand the background to the universities' suspicions and apprehensions. Another delightful piece of UK Byzantinism was how the National Institute should find its Governing Board, particularly the university members:

"It is proposed that the management of the National Institute should be vested in a Governing Board appointed by the Lord President in consultation with the Chancellor of the Exchequer. The Governing Board would be responsible for the policy of the Institute. It would have a Chairman nominated by the Lord President and the Chancellor of the Exchequer and would include seven representatives of the universities, one of the Royal Society, three of the Atomic Energy Authority, two of the Department of Scientific and Industrial Research and two of the University Grants Committee, a total of a Chairman and 15 members .... It is proposed that the names of the seven university representatives would be submitted to the Lord President by the Chancellor of the Exchequer who would be advised by the Chairman of the University Grants Committee after consultation with the Chairman of the Committee of Vice Chancellors and Principals."

At the very end of the clause the universities have themselves, at last, been contacted, at least in principle, although anybody with any knowledge of the operation of the CVCP would be hard pressed to think up a mechanism that its Chairman might acceptably activate.

The first several clauses largely concern the universities and their needs, but then comes the AEA nitty-gritty (my underlining):

"The staff of the Institute would consist of the following categories:—
(a) Operating and maintenance staff for the accelerators. (Permanent Atomic Energy Authority staff).
(b) Administrative and Industrial staff (Permanent Atomic Energy Authority staff).
(c) Atomic Energy Authority staff of experimental physicists (about 20 per machine, on secondment). This staff would be changed from time to time.
(d) Visiting and attached university staff (about 30 to 40 per machine).
(e) Research Staff of the Atomic Energy Authority and Industrial organisations doing long-range fundamental work on attachment."

It would be improper of me to enlarge upon the domestic politics of the AEA in the mid-1950s but it is clear that the envisaged AEA/Institute relationship was seen to be, as it were, symbiotic.

Although the unfortunate lack of consultation with the universities confirmed certain suspicions there was another feature of the document that allayed, to some degree at least, certain fears, particularly that of a full takeover bid by AEA for university nuclear physics; the internal business of the universities was to be left explicitly to them:

"The provision of such facilities in the Institute would be supplementary to the existing facilities, now established in the universities. The maintenance and operation of these machines will be met by the universities and the Department of Scientific and Industrial Research under the new scheme for the division of this responsibility between the University Grants Committee and the Department of Scientific and Industrial Research".

The present technical position was spelt out. As I have mentioned in connection with Devons' notes on the Manchester meeting of December 11th 1956, although there had been no organisational contacts with universities, there had been scientific consultations about possible machines and these had led, with substantial reservations being expressed by many university participants, to what was essentially a souped-up Bevatron:

"Design studies have been proceeding for a year on the first large accelerator to be built. There is now general agreement amongst Authority and senior university physicists that this should be a proton-synchrotron designed to produce protons of 6.5 million volts energy with an intensity about 100 times higher than that now available from the Berkeley 6 billion volt proton synchrotron."

Timing and the site were discussed:

"If high energy physics in Britain is not to fall even further behind that in America and Russia, it is extremely urgent to start the construction of the next machine as soon as possible. Buildings could be started at a developed site such as Harwell by April 1957, providing the location is decided upon by November 1956 and financial approval is obtained for the project by January 1957. This timetable should make it possible for the machine to be completed towards the end of 1960.

"A site adjacent to AERE at Harwell could be made available and the Atomic Energy Authority consider that it offers so many advantages, including a considerable saving in time and money, and that it should be used as the location for the first machine. Moreover, changes in the

security arrangements at Harwell will soon make it possible to give freer access for university workers to some of its own facilities such as the proton linear accelerator, the 110 inch cyclotron and the high flux reactor Dido".

In the event, the timing at the beginning of this suggested schedule slipped by a few months but that at the end by a few years: it was not 1960 that saw Nimrod's operation but rather 1963.

Staffing was foreseen as up to 160 during construction, reducing to 120 when research was in progress, " . . . *to design, build, operate and use one machine"* — and this to **include** 50 researchers from universities. As for the cost figures, the staffing estimates must still raise a blush.

The document made explicit the relationship between the new institute with its proposed machine and the defunct proton linac project:

"The machine now proposed for the National Institute is intended to replace the 600 MeV proton linear accelerator which the Atomic Energy Authority had already received approval in principle to build at an approximate cost of £2 million. The first stages of the proton linear accelerator going up to an energy of 50 MeV are now in an advanced stage of manufacture and will, when completed, provide an extremely valuable tool for nuclear research in the lower energy region. However, due to important technical advances in the field of accelerator design, it is considered that the country's interests will best be served by the construction of this 6.5 BeV proton synchrotron for the National Institute rather than the continuation of the Authority's proton linear accelerator project to an energy as high as 600 MeV".

So that was that. My final remark about this submission to ministers, specifically directed as it is to the future of high energy physics in the UK, is that it nowhere mentions CERN. At CERN, at the time of this submission to ministers, final specifications for the 25 GeV proton synchrotron had been drawn up and tenders were being received in respect of most of the major components. The UK was paying a major share in all this; indeed, in 1956, 23.84%. The President of the CERN Council was from the UK, Sir Ben Lockspeiser, and the UK's other delegate in the CERN Council was Sir John Cockcroft, chief architect of the proposed new national institute. The total, utterly astonishing, omission of mention of CERN in the paper for ministers, as from virtually all other relevant documents of the period, I do not interpret but suggest as a suitable subject for study by historians of science (and politics) in due course.

The foundation of the National Institute for Research in Nuclear Science was announced in the House of Commons on February 14th 1957. The Byzantine wheels had already turned and a letter from Peter Thorneycroft, then Chancellor of the Exchequer, dated February 15th invited me, in the name of himself and the Lord President, to become a member of the Governing Board. The letter also brought the news, although at the time none of us knew what good news it was, that the Chairman of the Governing Board would be Lord Bridges. Edward Bridges had recently retired from being head of the Treasury and of the Civil Service — he was the last to combine those appointments in one person. He knew Whitehall upside-down and back-to-front and when he coughed discreetly, mandarins blanched. He had earlier, I was given to understand, won World War II by being Secretary to the War Cabinet and recording in its minutes what Churchill ought to have said — but that is another story. Bridges, if I may dwell on him affectionately for a moment, was a tremendous chairman of the Governing Board of NIRNS. He never permitted a vote and he always brought home the bacon. He occasionally said *"damn"* and once even recorded his sentiments at some temporary frustration that we were experiencing with government in the form of the Minister for Science, Lord Hailsham, by going to the blackboard in the room in the Daresbury Laboratory (this was many years on) where the General Purposes Committee of NIRNS was meeting and writing on it *"Hell and blast"*. He believed in direct rule and always stood out against any suggestion of a London office for NIRNS. He was sad when NIRNS was dismantled in 1965 and absorbed into the new Science Research Council; he had apprehensions about the possible bureaucratisation of nuclear science on account of the long chains of command that would follow the setting up of the SRC and he expressed these apprehensions in a speech to the Lords on 23rd February 1965 in which he quoted from Oliver Goldsmith, fearing that the new set-up would be *"Remote, unfriended, melancholy, slow"*. But I am not, today, talking to you about the SRC: that is yet another story.

The announcement of NIRNS was on February 14th 1957 but it was not unexpected; for some months we had known that something of that sort was coming. For example, several weeks ahead of the date, Sir Keith Murray, in his capacity as Chairman of the University Grants Committee had, as part of the Byzantine process, approached me most courteously and diffidently to make sure, because it was not then public, that I was indeed going to move from Cambridge to Oxford at the beginning of the 1957/58 academic year revealing, as he did so, the import of his enquiry.

That things were known to be going to happen is also shown by a paper that Brian Flowers and I wrote jointly, dating it February 13th 1957, a day before the announcement in the House of Commons, entitled "National Institute for Research in Nuclear Science; Some Thoughts as the Curtain Rises". In this paper we still hankered after the idea that the new institute should be not just a high energy laboratory but more like a UK Brookhaven and also, if only implicitly, urged that only through the incorporation of certain valuable AERE facilities within the new institute could they become realistically available to universities:

"It is understood that the Cyclotron, together with the existing Cyclotron Group, may become part of the Institute although the final decision has not yet been made. If the conversion of the cyclotron proves to be feasible this would then be the first source of mesons at the disposal of the Institute; if not it would still be the highest energy machine for miles around and at the very least provide an excellent training ground in which university groups may establish high energy electronic and visual techniques. Much important work in fact remains to be done with this machine, and its inclusion would enable the Institute to get under way in a fine style that will not otherwise be possible. We therefore consider that incorporation of this machine is strongly to be urged.

"One machine, however, is in a special category. This is the Tandem Generator, the building of which was agreed on the understanding that it was to be used jointly by AERE and the universities — most people have in fact assumed that it would be an Institute facility. It is a regrettable but irrefutable fact that collaboration between universities and Harwell has so far failed to materialise on any appreciable scale, partly through lack of enterprise on the part of the universities, but partly through the unwillingness of all but a few teams at Harwell to make their facilities realistically available."

The "Cyclotron" referred to in the first of the above extracts was the AERE 160 MeV synchro-cyclotron, the up-grading of which was then under consideration and that was referred to in the "paper for ministers" as an instrument (there the "110 inch cyclotron") to which better access by universities might come about as a result of NIRNS. I do not here, 20 years on, want to dwell upon the "regrettable but irrefutable facts" of the last sentence of the above extract (except, perhaps, as a *Gegenbeispiel* to remark that I have never encountered more generous help from any member of AERE than I, and many other members of the university community, had from Godfrey Stafford in the years, before he moved to NIRNS, when he was running the AERE synchro-cyclotron). However, as I shall remark in a moment, the AERE synchro-cyclotron, explicitly put on offer to the universities as part of the NIRNS package, was the red rag to the bull and the question of even smaller machines such as the AERE tandem accelerator that Brian and I mentioned in our paper was never seriously raised.

Brian and I were also concerned about the director of the new laboratory in the context of relations with AERE (I should remark that it was being, by this time, almost universally assumed that the site of the NIRNS laboratory would be adjacent to Harwell although this could not yet be public):

"The problem of establishing and maintaining good relations with AERE cannot be dealt with fully except on a personal basis, and this is impossible until it is known who the Director of the Institute is to be. The Director has at the same time to maintain good relations and to bring about that measure of independence that the Universities have the right to demand. At the same time, the Director will have to maintain proper independence from AERE over matters of scientific policy for which he is to be responsible only to the Board. This should be no great additional hardship for a Director who will in any case have to walk the razor edge between various conflicting interests as his normal way of life."

I should remark at this point that the nature of the Director became a matter for serious debate within the NIRNS Governing Board in due course and it was not assumed, at the date of which I now speak, that the obvious choice of the evident "internal" (ie AERE) man, namely Gerry Pickavance, would necessarily be made. It might also be useful to remind you that, when NIRNS was set up, and throughout its existence, it was funded on a sub-head of the AEA's Treasury vote; this was, from time to time, of the very greatest value to us. We went on, emphasising the "AERE connection":

"Clarification is required at a very early stage of the degree of responsibility of the Director to AERE for administrative matters. When the purse strings are held by the Authority, administrative responsibility to AERE **could** *mean complete subservience which would be contrary to the aims of the Institute. With AERE at the present time, such diverse matters as appointments, promotions, permission to distribute documents, authority for travel abroad on business, workshop priorities, and the placing of contracts, are regarded largely as administrative matters. We doubt whether the universities would be content with a system that did not allow the Institute effective control over such matters. The Institute will therefore require a very large measure of control over its own budget, once approved, and a not inconsiderable secretariat to administer it. It will have to be free to place contracts with outside firms and universities on its own authority for minor work which would otherwise be delayed in the Authority workshops."*

Need I say that Brian's and my paper of February 13th 1957 makes no mention of CERN.

The NIRNS Governing Board, as should be expected under Bridge's dynamism, began to meet at once and a Press Release of March 19th 1957 specified the site of the new laboratory at Harwell, much to the surprise of no-one.

In a sense that is as far as I wanted to go but I should like to go on a little bit more for two reasons: one is to comment further, although guardedly, on the way in which events that I have recounted were received by the university community; the other is to show the way in which, quite soon, CERN returned to the forefront of the UK's thinking in high energy matters.

The announcement of the NIRNS Governing Board and the background to its operations had immediate and devastating effects. Herbert Skinner, whose commitment to high energy physics through his own Liverpool laboratory and through his insistent work for CERN (his wife, Erna, used to say of their house in Liverpool, "Terra Nova", "CERN was born here" and Herbert died in Geneva immediately following a meeting at CERN) was very great, was not a member. This hurt him perhaps to the point of its being, as Jim Cassels has opined elsewhere, a fatal blow. Philip Dee was not a member either and it was he, at least to me, who articulated most clearly the apprehensions that the older generation of UK nuclear physicists felt about the new move. Philip Dee, when my appointment was announced, wrote me a letter that I carried about in my pocket until it fell to pieces, warning against the Establishment, quoting from war-time experiences to which I have already darkly alluded and passionately, quite passionately, asserting on the universities' behalf their academic freehold. I was moved by this letter; it was a colleague-to-colleague message in spite of the discrepancy in years, friendly, sincere, compassionate; I replied to it as one respectful and responsible generation to another but heard nothing more from Philip for several months; then he wrote to me to say that he had carried my letter about with him until it had fallen to pieces in his pocket . . .

I did not know Rutherford; he died in 1937 and the first time I saw Cambridge was in 1939 when I just caught sight of JJ peering in a shop window in Kings Parade; but I have often wondered about the spirit of those times and whether we still have access to it today; to continue my sentimental digression, I well remember the meeting of the NIRNS Governing Board when the suggestion came forward that the new laboratory should be called the Rutherford Laboratory. I had not been party to the preceding Byzantine process and, brashly, wondered openly whether Rutherford, the archetypal string-and-sealing-wax man, the man who had tickled the nucleus into revealing its secrets as one might a trout, would wish his name to be associated with the sledge-hammer that Nimrod was designed to be. There was an awkward *mauvais pas* during which Bridges looked uncomfortable and glanced hopefully around the table for help; no problem; Patrick Blackett, with one of his most magisterial frowns, assured the Board that had Rutherford survived this was exactly what he would have been keenest on and the Rutherford Laboratory it immediately became. I am still not quite sure. . . . . But I have digressed as old men do, and, in a young man's game, such I already am at 55.

Philip Dee's essential thrust was: all right; we have to have a national laboratory (need I say that his letter did not mention CERN?) to take care of the UK's *highest* energy needs; not *high* energy needs because the Liverpool, Glasgow and Birmingham machines were still thought of as "high"; and forgetting, as everybody in 1957 did, CERN, "highest" was the new institute; but we must not allow the new institute to take over the universities. Although, implicitly, the founding charter of the new institute had left the universities to take care of their own in-house developments in nuclear physics, it had also offered to the universities the AERE synchro-cyclotron and, more remotely, high flux reactors and so on (and Jack Diamond's membership of the NIRNS Governing Board confirmed the AEA intention in this latter respect). These offerings, including the 50 MeV beginning of the defunct 600 MeV proton linac from which this tale began, were anathema to the old guard because they were clearly seen as the second-order bid by AEA, for takeover of university nuclear physics in the sense that I have earlier described ("If it is available to the universities in/through Harwell/the Institute it doesn't need to be anywhere else"). Dee, and others, passionately opposed the transfer of the AERE synchro-cyclotron and the first 50 MeV's-worth of the PLA to NIRNS for this reason: universities, as such, must be allowed to develop on their appropriate internal scales; there were 400 MeV coming on stream at Liverpool, 1 GeV at Birmingham, 340 MeV at Glasgow; to put 50 MeV and 160 MeV into a national institute would utterly sell the pass.

The outcome was the usual British compromise: the sawn-off 50 MeV of the PLA went to NIRNS but the AERE synchro-cyclotron and the rest, offered or not (and that is another unwritten chapter of AEA politics) stayed behind the fence — although, let me again say how fully the facilities of the AERE synchro-cyclotron were made available to universities, throughout the period of my report here, and indeed beyond it, by Don Fry as head of the division of AERE that housed it at the time of NIRNS' foundation, by Godfrey Stafford before he crossed the AERE/NIRNS divide and by Basil Rose as he stayed behind it: the "Cockcroft doctrine" which said that Harwell had come from the universities and should be for them was always strong although not uniformly espoused by all of John's senior colleagues, still less his *Nachfolger* . . . . But that is yet another story . . . .

But the main thrust of Philip Dee and his generation (for the most part) was that NIRNS should confine itself strictly to those activities that were, because of their scale or because of their nature, inappropriate for universities. In particular the AERE synchro-cyclotron was out. And out it stayed. I pass over a few months of in-fighting and come to September of 1957 when Cockcroft, on the 7th, addressed me in his wonderful tiny handwriting:

"Many thanks for your letter. I too have been very disappointed by the attitude of many of the university physics professors towards the Institute.

"I feel that we can only overcome this by patience and going stepwise. I hope that we can at least agree to transfer the PLA .... at the next meeting.

"After thinking things over it may be best to appoint only the 'Director' of the Bevatron as a starter and leave our further appointment until the PLA has been handed over and the emotion has cooled off a bit."

I do not think that I could possibly indicate more powerfully the stength of university feeling, at least for those of you who knew John Cockcroft or even only knew of him, than by drawing attention to his explicit reference to emotion at the end of his last sentence (my underlining).

So NIRNS was launched, and that is all I wanted to tell you about. But I feel that I should like to provide a post-script to take us forward into an epoch with which most of you will be more familiar than the almost pre-historic strata that I have been exposing to you.

NIRNS was launched in early 1957 but it went very slowly and many of us were worried. Many of us had felt, and continued to feel, that the specification of Nimrod had been excessively conservative; a quotation from a letter from me dated June 2nd 1959, to Gerry Pickavance (by then fully, and by major acclaim, Director of the Rutherford Laboratory) runs:

"Putting it very simply, our machine is relying for its being a decade too late on its superiority in shielding and provision of experimental facilities. To be true, as it stands at the moment, there is some superiority in flux, but as you yourself have always pointed out, there is no advantage that our machine has over the Bevatron in a basic way other than a slightly higher repetition rate. It may well transpire in fact that the Bevatron can make up for this on account of its much larger aperture."

Note that the 6.2 GeV Bevatron in Berkeley had already operated in 1954, five years **before** the date of my letter to Gerry, that Nimrod would not operate (at 7 GeV and at an intensity not then excessively above that of the Berkeley Bevatron) until four years **after** the date of my letter and that, in the meantime, the 30 GeV proton synchrotron in Brookhaven would operate in 1960, 3 years **before** Nimrod and that the CERN proton synchrotron would come into operation at essentially the same energy as BNL even a little **before** the Brookhaven machine. What is essentially a valedictory is no occasion for a critical historical assessment; I can only say *"Mea olim culpa"*. I could write it all up but I do not suppose that I ever shall.

Hopes and planning for major extensions of NIRNS domestic activities continued for some years. This is an extract from the notes of a meeting in early 1960:

"At a meeting of the Physics Committee of the National Institute for Research in Nuclear Science held on 8th January 1960 a Working Party was formed to consider the future accelerator policy in the United Kingdom. The Working Party met in Liverpool on the 28th/29th March 1960 for its Main Meeting. It was attended by the following:

Chairman: Sir John D Cockcroft
Professor J M Cassels
Professor S Devons
Professor B H Flowers
Dr M G N Hine
Professor A W Merrison
Professor P B Moon
Dr R G Moorhouse
Mr L B Mullett
Dr T G Pickavance
Professor R E Peierls
Professor A Salam
Professor D H Wilkinson
Dr W S C Williams
Secretary: Dr G H Stafford."

Recommendations were:

"It is considered that there are very strong grounds for continuing research in the field of elementary particle physics in the United Kingdom and a programme is proposed to cover the next decade. The following recommendations are made:

(a) An electron accelerator with an energy not greater than 4 GeV should be built at a National Institute Laboratory as soon as possible.

(b) Possible methods of producing intense beams of 'strange' particles should be studied with a view to starting the construction of an accelerator for this purpose in or about 1965."

(Suggestions as to (b) were: a proton linac of greater than 5 GeV; a resonanted AGS of greater than 15 GeV; a proton FFAG accelerator).

"(c) The Rutherford Laboratory proton linear accelerator should be extended to produce intense pi meson beams with energies up to a few hundred MeV.

(d) Money and effort should be devoted to making full use of existing high energy accelerators in the United Kingdom, and at CERN.

(e) Research into new methods of accelerating particles and into particle detection systems should be extended."

By this time the CERN PS was already running, and CERN begins to slip more insistently into NIRNS documents. CERN was taken care of in the UK by DSIR which to some degree explains the sparseness of reference to it in NIRNS papers; it does not, however, explain the almost total lack of policy documents before the 1960s, of whatever origin, considering the UK's high energy physics programme as a whole. All this changed in the mid-1960s especially after the DSIR/NIRNS merger with the SRC in 1965.

Not everyone welcomed the idea that there should be a major extension of the domestic UK programme in high energy physics. For example, G P Thomson reacted to the Working Party's report from which I have just quoted in a characteristically vigorous way:

"This draft Report is a most important document. It recommends a scheme which, if accepted higher up, will commit a substantial fraction of the best scientific manpower of the country for at least ten years .... The first thing to be considered is whether the scale is right, and this I venture to doubt.

"Many physicists consider that nuclear physics already receives too large a proportion of the whole effort devoted to physics, and that in addition the big machines take too large a proportion of this. The first round of big machines in this country has not so far made many startling discoveries. They compare very poorly with the outstanding successes gained here with cloud chambers and nuclear emulsions. It is argued that the usefulness of cosmic rays for nuclear research has diminished and that further progress will be made by the big machines. This is true at the moment, but one should remember that if for any reason energies in excess of 10^{11} *volts are needed they will have to come from cosmic rays, inconvenient as this will be. There is a large time-lag with big machines, and prediction is risky.*

"I suggest that this Report should be treated as a statement of what would be desirable for research on fundamental particles if other needs for physicists were disregarded."

How often in the years that followed were these sentiments re-echoed!

My piece has been about NIRNS and Nimrod and has dwelt on the astonishing insulation of thinking about the domestic programme from the UK's participation in CERN. This insulation was not because we were Little Englanders, far from it; it was really a quirk of timing of the two programmes although there was a strong undercurrent of feeling that, despite the need for, and desirability of, international collaboration we must also make certain of being able to go on doing our own thing. But with the operation of the CERN proton synchrotron the situation had to be faced and the UK's high energy policy explicitly discussed as a whole: a NIRNS/DSIR Joint Consultative Panel on Nuclear Research was set up. Cockcroft was Chairman and at a meeting on October 24th 1961 he asked me and Cecil Powell to write pieces on what we saw to be essential developments in CERN. I did a quick short qualitative note that went out on October 30th 1961 entitled "CERN: The Next Step". It contained:

"The 28 GeV proton synchrotron at CERN is a success. It shows that European scientists can carry through a great enterprise fully comparable with the parallel efforts of the Americans. It shows that a multi-national organisation can function without the frictions that the pessimists foresaw. But this is only a beginning. Europe must now make the contribution to high energy physics of which she is capable. This will happen only if full means for exploiting the existing accelerator are provided and if the assurance is given that Europe will now continue to take high energy physics seriously. Her high energy physicists may then look permanently to CERN as their intellectual centre and not have to fear the day when better facilities available elsewhere will again be an irresistible attraction. This assurance can come only through the establishment of a long-range CERN programme for a new accelerator comparable in scale with the 'national' programme now under discussion in the USA. This is a pressing issue; the time-scale for a major accelerator is 6 to 8 years and the American proposals are already being formulated.

"The alternative is to say that high energy physics does not justify the taking of this next step and that CERN will close down at the end of the useful life of the present synchrotron in 10 years or earlier if her physicists withdraw their support. But high energy physics is the deepest challenge that the world of nature throws against man's intelligence. The present machines barely suffice to make us aware of that challenge. More energy and more intensity are needed to give quantitative answers to existing questions; possibilities such as the existence of new symmetries and new hierarchies of particles and interactions can only be probed at much higher energies than we now have.

"The need is an explicit one and will be met in the USA. If it is not equally met in Europe we shall be denied our participation in this fundamental field to whose beginning we contributed much and which we have so recently rejoined; the best of our scientists, their appetites whetted by the present CERN Laboratory, will certainly go where their work can be continued. If CERN is not ultimately to stand for the betrayal of European physics as she now stands for its hope we must take this next step and our willingness to take it must soon be made clear."

In compensation for all the papers about the domestic programme that did not mention CERN this one about CERN did not mention NIRNS. And when Cecil Powell and I issued our big joint paper "The Future Development of High Energy Nuclear Physics", in January 1962, the first of the lengthy series of papers leading up to the UK's eventual backing of the CERN 300 GeV SPS project, that paper did not mention NIRNS either.

The only thing of which, in finishing, I want to remind you is that the little synchro-cyclotron behind the AERE fence that operated in 1949 and that was in at the very beginning of my story has seen NIRNS and Nimrod come and go but still itself runs happily on.

THE NIMROD PROJECT

by Dr. L.C.W. Hobbis

My talk on the Nimrod project is not going to be of parameter lists and organisational charts. Rather, I hope to remind you about the spirit of the project and tell you something of what it meant to be part of it.

The project as we know it began in 1956, receiving Treasury approval in mid-1957. Design and construction were carried out by the UKAEA for NIRNS at a capital cost of some £11 million. The machine reached the design energy of 7 GeV in August 1963, officially started high energy physics six months later and reached the design intensity of 10^{12} protons per pulse in September 1964.

The Nimrod project is a success story which really began with the decision taken early in 1955 to abandon construction of our 600 MeV proton linac, when it was realised that microampere proton currents could more economically be produced by synchrocyclotrons using the new method of extraction which had just been demonstrated on the Liverpool machine. At the same time Gerry Pickavance's accelerator team at Harwell was encouraged to think about a new higher energy machine which could be built quickly in Britain to provide a facility for our universities in addition to the new 25 GeV accelerator under construction at CERN.

Many of you will remember the meeting in the Cockcroft Hall in May 1955 when the possibilities for the UK were discussed against the background of the existing high energy machines, notably the Bevatron and Cosmotron, the alternating gradient projects at CERN and Brookhaven and the prospects offered by the new fixed field alternating gradient (FFAG) designs. You will recall that the designers of the alternating gradient machines were predicting intensities in the range of 10^9 to 10^{10} protons per pulse. The main designs discussed at this meeting were for an alternating gradient machine of energy 12 GeV and a high intensity cyclotron type giving about 2 or 3 GeV at about 1 microamp (ie an intensity about one thousand times higher).

At the May '55 meeting John Adams outlined the difficulties of building a large alternating gradient proton synchrotron and the sort of problem which had to be solved in order to build such a machine in a reasonable time. The Liverpool cyclotron could be compared in complexity to the linac injector for the CERN synchrotron. A large synchrotron was an order of magnitude bigger and more difficult to build compared with the existing UK machines. He discussed the details of various problems including the effects of misalignments, transition energy, non-linear effects and concluded with details on staffing and management.

The meeting did not need to decide anything firmly but Sir John Cockcroft felt the majority favoured a 12 GeV machine. A poem, written (I think) by Rudolf Peierls, seemed to summarise the meeting rather well:

> *A meeting at A.E.R.E.*
> *On how to get some GeV*
> *With no amount of trouble you*
> *Can do this C.W.*
> *C.G. has also had its fling*
> *A Common-Garden kind of thing.*
> *For Awful Gamble stands A.G.*
> *But if it works or not we'll see.*
> *If resonance we can't defy*
> *The diamonds will just dot our tie.*
> *F.F.A.G. will surely stand*
> *For Fancy Frills are Always Grand.*
> *Electrons I would rather leave*
> *They lose more speed than they receive*
> *The Linac would hardly win it.*
> *Its house is fine but nothing in it.*
> *Oh microgauss! Oh millithou!*
> *Oh megaquid (if funds allow):*
> *I get, when I attempt to guess*
> *The C.R. double E.P.S.*

A second meeting of Harwell and university scientists was held in December 1955. Sir John Cockcroft opened the meeting by reading out a summary of comments which had been sent in following the May meeting. The majority of people who wrote felt that the high intensity machine should be given higher priority. Everybody agreed that its energy should be as high as possible. At the meeting, Gerry Pickavance spoke about the progress on the machine designs, Hans Bethe on high energy physics and John Dickson about a recent trip to the USA.

Gerry Pickavance discussed ten possible machines of energy 6 GeV, as outlined in Table I. The Harwell accelerator group had discovered problems with FFAG cyclotrons, but considered that with frequency modulation it would be possible to get at least 1 microamp at 3 to 3.5 GeV. This energy was set essentially by economics. We had also examined the possibilities of several synchrotron-type machines which would give about 6 GeV with intensities considerably higher than the Bevatron, which was then running at 2 × 10^{10} protons per pulse and 10 pulses per minute. A double magnet synchrotron and a spiral ridge synchrotron looked the best bets (ie types 8 and 9 in Table I). We were confident they could yield at least 100 times the Bevatron intensity, possibly 500 times. The inten-

Table I. 6 GeV Machine Designs Described at the December 1955 Meeting

	Machine type		Magnet weight (tons)	Performance relative to the Bevatron (3×10^9 protons per second)
1	Bevatron	2×10^{10} protons per pulse, 10 pulses per minute	10,000	1
2	Bevatron Mk II	Better injector, magnet, higher repetition rate	7,500	100-500
3	Alternating gradient synchrotron			1
4	Kerst Mk I	FFAG type, very difficult to build		50
5	Kerst Mk II			
6	2 GeV synchrocyclotron + 6 GeV synchrotron	Both with spiral ridges difficult and costly to build		1,000
7	Synchrocyclotron + synchrotron	Both with weak focussing		50
8	Double magnet synchrotron	10^{12} protons per pulse, 30 pulses per minute	2,000	100-500
9	Spiral ridge synchrotron with saturable lips		2,000	100-500
10	Spiral ridge cyclotron	Difficult and very costly to build	30,000	1,000

sity increases were to come about through injecting more charge, running at higher repetition rate and reducing losses during acceleration. Both magnet designs (see Fig 1) were aimed at reducing stored energy by having substantial regions which would saturate at high fields. A new unit was suggested — the *Pick* = the existing Bevatron intensity. We were aiming towards a kiloPick!

The lively discussion which followed Gerry Pickavance's talk quickly homed in on the machine types 8 and 9 in Table I, which could produce about 6 GeV at an intensity of 100 to 500 times that of the Bevatron.

The events of 1955 are interesting in that they emphasise the basic objectives of the Nimrod designers. We needed an injection system which would provide at least 5 microcoulombs per pulse. We needed an economical magnet design so that we could afford 6 GeV and so that industry could make a power supply to excite it. We wanted a C-type magnet to facilitate the extraction of beams. All this, including the ability to run at 30 pulses per minute effectively defined the machine, especially the design of the magnet and vacuum vessels. An economical magnet would have as small a vertical aperture as possible and pressure in the vacuum vessel would need to be kept to about 10^{-6} torr. This restricted strongly the vacuum vessel material and the components which could be tolerated inside. It effectively excluded having the

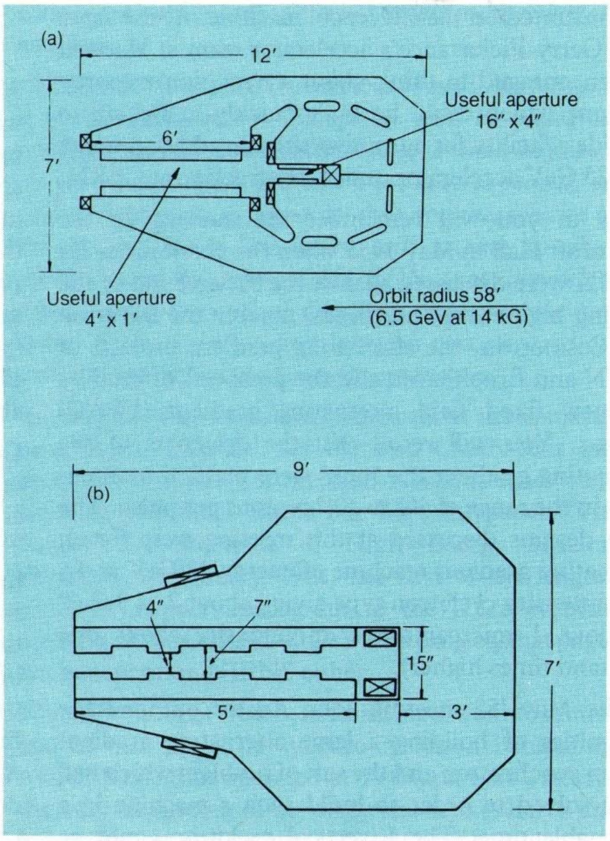


Fig 1. Early magnet designs for a) a double magnet synchrotron and b) a spiral ridge synchrotron presented to the Cockcroft Hall meeting in December 1955. These designs correspond to the machine types 8 and 9 in Table I.

pole pieces inside the vessel as had been done on the Bevatron.

In September 1956 a parameter list was issued (see Table II) for a machine at 7 GeV to yield 10^{12} protons per pulse at 30 pulses per minute. The double magnet synchrotron had been abandoned because of its mechanical complexity. A spirally ridged magnet was favoured although the magnet yoke would be able to accommodate 'normal' constant gradient pole pieces if the ridged system turned out to be too difficult. Early thoughts on using Van de Graaff injector(s) had quickly given way to a strong focussed drift tube linac. By December 1956 severe dynamical problems had been discovered in the spiral ridge machine due to the presence of the straight sections. Strong coupling between the radial and vertical motion would cause unacceptable beam losses, so the spiral ridge design was abandoned. It was therefore decided to build a constant gradient machine (listed as type 2 in Table I).

Table II. Parameter List for a 7 GeV Proton Accelerator Specified in September 1956

Proton (kinetic) energy	7.0 GeV
Type	Spiral ridge synchrotron or constant gradient synchrotron
Peak magnetic field	14,000 Gauss
Machine radius	61.6 feet
Number of magnet sectors	8
Length of straight sections	12.5 feet
Number of ridges	24 (spiral ridge)
Radial aperture	36 inches
Vertical aperture (mean)	5 inches (spiral ridge) 9 inches (constant gradient)
Steel weight	6,000 tons
Peak stored energy	2×10^7 joules (spiral ridge) 3.5×10^7 joules (constant gradient)
Residual gas pressure	10^{-6}mm Hg
Vacuum chamber	Stainless steel, enclosing pole tips
Power supply	Alternator-ignitron set
Injection energy	10 MeV to 13 MeV
Type of injector	Linear accelerator (single tank, 100 MHz or 133 MHz, 40 feet long)
Injected current	2 mA peak
Injection time	1 ms
Number of protons accelerated to full energy per pulse	10^{12}
Repetition rate	30 pulses per minute
Mean current	5×10^{11} protons per second

The Injector

The Nimrod injection energy was set at 15 MeV to keep the magnet field at injection well above remanent levels. Since 10^{12} protons per pulse would be required (ie 1/6 of a microcoulomb) we aimed to produce 10 to 20 microcoulombs at 15 MeV. A view of the RF cavity is shown in Fig. 2.

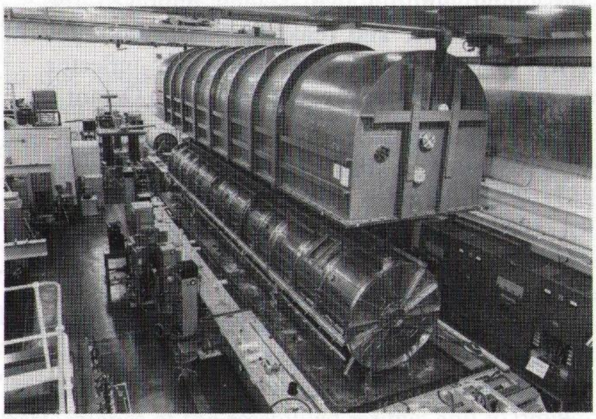


Fig 2. The RF cavity of the Nimrod injector with the vacuum cover raised.

Several important developments took place in providing the high intensity, long pulse, ion source and energy stabilisation of the 600 KeV pre-injector beam. We knew little about the radial phase space we would need in the linac to accommodate such intense beams and believed we should use large apertures, of about 2 cm diameter, in the drift tubes. This and the design of the first few quadrupoles led to our choosing a linac frequency of 115 MHz, much lower than the conventional 200 MHz. Although there was a lot of local experience in proton linac design, some new ground had to be broken and our engineers produced a fine result. It is amusing to recall that the RF liner, an accurately dimensioned copper vessel supported in a stainless steel fuselage-like structure, was made by a well-known saucepan manufacturer from Birmingham called London Aluminium Containers Ltd.

We made our first 15 MeV beam on 1 August 1961, but there were persistent difficulties with breakdown in the 1.5 MW RF circuit feeding the linac and with multipactor discharges in the linac itself. The first trouble was cured eventually by introducing a small oil meniscus into a narrow air gap adjacent to the polythene anode capacitor; this started as a temporary solution but worked permanently. The multipacting was harder to tame. All the standard tricks for conditioning the drift tube surfaces failed but one idea kept haunting us. We knew carbon black had a low secondary emission coefficient but the idea of applying it to 98 drift tube surfaces seemed preposterous. We tried it in desperation and were successful with soot deposited from a taper flame. Lampblack applied by brush from a home-made suspension in alcohol also worked and became the standard treatment. It was eventually refined by experience to the stage where coatings were applied to a limited number of drift tubes and only to the regions where the electron trajectories terminate, ie close to the quadrupole ends. We obtained a 15 MeV beam again in February 1962 and never really looked back.

In the 18 month period till the beam was needed for commissioning Nimrod, we steadily improved the injector performance and reliability. Over 20 milliamps was passed through the achromatic inflector system as soon as we could get our hands on Octant 1 of the magnet in July 1963. The injection system was provided with a wide range of gadgetry for selecting different pulse lengths, beam intensities and phase space characteristics in preparation for the final tests of the synchrotron.

Limerick writing was a happy pastime during injector commissioning while waiting for running repairs, and the custom was continued by the Nimrod and beam line crews in later years. Here is my late contribution:

> *There was a young fellow named West*
> *Who found multipacting a pest*
> *He purged the cursed cavity*
> *Of electron depravity*
> *His candle flame laid it to rest.*

The Magnet

The magnet design needed to use the minimum weight of steel to provide the desired peak field of 7 GeV with the correct gradient (n value) for focussing at all field levels starting from injection. We wanted 36 inches of good radial aperture in the main ring magnet at injection, but could economise in steel by allowing the good field width to shrink during acceleration to 14 inches at peak field. This was to be realised by a clever pole-piece design where, in effect, the pole-piece shape changed appropriately as the beam shrunk by allowing some of the laminations to saturate. To do this, the pole pieces were made using various combinations of laminations stamped from .020 inch and .030 inch thick sheet, shaped so as to saturate as required and to give the desired gradient value of n = 0.6 . Four different types of laminations were used (see Fig. 3). Pole face windings were added to make fine corrections of the field distribution and to adjust the height of the median plane.

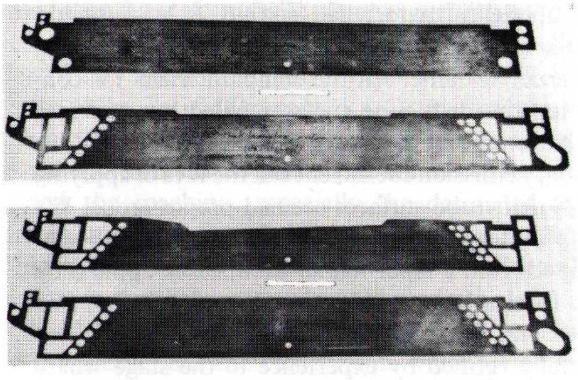


Fig 3. Four types of lamination used to fabricate the pole pieces.

The 7,000 ton magnet was built from 1/4 inch steel plate assembled into 336 sectors, 1 foot thick and 10 feet square, each weighing 20 tons. Three 2/7 scale models were used to check the calculations on the yoke design and to model the pole pieces. Both high and low (remanent) field characteristics were determined, the high fields under pulsed conditions. Then three full-scale models were built. One was used for measuring the sector magnetic characteristics as they were delivered, another was used for the final modelling of the pole pieces and the third as a full engineering mockup to check out all the mechanical engineering details and the final design of the straight section boxes. The sector design was committed before the final pole-piece design could be made.

The manufacture of the sectors started badly. Before randomising and assembly, all the individual plates were annealed to optimise their magnetic properties, about 75 tons at a time. Initial attempts at annealing produced sheets with huge distortions up to 6 inches out of plane! The annealing process was eventually mastered using a process cycle of about 1 month duration, much longer than initially envisaged, with six ovens operating simultaneously.

The sector assembly required special dust-free conditions and sector machining needed a specially constructed hygienic, temperature controlled room, and special techniques to avoid creating shorts between laminations in the throat aperture. A sector under test is shown in Fig 4.

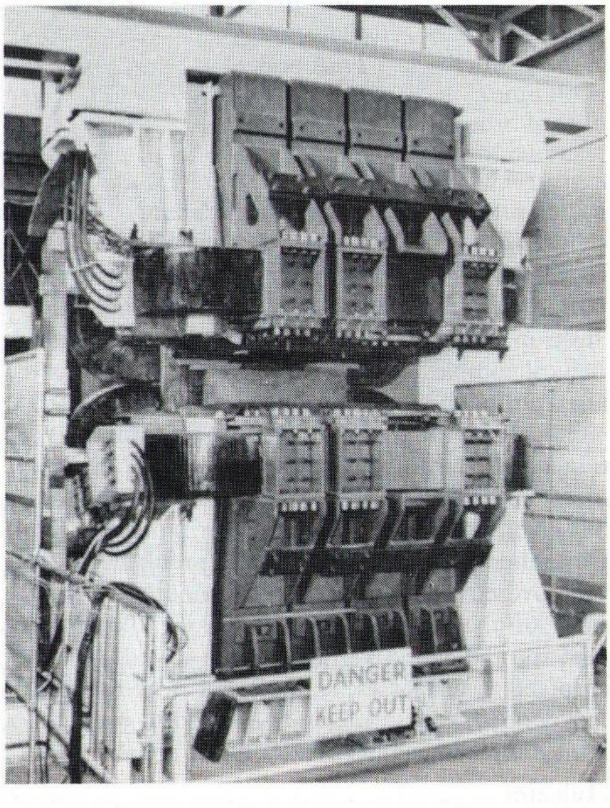


Fig 4. The sector test rig

The magnet sector contract was placed with Joseph Sankey (Bilston) in 1957. By mid-1959 sectors were reaching us at the rate of 1 per day and some had to be stored temporarily. It took 2 weeks to measure the first sector in model 4 test rig. Of course this improved, but there was a lot to catch up. John Wilkins offered a pint of beer for each person involved in the testing for every 1/4 sector per day improvement in the rate compared with the previous record. This was when the rate was 1 per day and he reckoned he was at risk for 16 or 20 pints (4 or 5 people). In fact it cost him 6 times this amount.

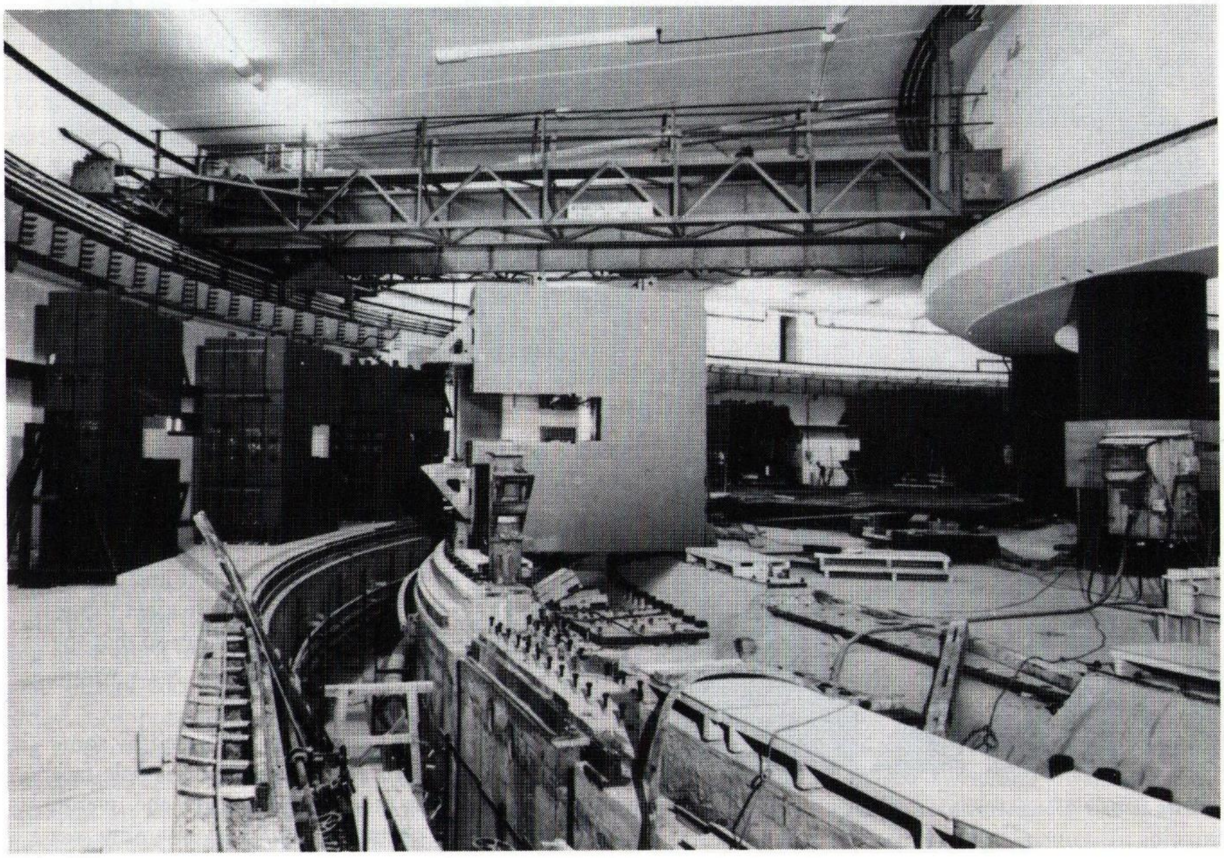
Following the sector measurements, they were distributed on the magnet monolith shown in Fig 5 so as to optimise the distribution of the sector errors. The resulting average error in the field per octant at 300, 10,000 and 14,000 Gauss was less than the accuracy of measurement (ie better than a few parts in 10^4), corresponding to a radial aperture loss of less than 1 inch. The total variation in octant weight was 12 cwt (0.1 %), although individual sectors (42 per octant) varied by as much as 4 cwt.

The pole-piece laminations were insulated and glued using a thin layer of epoxy resin. A whole stock of lamination material was shuffled to ensure uniformity of magnetic properties in all pole pieces, which were produced in matched pairs. Variation in profile did not exceed .004 inch between laminations. Each pole-piece assembly was made to a very carefully controlled sequence to give a final weight tolerance of ± 1 %. The assemblies were cured in a cycle lasting 15 hours. They were despatched to the Laboratory only after exhaustive tests of dimensions, insulation, strength, etc. The whole process lasted 2 years.

In due course the pole pieces were installed in the magnet sectors and a magnetic survey of the machine was made, octant by octant, and taking 6 months during 1962. Fields and field gradients were measured with search coils throughout the rising field pulse. The raw data (voltages) were recorded on a 2-channel tape recorder compatible with the CEGB's IBM 7090 computer. The coil outputs were sampled at 10 kHz, and digitised and recorded. The 7090 manipulated these data to give the desired field and field gradient information which could then be used to check the performance of the assembled magnet and to provide data for planning pole-face winding currents, running orbit calculations, setting components like the inflector, target mechanisms, ejection magnets etc. It is believed this was the first time such a survey was done with the measured data going straight on to tape in a form suitable for handling by a computer.

The magnet survey showed that the sectors and pole pieces were properly located and that there were no

Fig 5. Inside the magnet hall showing the monolith on which the magnet sectors are being placed.



errors in the magnetic field shape which could not be corrected by using the pole face windings. A graph of the results of the field gradient measurements versus the orbit radius is shown in Fig 6.

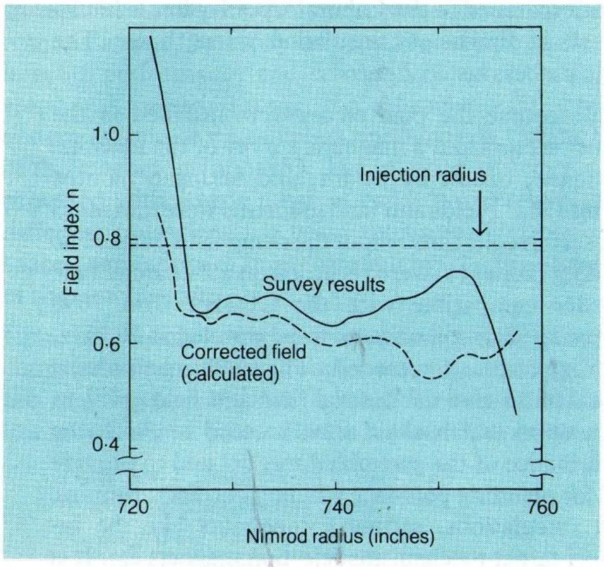
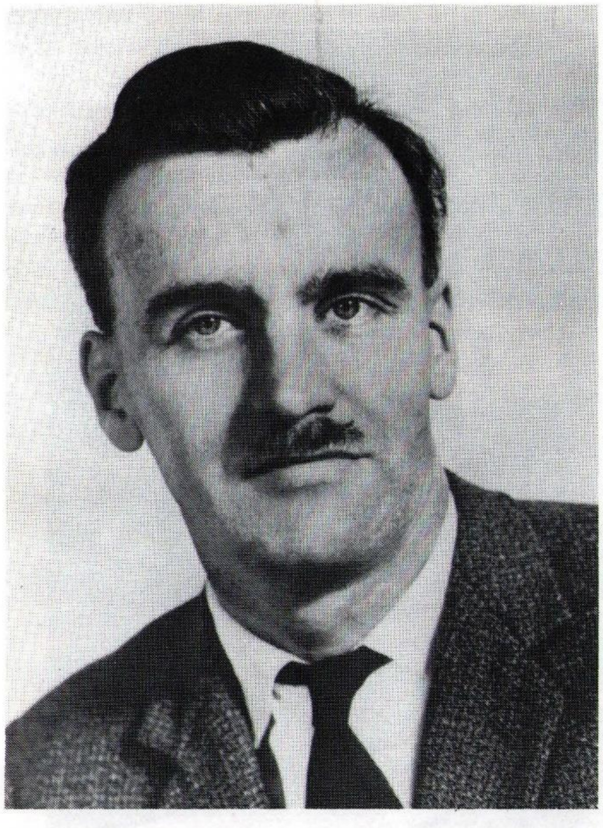


Fig 6. Variation of the field index n versus the orbit radius in Nimrod at 300 Gauss.

The overall magnet design proved to be very efficient. Had we simply scaled the design from the 3 GeV Saturne synchrotron we would have needed nearer 10,000 tons of steel compared with the 7,000 tons actually used. Moreover the special pole-piece design won us 5 % higher energy. The whole result was a tribute to the magnet group led by John Wilkins, who was tragically killed in a car accident in Zurich in 1962. We remember today the enormous contribution he made to the success of Nimrod through his ability as an engineer physicist and the exacting standards he insisted upon. Nor do we forget the twinkle in his eyes.



John Wilkins, who led the magnet group until his death in a car accident in 1962.

The Vacuum Vessels

Now we must dwell a while on the vacuum vessels. None of the solutions adopted in the existing proton synchrotrons would meet our requirements. One way or another they used too much vertical aperture, used materials with poor ultimate vacuum, would get too hot through eddy current heating or would deteriorate too rapidly through radiation damage. We flirted with one promising idea where a glass-fibre reinforced epoxy resin laminate would have its single wall supported by sky hooks fastened to the magnetic sectors. It lost 2 inches of vertical aperture but might have been accepted were it not for uncertainties in the effect of the metal attachment bushes on the magnetic field and the loss in strength of the laminate at the attachment point due to radiation.

The actual double-walled design adopted (Fig 7) became notorious. With an outer wall thickness of 1/8 inch and inner of 1/4 inch it lost only 3/4 inch of vertical aperture, but it was difficult and costly to make. Each of the 3 vessels in each octant was to be about 15 m long to within a 3 mm tolerance and curved to subtend 45°. They had to fit into the magnet throat accurately and mate closely to one another so that the special PVC nitrile elastomer vacuum seals would work.

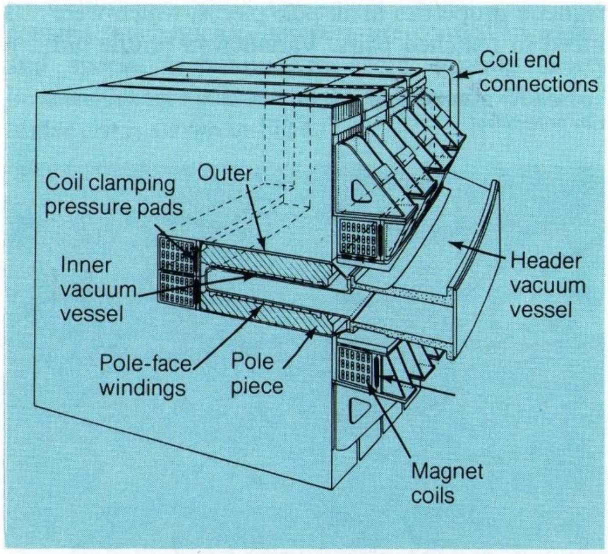


Fig 7. Pictorial view of a cross-section through a magnet octant.

In addition, the outer vessel was perforated by some 500 assorted holes for pole-piece supports, pole-face winding connections etc. The design of the overall sector, pole piece, vacuum vessel, coil assembly was full of genius. The outer vessel was supported by the pole pieces which were themselves fixed by jacks in the throat and bolts running through the outer vessel to brackets on the sectors. The coils were clamped in position by flat profile pressure bags which provided just the kind of restraint needed.

Nothing like our vacuum vessels had ever been made before. As with many of the Nimrod contracts, we could only look for firms which were already using similar processes. Plastic boat builders were considered, but manufacturers of aircraft nose-cones looked the most likely. In February 1959 the order was placed with Marston Excelsior of Wolverhampton at a cost of £375,000. The technique envisaged was to make separate thirds of each side (top and bottom) of a vessel, then splice and join to make complete sides, followed by a dorsal splice to complete the back as shown in Fig 8. End flanges would then be added.

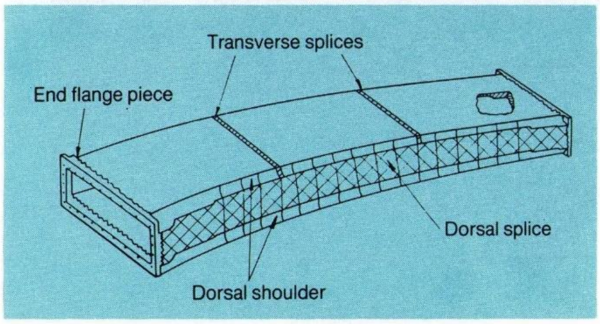


Fig 8. Sketch illustrating stages in the manufacture of the vacuum vessel.

Each third would be made by laying up the laminate on a die bed and then curing a limited length at a time using several top tools or punches. Curing was at 150°C. Early runs showed resin shortage in splices, resin richness in flanges, damaged cloth at tool joints and 'fir tree' like inclusions. Vacuum tests of samples of faulty-looking regions were apparently satisfactory. The Laboratory was very unhappy, but the first prototype went ahead. After bouncing 100 miles down the A34 it leaked at all the suspect places and extensive repairs on the scale indicated could hardly be contemplated. This was February 1961 and the agreed price was now £750,000.

The situation looked hopeless. Gerry Pickavance held a public house meeting at Wolverhampton with the Rutherford team and decided to carry on. Four Rutherford Laboratory and AEA staff then became full-time supervisors on the shift at Marston's. After a 6 week production engineering study, 50 faults were rectified in the procedures for making 'thirds'. The first good third was made after 3 months, followed by good outers and eventually inners and headers. An added complication for the inners was the application of a 10 cm wide stainless steel foil to give an antistatic surface and limit the area of exposed epoxy to the vacuum. In all, about 100 men worked on shift to make the vessels, for which the final price was about £1.5 million. Their successful manufacture was a major technological achievement.

During the dark days of 1961 some alternative vacuum vessel designs were considered. We called them commissioning vessels but the situation looked so serious at one time the alternatives might have been permanent and their use would have down-graded Nimrod significantly. However all such defeatist ideas were killed by decision at a Technical Committee meeting in November 1961.

Following delivery to the Laboratory, the vacuum vessels went through an exhaustive programme of leak testing (illustrated in Fig 9) and rectification involving 46 men on shift. All vessels were brought to within stringent leak rate limits before installation (see Fig 10).

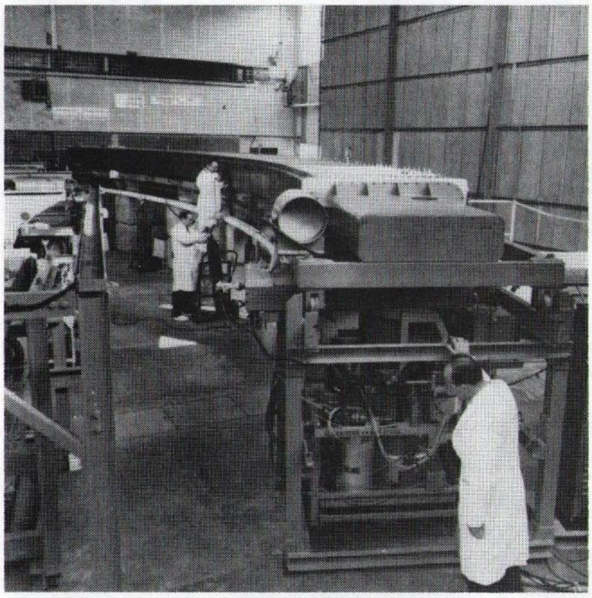


Fig 9. Vacuum vessel leak testing.

Late delivery of outers meant that about three-quarters of the pole-piece installation had to be done twice to avoid delaying the magnetic survey. After the final pole-piece installation there was a 24 hour magnet shakedown period for each octant, and a final tightening of pole-piece fastenings. Inner vessel installation then began and, as octants were closed and pumped down, a progressive commissioning with beams started. Before taking up the story of commissioning proper, several other main aspects of the installation must be mentioned, albeit briefly.

The Power Supply

Some unusual plant was required to power the main ring magnets, as seen in Fig 11. The plant had to isolate the pulsed magnet load from the national grid. The system used phased switching of ignitrons to transfer energy to and fro between the flywheels of the twin rotating plant system and the magnet. In spite of

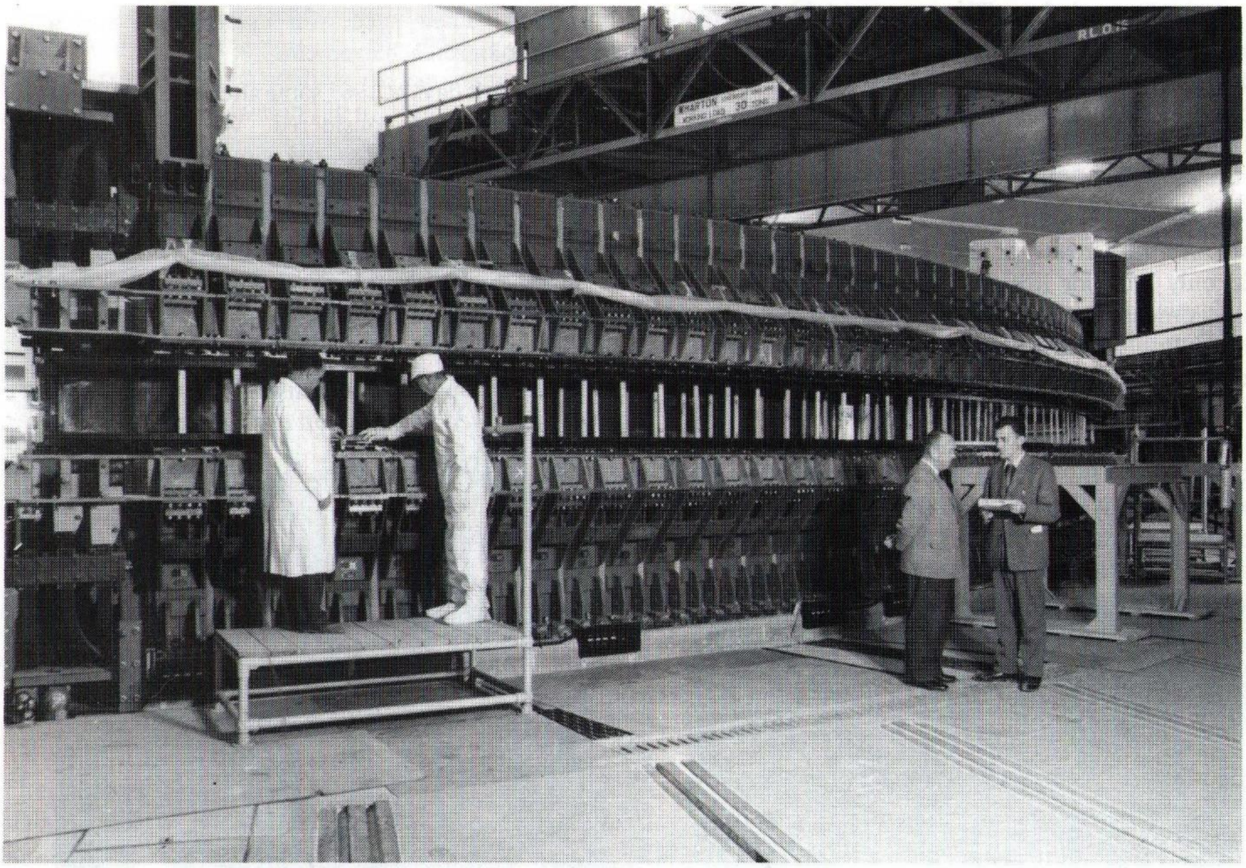


Fig 10. Installation of the vacuum vessel.

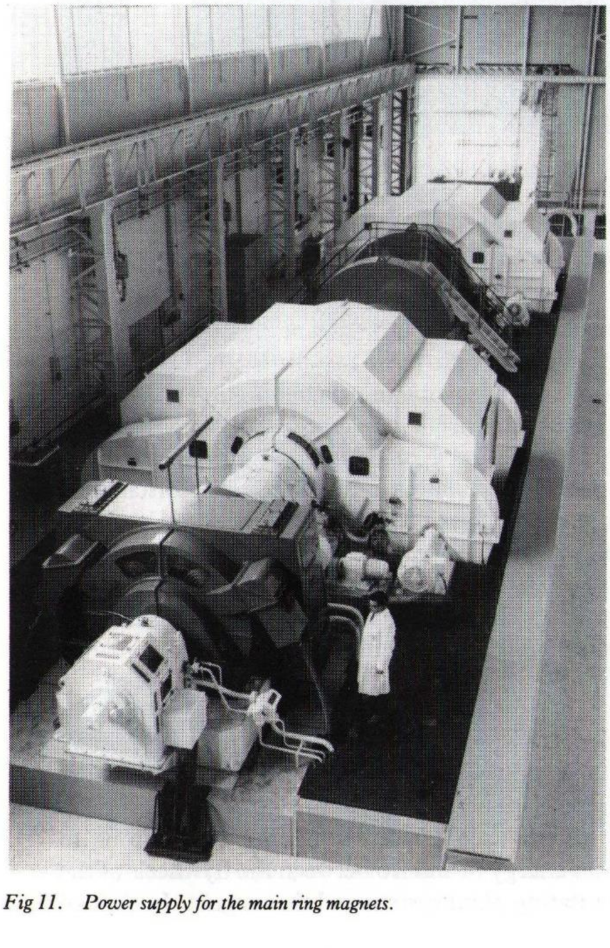


Fig 11. Power supply for the main ring magnets.

all the work on magnet optimisation, we required the largest pulsed power supply of its kind anywhere. It was an exacting duty in which each machine was loaded from zero to 100,000 horsepower in 0.75 second and then loaded again to the same extent in the opposite direction, sometimes after only a few milliseconds. The plant was provided with automatic ultrasonic scanning of the rotor system from a central borehole through the shafts and also a strain gauging system on the shafts. (A well meant attempt to protect the strain gauges by taping over some shim steel almost resulted in an injury to Bert Brooks when the shim steel flew off. The hole in the ceiling still remains to this day!) We found there would be significant savings by splitting the job between two contractors, one for the rotating plant and one for the convertors, instead of buying all the plant from one source. This was a crucial decision to take but we decided to accept the responsibility of co-ordinating the two contracts and defining interfaces.

In operation the power supply proved to be remarkably reliable and versatile, its record being marred only by the fatigue failure of the alternator rotor end plates in 1965.

The RF Accelerating System

Peter Dunn always told us that a synchrotron was an RF accelerating cavity with a magnet and a vacuum vessel to ensure circulation of protons through it. His group made it all seem so easy that the only real problem with the high power part was holding the 5 tons of ferrite together. The ferrite tuned the cavity over the range of 1.4 to 8 MHz during acceleration. Eventually a hot-setting Araldite seemed to work but the ferrite was clamped mechanically to make sure. The cavity gave very little trouble although its polythene insulators did break down once or twice due to an accidental pressure rise in the main vacuum vessel. The arrangement of the RF cavity is shown in Fig 12.

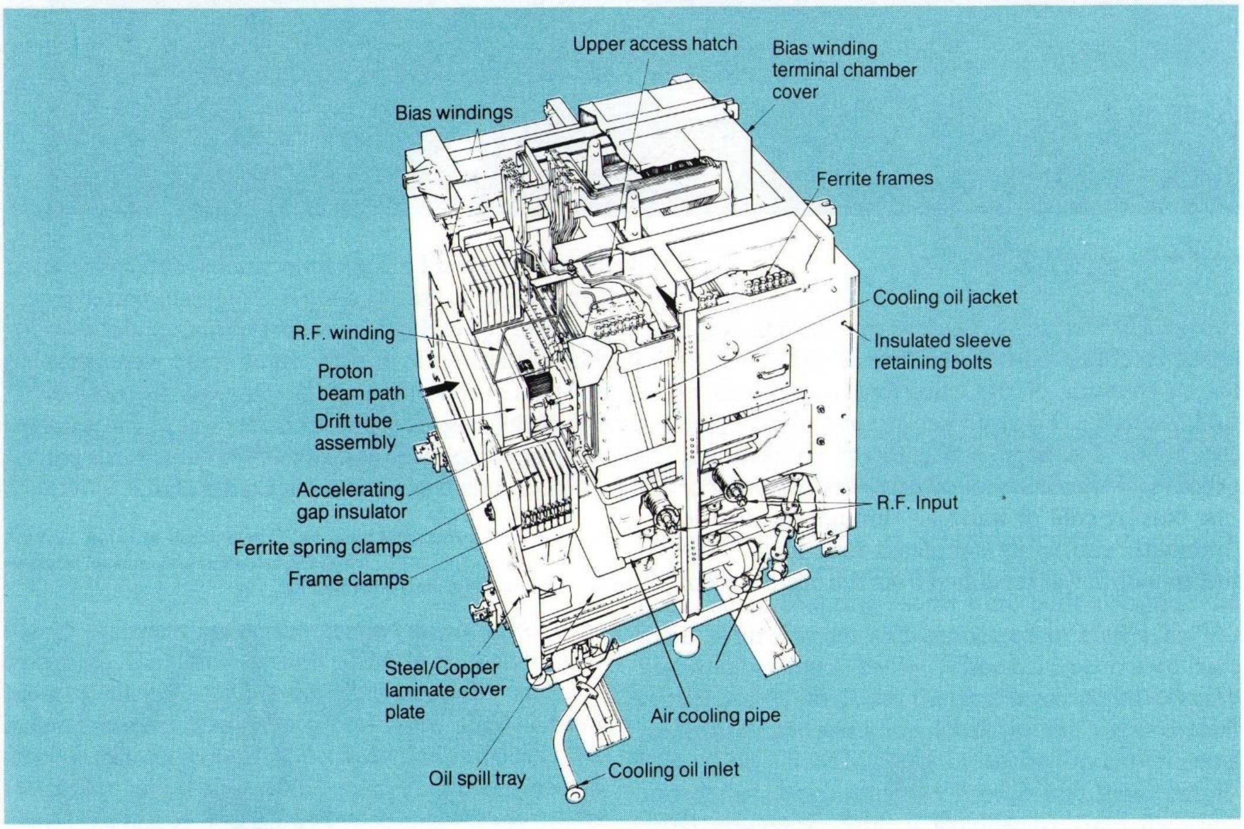


Fig 12. The RF accelerating cavity.

A very important part of the accelerating system was the primary frequency generator (PFG) whose job it was to make the accelerating frequency follow the variation of the magnetic field as shown in Fig 13. The PFG had to generate the required frequency/field characteristic very accurately with a permissible error close to zero (about 0.05%) at the start of acceleration, rising to perhaps 0.5% halfway through acceleration but falling again to 0.05% towards top energy.

Signals from the magnet field were used to control the master oscillator frequency with residual errors being initially corrected by a manually set function generator or curve corrector, and later by servoing from the radial pickup electrodes. Also associated with the RF system was the phase-lock loop which kept the cavity RF phase-locked to the passage of the proton bunches.

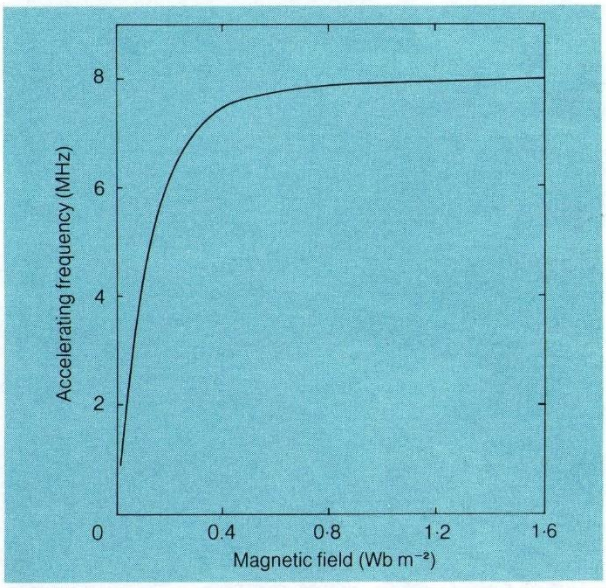


Fig 13. Variation of the accelerating frequency versus the magnetic field.

Buildings

All the care taken in the manufacture of magnet sectors, and in their selective location in octants, had to be matched with similar care in ensuring foundation stability and correct alignment. The magnet had to remain planar throughout the life of the machine, radial symmetry had to be preserved and each block had to remain perpendicular to the orbital plane. The foundation should not hog or sag more than 0.040 inch, nor tilt more than 0.25 inch across its diameter. The site at Chilton was proved by trial pits, load tests and boreholes to confirm the absence of voids and the Geological Survey's prediction that the lower chalk

Opening up the site.

was about 230 feet deep. The combined loads of the building, shielding and machine were large and entailed the stressing of the whole foundation area to about 2.5 tons/ft^2. To avoid expansion problems, the magnet room was kept at a constant temperature, irrespective of whether the machine was operating or not, by passing cold or warm air through the duct in the monolith and using simulated heat loads. The magnet monolith was separated from the magnet room walls and the shield bridge foundations, although the roof and earth shielding on top had to be supported by all of these. An early view of the building under construction is shown in Fig 14. In all there were some 50,000 tons of concrete and 3,500 tons of steel used to construct the magnet room. Concrete was at one stage of construction poured at a rate of 400 cubic yards per day, higher than was achieved at Calder Hall.

Fig 14. Construction of the Nimrod buildings.

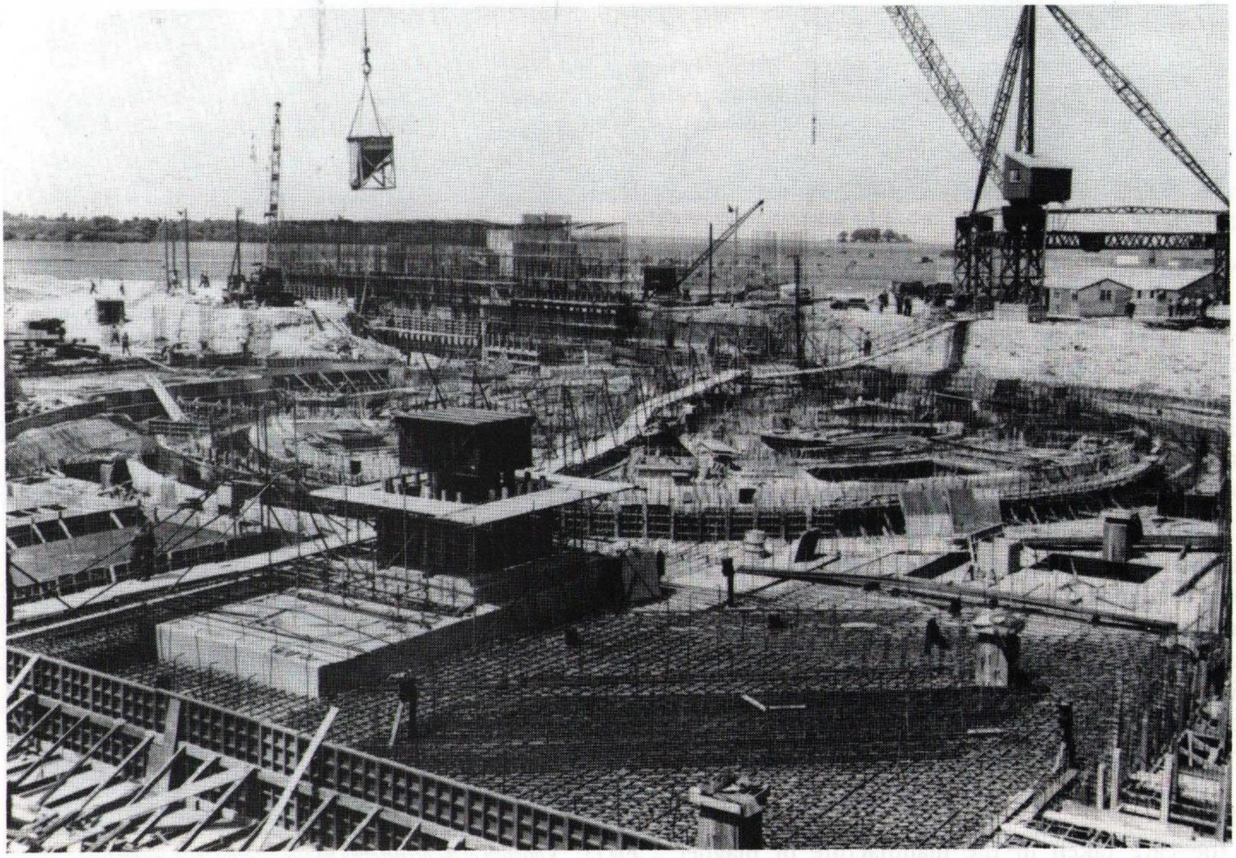


Fig 15. Inside the magnet room. Floor datum points were used to accurately survey the location of the sectors and pole pieces.

Fig 15 is a view inside the magnet room. After construction, the floor datum points were established using a survey system designed specially and the magnet sectors and pole pieces were aligned from them so that all sectors lay between limiting planes .02 inch apart, and within ± .005 inch of the mean orbit in plan. Pole pieces were correct to sectors within ± .003 inch. Heights of pole pieces were initially within ± .01 inch of the orbit plane but drifted to ± .02 inch under the influence of roof loading. About 95% of pole pieces had a tilt of less than ± .003 inch across the surface; 50% were less than ± .001 inch.

At the time of building design we knew very little about shielding. The data were simply not available. So the 10 to 20 feet of earth put over the magnet was certainly excessive. (There is a very early sketch which shows no shielding over the magnet at all, and with all the buildings surrounded by a large earth embankment). The same uncertainty influenced the shield bridge design and the azimuths at which secondary beams could be taken from the magnet room. This bridge was given additional support near its centre in the form of a special pillar limited to an 8 feet by 1 foot cross-section at the median plane. There was much discussion as to whether it would be safe for people to run experiments from the experimental floors. This was being done at the Bevatron and Cosmotron, but remember the factor of 100 improvement in intensity at which we were aiming. To cope with a possible need, some counting rooms were built alongside the main control room and connected to Hall 1 by 140 high-quality signal cables. They have been used once or twice only. We learnt that it was comparatively easy to realise safe radiation levels in the experimental halls. The whole business of setting up experiments and data taking would have been infinitely complicated otherwise. Still, it would have been the same world wide, so we would not have noticed the difference.

The Final Commissioning

In principle the final commissioning was straight forward:

- Set the magnet and injector pulsing.
- Inject at the right time to get protons circulating freely in the ring.
- Turn on the RF at the right frequency, follow the magnetic field rise accurately and the protons would be bunched and accelerated in phase-stable orbits.

In practice we had to take the steps one at a time using the beam itself to check that the necessary conditions were being met adequately at each stage.

We began to think about the commissioning experiments during 1960 at the Nimrod Physics Committee. At that time we expected the machine to be ready about mid-1962. (Delays with the vacuum vessels, pole pieces, buildings, stainless steel delivery had forced a revision from the 1961 date). I began to co-ordinate the ideas of the physics groups about mid-1960 and from January 1962 moved full-time onto this task and started holding commissioning meetings. By then we expected the machine to be ready in September 1963. There were no more slippages and in fact the installation was finished early in August 1963. Through these meetings we planned the commissioning strategy, decided on a lot of diagnostic hardware which would be needed and co-ordinated the final installation work in the magnet room to enable all the sub-system commissioning to be completed as early as possible. On one occasion we heard about enormous median plane errors which were found in the magnetic survey — like the 3 inch median plane bump in one spot caused by a temporary mild steel jig and smaller perturbations caused by some rogue mild steel screws inadvertently used in pole-piece jacks. At a late stage we recognised the damage which might be done to the vacuum vessel by the 15 MeV injected beam, especially if it were mistimed, and introduced a lot of protection in the form of graphite screens and beam trimmers round the vessel.

During 1962 we became concerned about the hazards in the magnet room as construction work had to go on with magnets under power, and later there was the hazard from implosion of the large glass windows on the vacuum vessels. Many of the construction workers were unfamiliar with these dangers, so we had several meetings in the Lecture Theatre at which I told everyone working on the job about the hazards and the need to follow the defined procedures. We were also concerned about the possibility of sabotage which was not unknown towards the end of major construction jobs. For both these reasons we instituted a strict check on access to the magnet and injector rooms — everyone going in and out had to be on an approved list. There were in fact no fatal accidents at this or any other time on the construction project but we did lose one glass window on the vacuum system in June 1963 due to leaving a flood lamp shining on it. There were lots of glass and graphite chips to clean up and we were delayed a couple of weeks running the first beams in Octant 1, nothing more.

We taped special announcements, using the voice of Ted Eglinton, to warn those in the machine rooms when magnet pulsing or beam-on was imminent and we evolved the search routine to ensure nobody was left behind for any reason. It was also necessary to find all the possible non-standard ways someone might conceivably get into the machine rooms and block or lock them off. There were many of these — I walked, climbed and crawled with Ron Russell into places I'd never dreamt existed. We also had to prevent access to the experimental areas until we had established the radiation levels there.

As N-day approached we held commissioning meetings every week instead of fortnightly, and of course there were as many *ad hoc* meetings as necessary. In May 1963 it became necessary for Ron Russell to exercise hour-to-hour co-ordination of work in the magnet room. (The Nimrod operations group had taken over from the AEA construction group in March). We planned the first crewing arrangements.

The main items of diagnostic equipment in the ring are illustrated in Fig 16. There were large full-aperture hinged beam stops at the end of Octants 1, 3 and 7 which could give current signals. Remotely movable probes to carry small targets, fluorescent screens, apertures etc were available — three in Octant 1 and three in other positions. There were full-aperture fluorescent grids in six octants. TV cameras transmitted the beam images to a special multiframe display tube in the main control room. Then there were the induction electrodes of the permanent beam control system which gave us the radial position of the beam centre-of-gravity as well as the circulating charge.

Between 24 May and 19 July we had 15 days of running with the 15 MeV beam into Octant 1, then through to Octant 3, enabling us to commission the inflector system, the injection timing, and most of the commissioning diagnostic systems. Once we were puzzled when the beam wouldn't go past Octant 1. A full aperture stop had stuck in, but Fred Gilbert sorted it out. Conclusions in the log for that run were faithfully recorded by the duty officer, David Gray:

1. *The inflector will stand the electric field.*
2. *Timing system for injection works.*
3. *The grids work.*
4. *Fred Gilbert is strong.*

Each day the installation work went on plus the modifications or repairs decided on after the previous night. There were fenced areas at the octant ends and we aimed to finish any work there by noon, so that magnet pulsing could be started in the afternoon. We tried to clear the magnet room by 5 pm and make beams by 6 pm. It was incredibly difficult to retrieve a beaming situation by 6 pm when perhaps 30 people had been loose improving their own patches throughout the day. I find a note in some minutes which says *"everyone* **must** *obey message 3 to clear the magnet room"* and *"people are* **still** *going through straight section fences"*.

When the whole ring was available on 6 August we started periods of 4 days of running separated by 3 days of maintenance. We would run up the injector in the morning, start the magnet pulsing about noon and attempt to make beams from about 2 pm. On the first attempt to inject we saw the beam on all the grids and there were indications of a second turn. I will not give a blow-by-blow account of events over the next few runs. We gradually improved the survival time of beam at injection and the circulating beam intensity

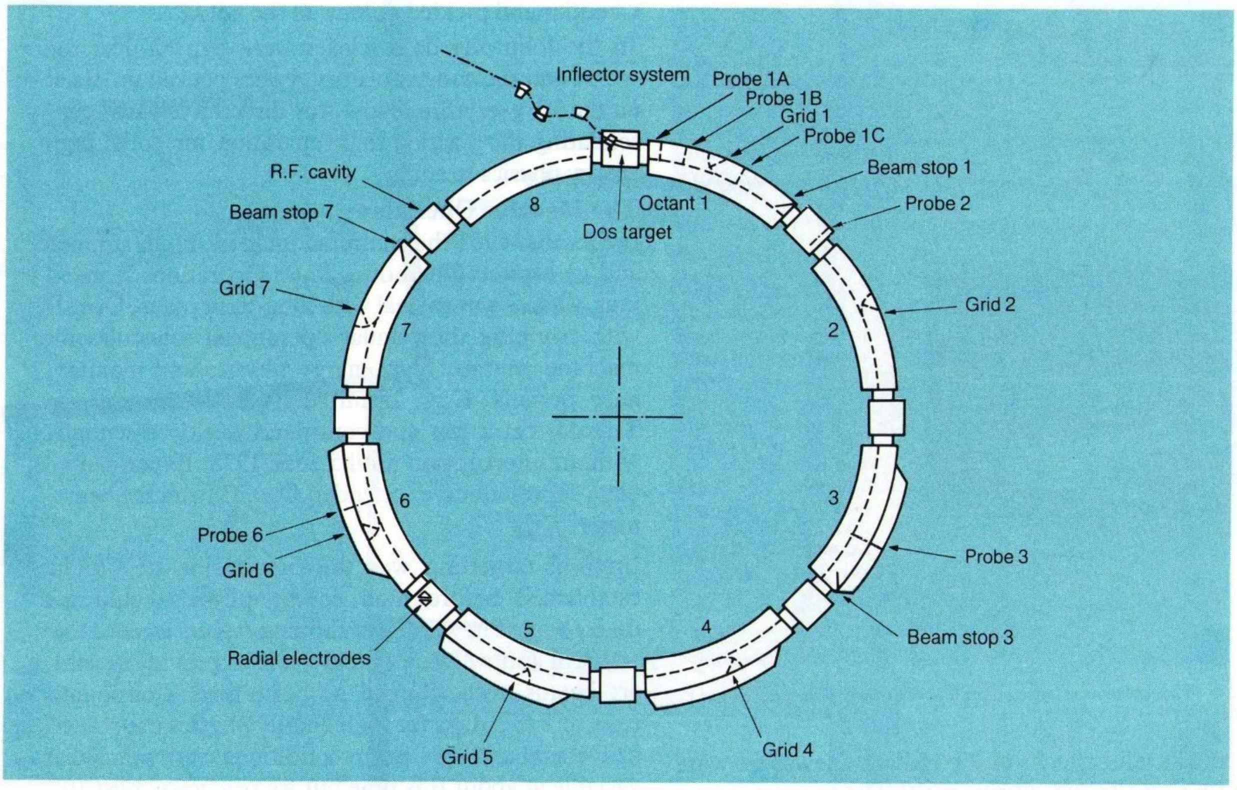


Fig 16. Diagram illustrating the major items of diagnostic equipment in the main ring.

and we learnt to interpret the diagnostic signals. Early attempts to accelerate failed, but we had no proper pole-face winding supplies then or the curve corrector for the RF frequency programme. At one stage we were at, or very close to, the disastrous working point of $Q_V = 1$, $Q_R = 0.5$. Clearly such a working point on or close to integral and half-integral resonances was not conducive to beam survival. There were the usual, easily diagnosed, equipment failures and less obviously the occasion when the grids were parked in the aperture during an attempt to accelerate, and the fact that initially all the pole-face windings were connected back-to-front. One night we were frustrated by having to stop early because the unruly element of the team had too much Morlands for tea.

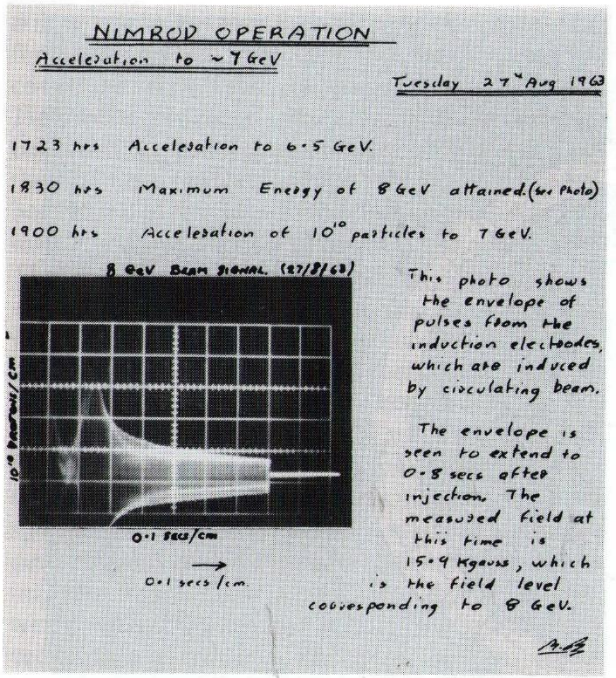
By 23 August we had proper pole-face winding power supplies and had learnt how to produce good circulating beams with good pulse-to-pulse reproducibility. Limited acceleration had been realised on several occasions, the last being to around 200 MeV, still without the curve corrector.

It was finally available on 26 August and we got out to nearly 1 GeV that night, but the curve corrector settings were critical. On August 27 we carefully optimised the injection conditions again, then the RF, and finally with the curve corrector got out step-by-step to 3 GeV. Terry Walsh recorded in Orbit that *"there was a ripple of a cheer as Nimrod graduated from being the highest energy accelerator in Berkshire to the highest in Britain. The long reign of the Birmingham synchrotron was over!"*

Everything now depended on the RF (just as Peter Dunn had taught us) and only 2 or 3 of the RF group knew how to tweek the curve corrector. Bill Galbraith was pacing about demanding to know what was the hold up. The trouble was that 3 GeV corresponded to the last segment of the curve corrector and beyond that the PFG was running at a frequency outside tolerance. This was just around the knee of the frequency law. Evidence was that the knee was too sharp and it was decided to change a small coil in the oscillator circuit to give a shallower knee. This would take some time. While Len Appleby was doing this, some of us got hungry and went off to the restaurant.

Always trying to do better, Roy Billinge worked away to re-optimise the injection conditions and got a 1 milliamp beam circulating, the highest yet. The RF came on again but of course the adjustment of the curve corrector had to start again from the beginning. Beam was soon taken right out to the end of field rise at 6.5 GeV, and dutifully recorded in the log (see Fig 17).

Bill Galbraith had set a small Cerenkov detector near the vacuum vessel and connected it to an oscilloscope in one of the counting rooms. He was probably the first to see full acceleration on his detector which was more sensitive than the induction electrodes. Anyhow, he burst into the MCR and said *"You're there!"*. The weaker ones in the restaurant heard first from Jeff Louth who ran all the way. As he approached our table I had a sudden horror there had been some kind of major set-back. He gasped out the news and we all returned, excited, to the control room.



NIMROD OPERATION

Acceleration to ~7 GeV

Tuesday 27th Aug 1963

1723 hrs Acceleration to 6·5 GeV.

1830 hrs Maximum Energy of 8 GeV attained. (see Photo)

1900 hrs Acceleration of 10^{10} particles to 7 GeV.

8 GeV BEAM SIGNAL. (27/8/63)

This photo shows the envelope of pulses from the induction electrodes, which are induced by circulating beam.

The envelope is seen to extend to 0·8 secs after injection. The measured field at this time is 15·9 Kgauss, which is the field level corresponding to 8 GeV.

0·1 secs/cm

→ 0·1 secs/cm.

Fig 17. The control room log recording the achievement of design energy on 27 August 1963.

We did careful checks of the radiation levels we were creating and found all was well. The magnet power supply was coaxed to give a peak field of 15.8 kG and beam went right up to 8 GeV with an intensity of 10^9 protons per pulse. Running again at 15 kG we found we could optimise everything to achieve 10^{10} protons per pulse. It was a great occasion, duly celebrated on the way home. The master of the Cherry Tree in Steventon evidently could sense the importance of the occasion because he provided a very large lump of Cheddar and pickled onions on the house.

In the following days telexes were sent all over the world and the congratulatory replies poured in. Back on the job everything now was directed towards consolidating the position in preparation for some high energy physics running.

The Machine Operation

In October 1963 the commissioning meetings stopped and we started Operations I and Operations II meetings. Ops. I was to deal with long-term plans, Ops. II with planning the regular operational schedules for machine studies, high energy physics and maintenance periods. Ops. II initially met every week on Tuesdays at 2 pm and continued weekly essentially without interruption until 5 June 1978. Experimental team representatives attended Ops. II from the beginning.

Internal targetting was the first major step to be established before high energy physics could get under way. The system of flipping targets worked like a charm and we were quickly able to provide secondary beams to the P2 and $\pi 1$ beam lines. Continuous runs of 2 to 3 days for high energy physics use started in December 1963. Beam conditions were somewhat variable at about this time but we reached almost 10^{11} protons per pulse in January 1964, when we had the radial servo running on the RF to centre the beam. The plunging mechanisms for the external beam were also being installed. A layout of the beamlines early in 1964 is given in Fig 18.

In February we managed to circulate 7.5 milliamps in the ring at injection and we had a new PFG. With much better conditions we were up to 10^{11} protons at 6 GeV

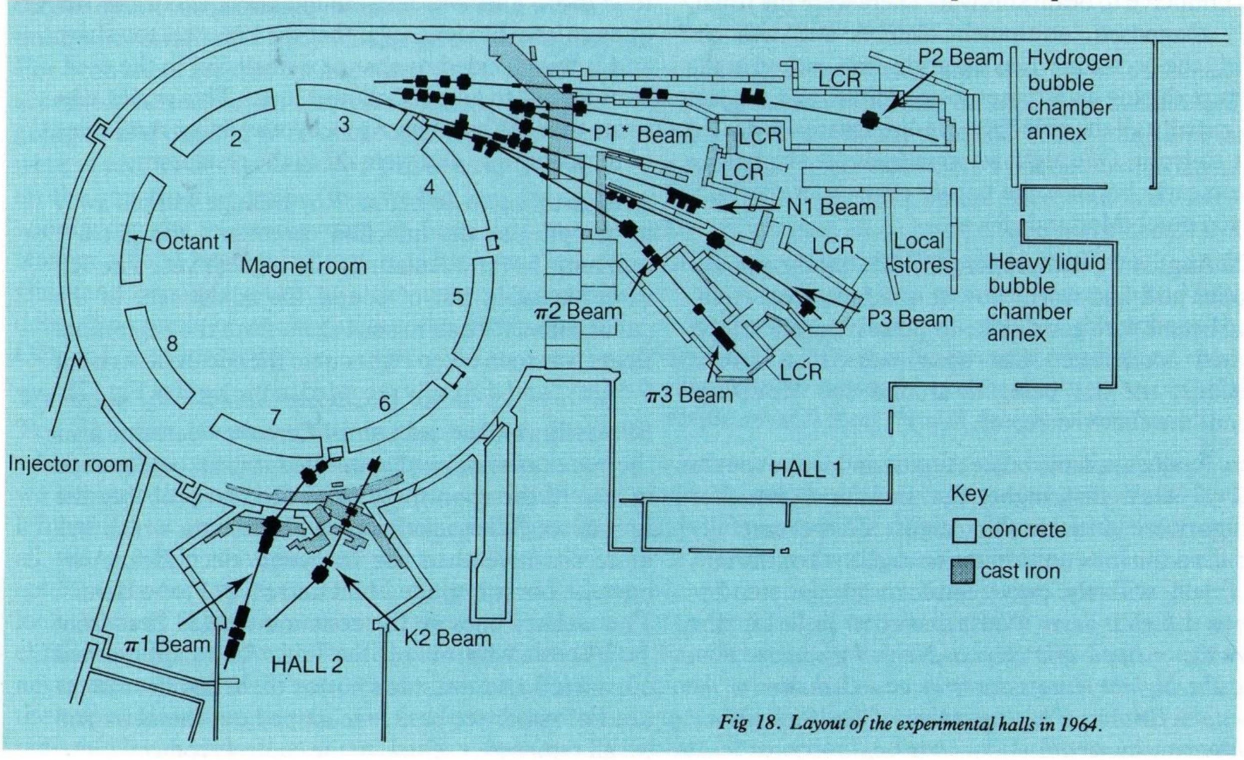
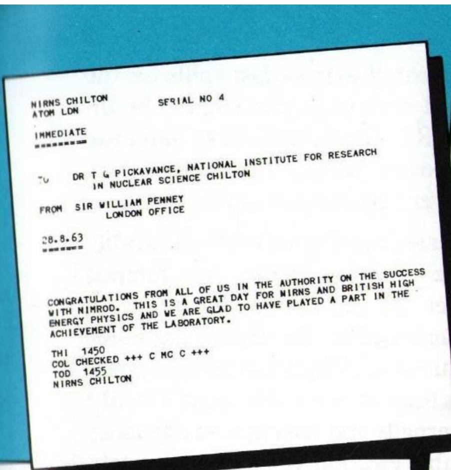


Fig 18. Layout of the experimental halls in 1964.



NIRNS CHILTON SERIAL NO 4
ATOM LDN

IMMEDIATE

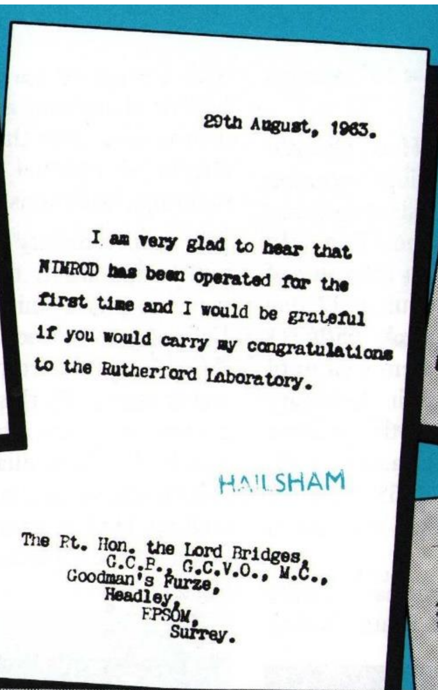
TO DR T G PICKAVANCE, NATIONAL INSTITUTE FOR RESEARCH
IN NUCLEAR SCIENCE CHILTON

FROM SIR WILLIAM PENNEY
LONDON OFFICE

28.8.63

CONGRATULATIONS FROM ALL OF US IN THE AUTHORITY ON THE SUCCESS WITH NIMROD. THIS IS A GREAT DAY FOR NIRNS AND BRITISH HIGH ENERGY PHYSICS AND WE ARE GLAD TO HAVE PLAYED A PART IN THE ACHIEVEMENT OF THE LABORATORY.

THI 1450
COL CHECKED +++ C MC C +++
TOD 1455
NIRNS CHILTON

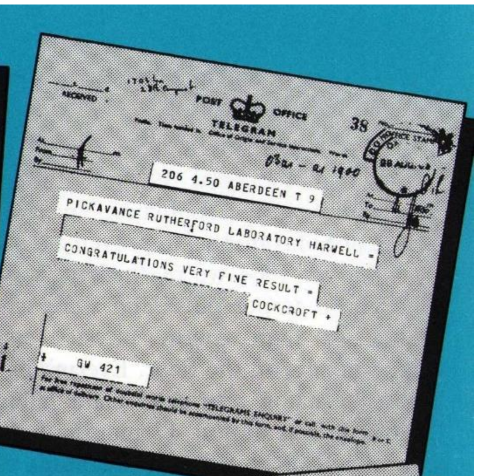


29th August, 1963.

I am very glad to hear that NIMROD has been operated for the first time and I would be grateful if you would carry my congratulations to the Rutherford Laboratory.

HAILSHAM

The Rt. Hon. the Lord Bridges,
G.C.B., G.C.V.O., M.C.,
Goodman's Furze,
Headley,
EPSOM,
Surrey.



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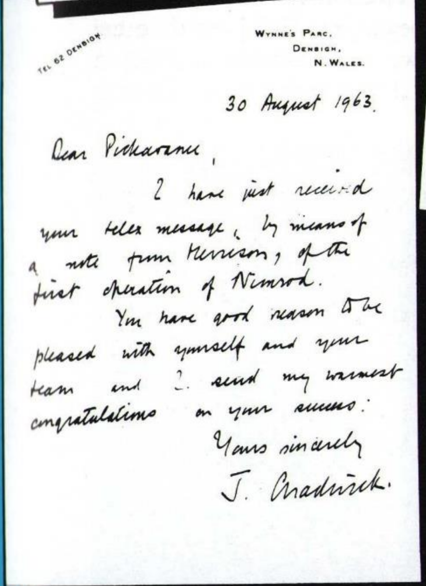


TEL 62 DENBIGH

Wynne's Parc,
Denbigh,
N. Wales.

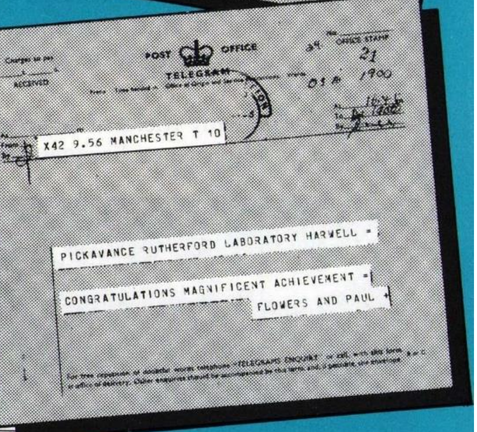
30 August 1963

Dear Pickavance,

I have just received your telex message, by means of a note from Harrison, of the first operation of Nimrod.

You have good reason to be pleased with yourself and your team and I send my warmest congratulations on your success.

Yours sincerely
J. Chadwick.



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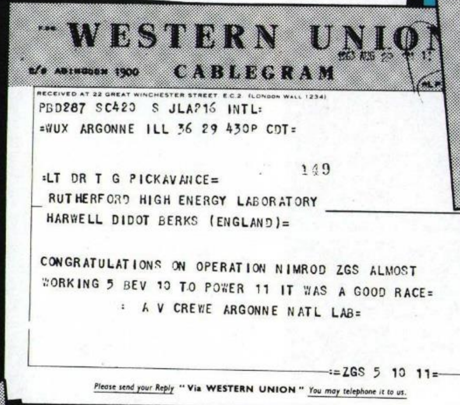

WESTERN UNION
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D/S ABINGDON 1900

1963 AUG 29

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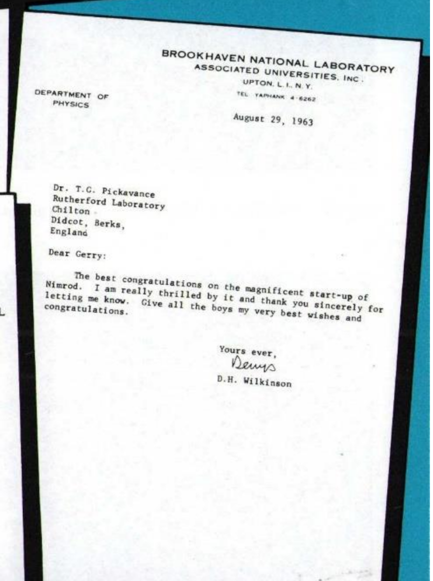
=LT DR T G PICKAVANCE=
RUTHERFORD HIGH ENERGY LABORATORY
HARWELL DIDOT BERKS (ENGLAND)=

CONGRATULATIONS ON OPERATION NIMROD ZGS ALMOST WORKING 5 BEV 10 TO POWER 11 IT WAS A GOOD RACE=
: A V CREWE ARGONNE NATL LAB=

:=ZGS 5 10 11=

Please send your Reply "Via WESTERN UNION" You may telephone it to us.



BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.
UPTON, L. I., N. Y.
TEL. YAPHANK 4-6262

DEPARTMENT OF
PHYSICS

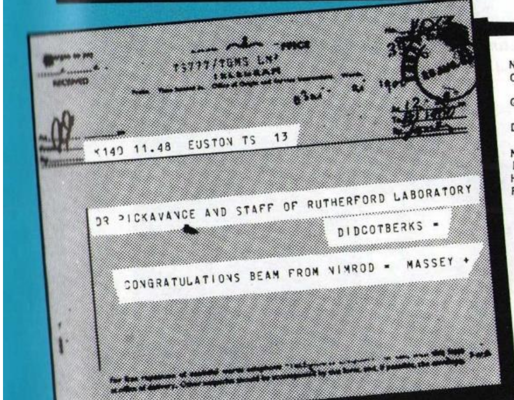
August 29, 1963

Dr. T.C. Pickavance
Rutherford Laboratory
Chilton
Didcot, Berks,
England

Dear Gerry:

The best congratulations on the magnificent start-up of Nimrod. I am really thrilled by it and thank you sincerely for letting me know. Give all the boys my very best wishes and congratulations.

Yours ever,
Denys
D.H. Wilkinson



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DR PICKAVANCE AND STAFF OF RUTHERFORD LABORATORY
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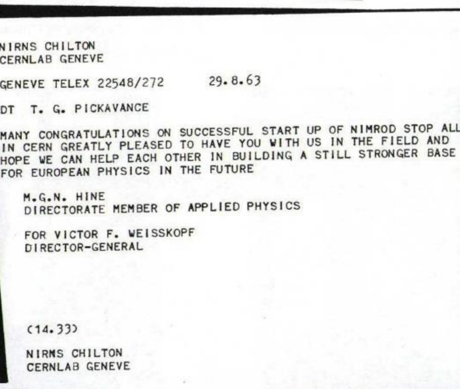
CONGRATULATIONS BEAM FROM NIMROD = MASSEY +



NIRNS CHILTON
CERNLAB GENEVE

GENEVE TELEX 22548/272 29.8.63

DT T. G. PICKAVANCE

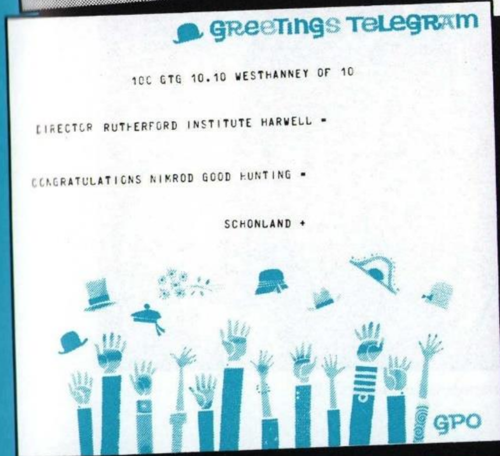
MANY CONGRATULATIONS ON SUCCESSFUL START UP OF NIMROD STOP ALL IN CERN GREATLY PLEASED TO HAVE YOU WITH US IN THE FIELD AND HOPE WE CAN HELP EACH OTHER IN BUILDING A STILL STRONGER BASE FOR EUROPEAN PHYSICS IN THE FUTURE

M.G.N. HINE
DIRECTORATE MEMBER OF APPLIED PHYSICS

FOR VICTOR F. WEISSKOPF
DIRECTOR-GENERAL

(14.33)

NIRMS CHILTON
CERNLAB GENEVE



GREETINGS TELEGRAM

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CONGRATULATIONS NIMROD GOOD HUNTING =

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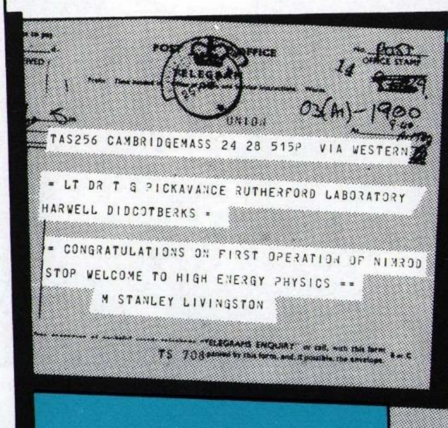

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TAS256 CAMBRIDGEMASS 24 28 515P VIA WESTERN

= LT DR T G PICKAVANCE RUTHERFORD LABORATORY
HARWELL DIDCOTBERKS =

= CONGRATULATIONS ON FIRST OPERATION OF NIMROD
STOP WELCOME TO HIGH ENERGY PHYSICS ==
M STANLEY LIVINGSTON

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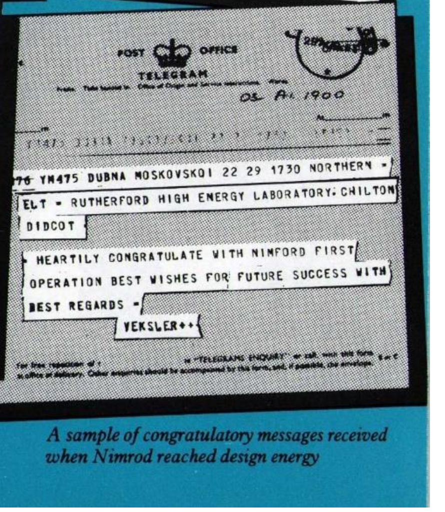

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ELT = RUTHERFORD HIGH ENERGY LABORATORY: CHILTON
DIDCOT

= HEARTILY CONGRATULATE WITH NIMFORD FIRST
OPERATION BEST WISHES FOR FUTURE SUCCESS WITH
BEST REGARDS =
VEKSLER++

For free repetition of ... "TELEGRAMS ENQUIRY" or call, with this form at office of delivery. Other enquiries should be accompanied by this form and, if possible, the envelope.

A sample of congratulatory messages received when Nimrod reached design energy in 24 hours, and before long there was 2×10^{11} protons being shared on flat top at 7.2 GeV.

Nimrod was inaugurated by the Rt. Hon. Quintin Hogg on 24 April 1964 at the official opening ceremony of the Laboratory. The story since then has been one of continual improvement on most fronts. In September 1964 we reached the design intensity of 10^{12} protons per pulse and were scheduling 12 day runs for high energy physics research with 11 experiments on the floor. Everything went well until the alternator rotor end-plate failure in February 1965. It was almost 12 months before things were normal again, but in the meantime we ran at 2 GeV straight off the grid until November 1965 when we could use one alternator to reach 7 GeV at reduced repetition rate.

Over the years many tricks of beam gymnastics were developed to improve techniques for beam sharing with a range of burst conditions — fast spills for the bubble chambers, and slow spills (see Fig 19) by injecting noise into the RF signal, servoed to minimise effects of residual power supply ripple. Pole-face winding corrections were made more sophisticated.

The beam intensity (as seen in Fig 20) climbed steadily with steps when the injection energy was ramped using a phase shifter on the debuncher cavity in February 1968 and again when the second harmonic RF cavity was introduced in 1973 to increase the phase stable region. By this time we were able to get 4×10^{12} protons per pulse internally and the ejection efficiency was 50 %. Two reliable ejection systems were established, one feeding two beams into Hall 1 and the other feeding Hall 3 which we had available in 1968. A layout of the experimental halls at that time is given in Fig 21.

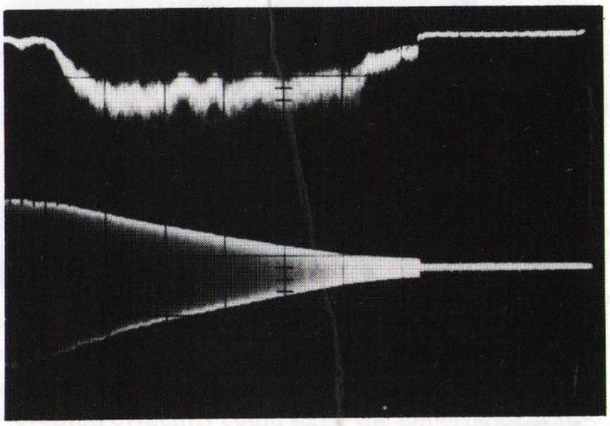


Fig 19. Oscilloscope display of the slow spill and circulating beam signals.

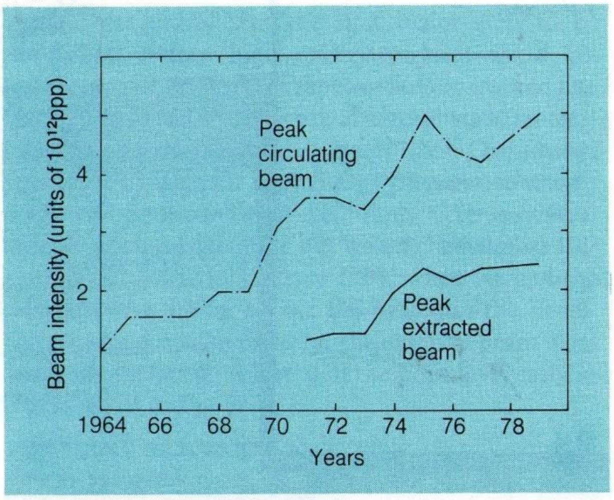
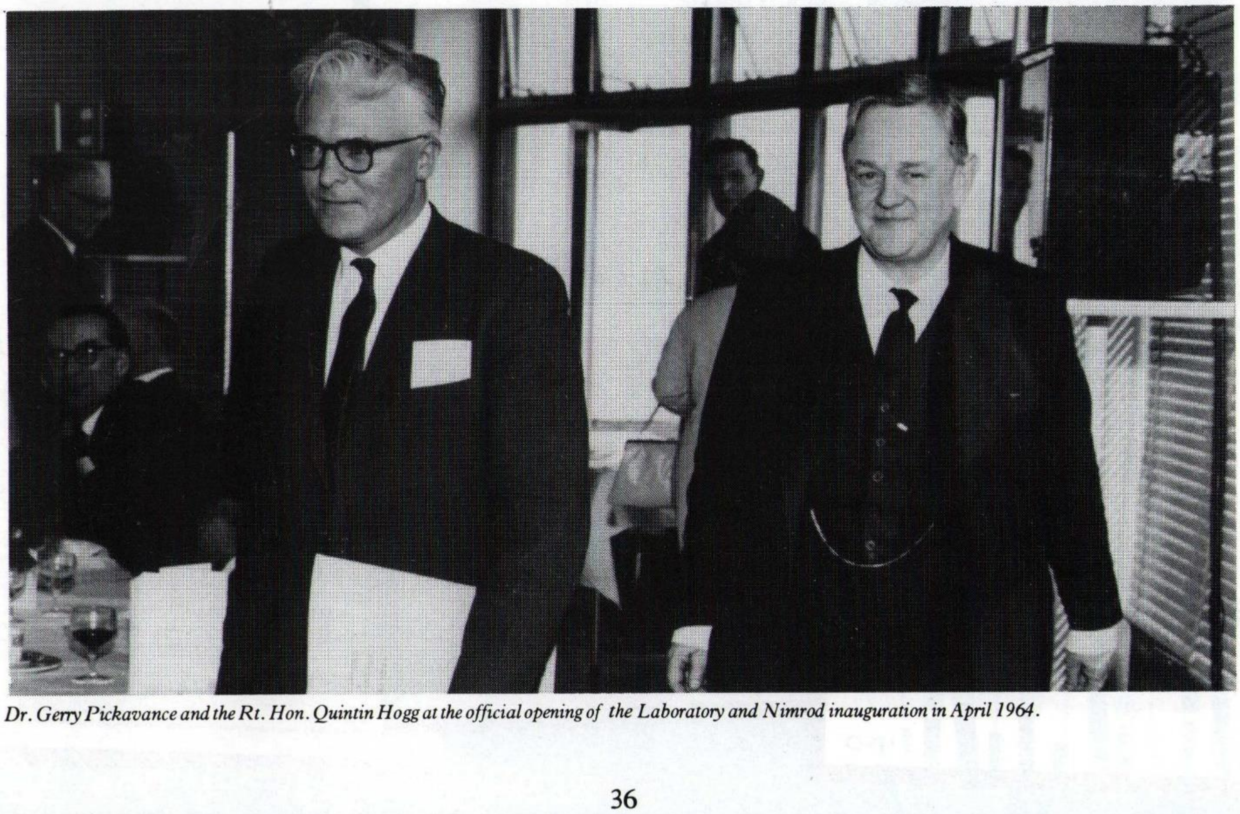


Fig 20. Record of circulating beam and extracted beam intensities for Nimrod operation over the years 1964 to 1978.



Dr. Gerry Pickavance and the Rt. Hon. Quintin Hogg at the official opening of the Laboratory and Nimrod inauguration in April 1964.

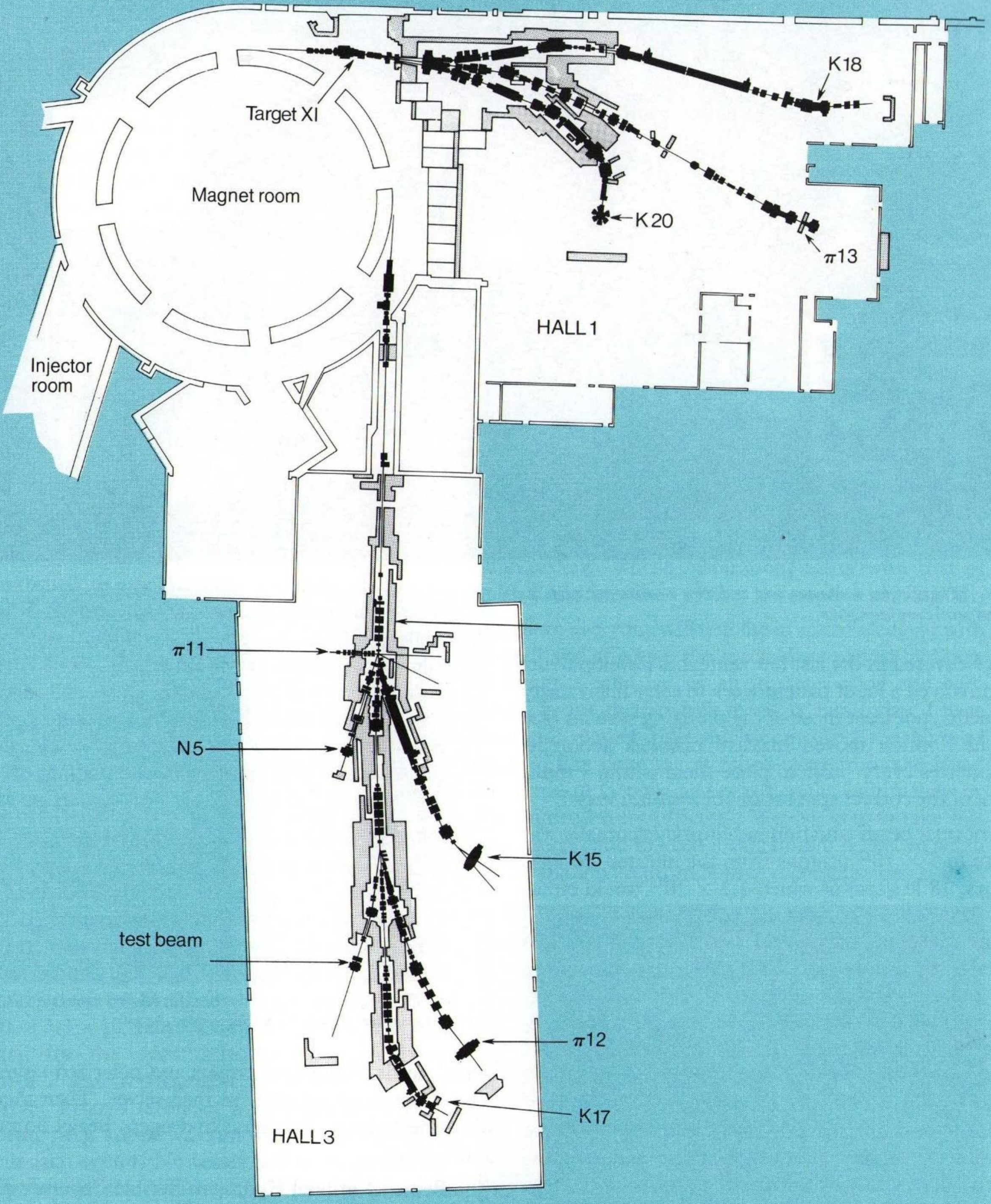


Fig. 21 Layout of the experimental halls in 1977

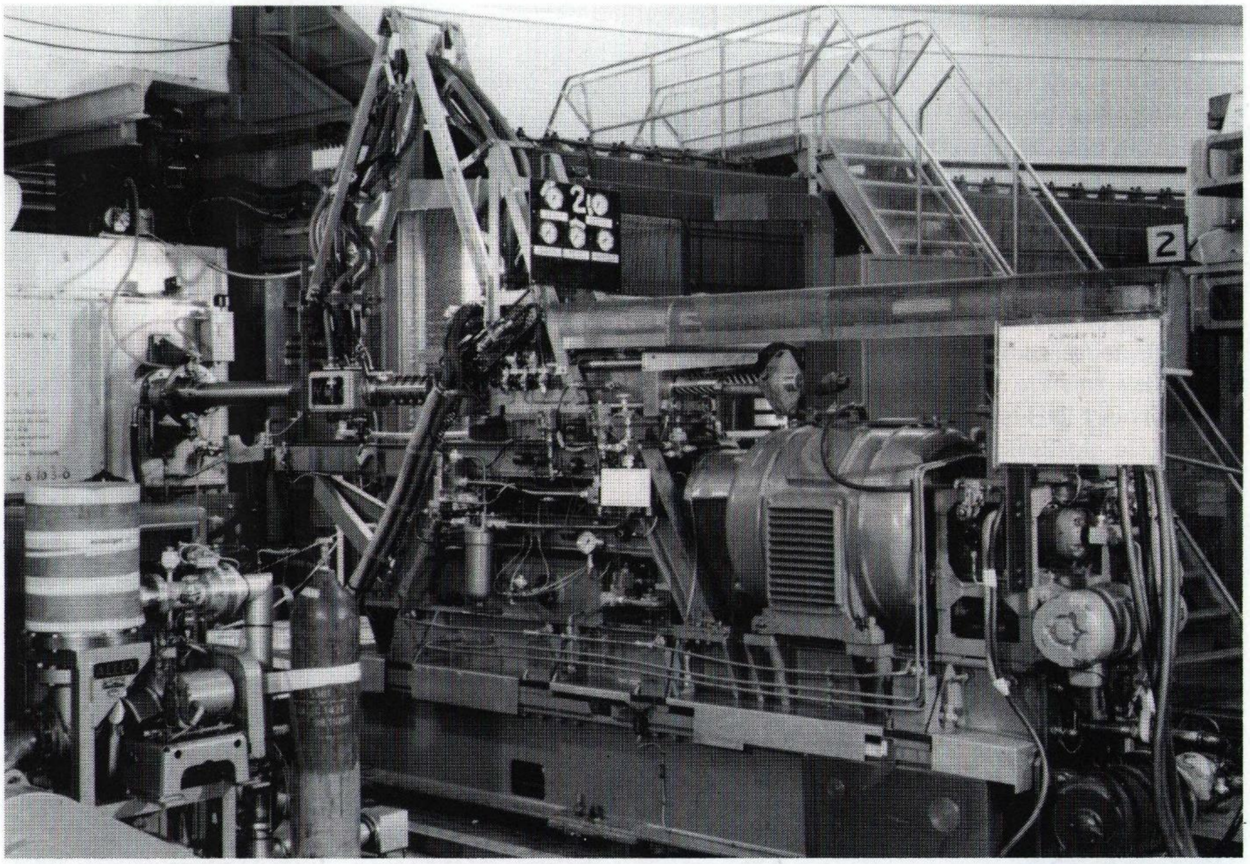


Fig 22. The plunging mechanism used to drive a 1 ton magnet inside the vacuum vessel every beam pulse.

I have covered in the last few sentences developments that involved a lot of patient work to solve many nasty problems, not least those of the plunging systems (see Fig 22.) which moved massive magnets about 30 centimetres every pulse to place them within 1 millimetre of the correct spot inside the vacuum vessel.

The normal beam situation in Nimrod eventually was to fire in 5 × 10^{13} protons from the injector (20 mA, 350 μs, 18 kG/sec) of which 1.5 × 10^{13} would circulate. About 7 × 10^{12} were trapped by the RF with the second harmonic cavity and 4 to 5 × 10^{12} were accelerated to full energy. This performance achieved that hoped for by the designers in 1955 (see Table I) — ie about 500 times the Bevatron intensity. Measurements showed (see Fig 23) that the trapped charge was space-charge limited and that further improvement would require a higher injector energy. The 70 MeV injector was built for this purpose in 1976 to take Nimrod up to 10^{13} protons per pulse and commissioned successfully as a linac. The decision to close Nimrod in 1978 led to the linac being mothballed until it was decided to use it for the Spallation Neutron Source.

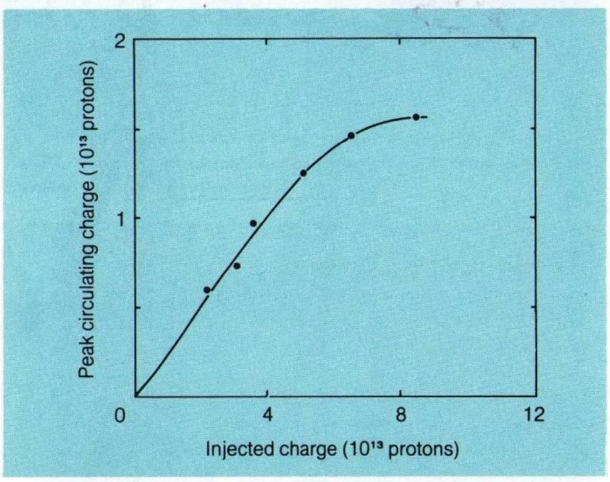


Fig 23. Plot of the peak circulating charge in Nimrod versus the injected charge, showing the saturation effect due to space charge.

There is not time today to do more than remind you of the vast quantity of beam handling apparatus such as magnets, quadrupoles and separators, which were built, installed and re-installed many times to meet the users' needs. Hydrogen targets and polarized targets were formidable projects in themselves. Then there were the bubble chambers: hydrogen, heavy liquid, and helium.

The British National Hydrogen Bubble Chamber did pioneering work at CERN and was eventually used to establish the new track sensitive target technique. We were also hosts for 3 years to the Saclay bubble chamber which, some say, arrived with its main vessel full of champagne!

In Conclusion

I would like to comment on one area of vast change which took place during the Nimrod project and shortly after — the electronic revolution — unrivalled in any other technological field. Much of Nimrod was built using electronic valves and old-fashioned wiring. However, transistors were used in many places and over the years there has been much miniaturisation. The RF group were so far ahead of everyone else (clearly overstaffed) that they were able to rebuild the PFG twice before it was needed in 1963, each time using more solid state components.

Computing also changed profoundly over this time. The first dynamics studies done by Bill Walkinshaw's group had to be made using approximate analytical solutions or mechanical desk calculators. When the job got too big for the desk machines they would use someone else's computer to run a few calculations to check the accuracy of the approximate analytical solutions. The early spiral ridge designs were done that way by running 3 or 4 orbits per week on the NPL or AWRE DEUCE computer and later on the Manchester Mark 1 Ferranti Mercury computer. This was 1955-57. Bill Walkinshaw had seen that computers would be essential when he and Gerry Pickavance were visiting America early in 1955, but it was 1958 before we had access to our own — the Harwell Mercury. Its first use was on the injector linac design which had been started on the Manchester machine. We continued to use Mercury for orbit studies in Nimrod including the assessment of vacuum vessel damage. The ORION was working in the Laboratory in August 1963, but before it was available we were forced to buy time on the CEGB 7090 and at Darmstadt, mainly for bubble chamber film analysis. The use made of computers for Nimrod up to then was miniscule compared with what happens in accelerator projects today.

So far as Nimrod hardware is concerned it is fitting that my last word should be about the vacuum vessels which gave so much concern in the early days. Even after we had learnt how to build them, their radiation lifetime remained a great unknown. The inners were the most vulnerable and our studies of radiation damage led us to believe that we could irradiate them to levels of 3.3×10^9 rads. A lifetime of 2 to 4 years was estimated for the worst affected vessels and we thought a 2 year replacement programme was just acceptable. From the outset a comprehensive dose monitoring programme was run with dosimeters at 36 places in each octant. No vessel failures occurred. In the quiet octants the total accumulated dose over the Nimrod lifetime is barely a few hundred megarads but the maximum dose has certainly passed 1000 megarads. The vessels have done a magnificent job. It will be interesting to examine them as Nimrod is dismantled. One wonders what we can now do with the vacuum vessels. The artist's impression in Fig 24 is one solution.

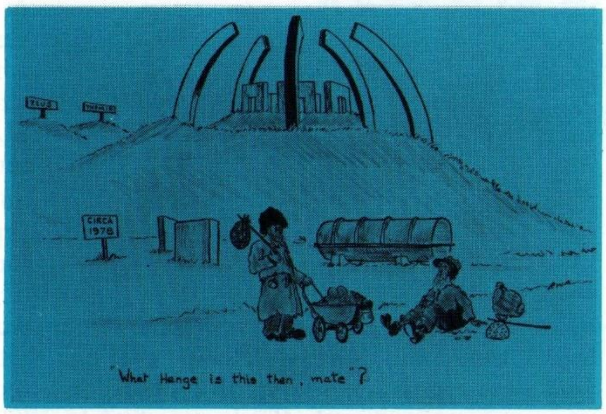


Fig 24. Artist's impression of what could be done with the Nimrod vacuum vessels.

I am sure that those who were present at those 1955 meetings in the Cockcroft Hall will agree that Nimrod was a success, an achievement reflecting credit not only on the project team but also on those who went on to operate and develop the facility.

It has been a privilege for me to tell you today something of its history. I have done this on behalf of all who were involved in whatever capacity in the 23 years since our saga began in 1955. It is good that we can share today the fellowship of the four people, Gerry Pickavance, Les Mullett, Percy Bowles and Bill Walkinshaw, whose inspiration, along with that of John Wilkins, was so vital to the creation of Nimrod.

I am sure everyone will join me in acknowledging with respect and affection the special part played by Gerry Pickavance who led us all in times good and bad. There is one further task to ask of him today. I have here the master key which controlled the Nimrod beam. (The large tag attached came from the Hotel Moderne in Geneva and was bought legitimately by the Nimrod duty officers). We ask you to take this key, Gerry, and to keep it safe until it is needed when the Spallation Neutron Source is commissioned.

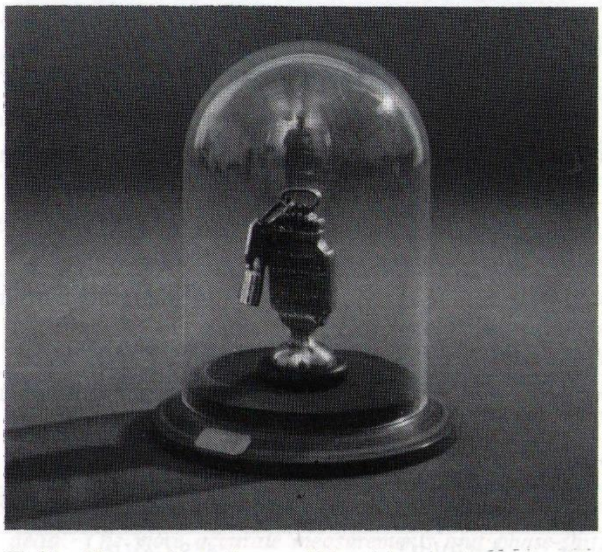


Fig 25. The master key used to control Nimrod throughout its operating life.

THE PHYSICS CARRIED OUT WITH NIMROD

by Professor R.H. Dalitz FRS

I propose to speak here about the physics results which have been achieved with Nimrod, attempting to put them into some appropriate setting. It would be too much to try to mention, in only 45 minutes, all of the experiments carried out with Nimrod. In consequence, I have had to be rather selective about which experiments I shall mention in any detail. My choice has involved a large element of randomness, but I have tried to indicate all the main areas of phenomena involved. I shall certainly not be able to mention all the major experiments, so I shall begin by expressing my apologies to all of those whose experiments are not mentioned by name.

Let me first remind you about the situation in elementary particle physics about the time Nimrod commenced operation. By 1964, almost all of the mesons and baryons which we now call semi-stable had become established. We knew about the quantum number of strangeness and even about SU(3) symmetry; Gell-Mann's famous Cal-Tech Report on the Eightfold Way was circulated early in 1961. All of the vector mesons, ρ, ω, $K^\star$ and ϕ, had been found. Pion-nucleon and pion-hyperon resonances were well-known to exist, although their detailed parameters were not always known. The (3,3) resonance, the $\Delta(1236)$ state, had already been known for about 8 years. Indeed, most of the baryonic resonance states which were later seen to form an SU(3) decuplet were already known to exist, and you will remember that SU(3) symmetry was clinched by the discovery of the semi-stable Ω^- baryon, which took place in 1964.

So Nimrod was not provided for the exploration of a completely unknown territory of physics, but rather for the deepening and consolidation of our knowledge of an area which had already been roughly surveyed. I would like to quote Maxwell on this subject, from the introductory lecture he gave at Cambridge when he took up his appointment there in 1871:

"The history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurement of quantities with which she has long been familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers."

And that is how it was with Nimrod — by the end of 1963, many peaks and bumps were known in a variety of scattering data, and now the time had come for their detailed study and analysis.

The major achievements with Nimrod have certainly been those in resonance physics and especially those involving baryonic resonances. I shall proceed to classify the experiments that were carried out by listing them as a series of "generations", summarized in Tables I to IV.

First Generation Experiments (from 1964)

The first generation of experiments, the beginning of the Nimrod programme, is well illustrated by the entries in Table I. The majority of them were con-

Table I. Some "first generation" experiments at Nimrod.

Experiment	Details	Collaboration
π^+p π^-p $d\sigma(\theta)/d\Omega$ and $P(\theta)$	$p_\pi = 650$-2140 MeV/c (12 momentum values)	Oxford-Rutherford
π^+p π^-p $d\sigma(\theta)/d\Omega$	$p_\pi = 1700$-2800 MeV/c (10 momentum values)	University College-Westfield
$\pi^-p \to \pi^0 n$ $\to \eta n$ $d\sigma(\theta)/d\Omega$	$p_\pi = 1710$-2460 MeV/c	Oxford-Rutherford
$\pi^\pm p$ and $\pi^\pm d$ $K^\pm p$ and $K^\pm d$ σ_{tot}	Incident momentum 600-2700 MeV/c	Birmingham-Cambridge-Rutherford
$pp \to pp$	Nuclear-Coulomb interference at 8 GeV/c	AERE–Queen Mary College–Rutherford
$pp \to pX^+$	$p_p = 2.8$ - 8 GeV/c	
pp and pd σ_{tot}	Lab. momentum 1-8 GeV/c	Cambridge-Rutherford
$np \to pn$ $d\sigma(\theta)/d\Omega$	at 8 GeV/c	AERE-Birmingham-Bristol-Rutherford

cerned with the study of pion-nucleon scattering, the measurement of differential cross-sections $d\sigma(\theta)/d\Omega$ and polarization differential cross - sections $P(\theta)d\sigma(\theta)/d\Omega$. The work of the Oxford-Rutherford collaboration, especially on π^+p scattering, demonstrated that the bump which was known to occur in the π^+p total cross-section $\sigma_{tot}(\pi^+p)$ at 1920 MeV centre-of-mass energy did in fact have a large F-wave component with spin 7/2. We know this state now as ΔF37(1920). The bump in $\sigma_{tot}(\pi^-p)$ at 1688 MeV was also found to be F-wave, but with spin 5/2; we know this state today as NF15(1688). From an analysis of the π^-p angular distribution over this bump, it appeared that there was almost certainly a further N* resonance, previously unknown, with negative parity and mass value close to 1670 MeV; we know it today as ND15(1670). The experiments on π^+p angular distributions by the University College-Westfield group gave the first evidence that the bump known at mass about 2420 MeV in $\sigma_{tot}(\pi^+p)$ had a large H-wave component with spin 1/2, now known as the state ΔH311(2420). Measurements of the differential cross-sections for pion-nucleon charge-exchange were also carried out by the Oxford-Rutherford collaboration, which were later relevant to the analysis of the N*(2190) bump.

In this first phase, there were also accurate measurements made by the Birmingham-Cambridge-Rutherford collaboration of the total cross-sections for positively and negatively charged pion and kaon beams incident on proton and deuterium targets. The data they obtained for the $\pi^\pm p$ total cross-sections are shown on Fig 1, together with the earlier data from other laboratories. In Fig 2 we show some of the data obtained for $\sigma_{tot}(K^-p)$, compared with the mean curve obtained from earlier data, in order to show the small peak at a laboratory momentum value of about 800 MeV/c on the side of the major peak. This small peak was the first sighting of the resonance state we now know as ΛD03(1690). The data obtained for the π^+d and π^-d systems allowed a comparison between the charge-symmetric systems π^+p and π^-n, and also taught us a good deal about the relationship of cross-sections on deuterium with those for nucleon targets.

The event which we may logically use to mark the ending of this first generation of experiments was the 1967 Conference on Pion-Nucleon Scattering, held on the Irvine campus of the University of California. I would like to quote from the keynote speech by Herb Steiner, as follows:

"New measurements of π^+p and π^-p total cross-sections for momenta between 0.5 and 2.65 GeV/c have recently been reported. These results, which are of a very high quality, were obtained in a collaborative effort between Birmingham, Cambridge and Rutherford at Nimrod. Another group, also at Nimrod, has made very detailed measurements of polarization in π^-p scattering at 50 different momenta between 0.64 and 2.14 GeV/c. As you will see, they did a very thorough job of it".

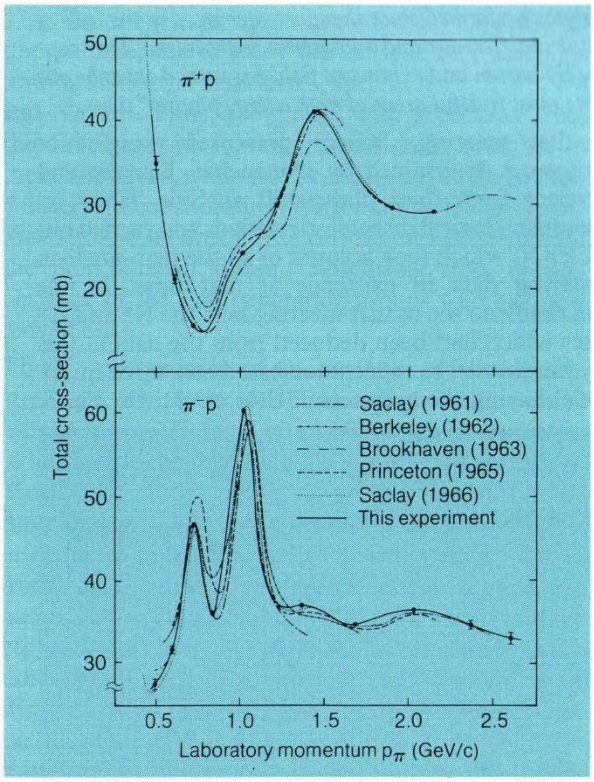


Fig 1. The values of the total cross-sections $\sigma_{tot}(\pi^+p)$ and $\sigma_{tot}(\pi^-p)$ reported by the Birmingham-Cambridge-Rutherford group in 1968 are plotted against laboratory pion momentum p_π, and compared with the cross-sections published earlier by other groups. The error bars are given for a few data points, to indicate how the systematic errors vary with p_π.

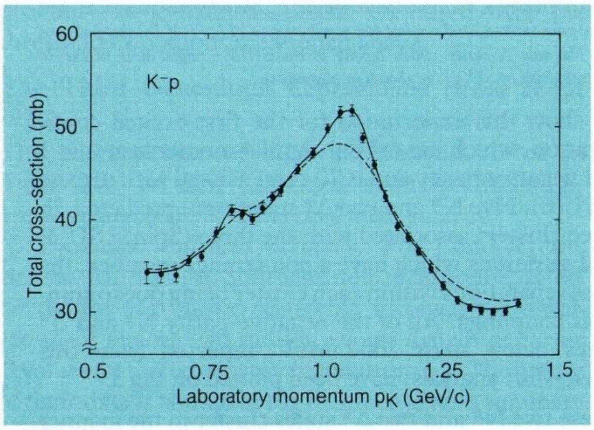


Fig 2. The values obtained for the total cross-section $\sigma_{tot}(K^-p)$ by the Birmingham-Cambridge-Rutherford group for laboratory momenta p_K from 630 to 1410 MeV/c are plotted against p_K. The dashed line denotes the mean cross-section given by earlier K^-p experiments in this momentum range.

Steiner also referred to the work of the University College-Westfield group on the angular distributions for $\pi^\pm p$ scattering.

Further, let me quote from Lovelace, in his address concerning the results of the phase-shift analysis of all these new experimental data on the pion-nucleon system, to which Nimrod's contribution had been dominant. His closing remarks were:

"About 1961, most people wrote off the subject of pion-nucleon scattering in the region of higher resonances as dead. The more accurate measurements and phase-shift

analyses performed since then were achieved in the face of initial indifference and subsequent incredulity. This supposedly barren and exhausted field has turned out to be one of the most fruitful areas of high energy physics".

At that meeting, Steiner presented the list of resonances determined by Donnachie, Kirsopp and Lovelace from their phase-shift analysis, based on phenomenological dispersion relations and carried out at CERN, which took account of all the pion-nucleon scattering data in existence at that time. It was remarkable to see at that meeting how the N* and Δ* states which had been deduced from the data in this way ran parallel to expectation based on a three-quark model having the symmetry SU(6)×O(3). On Fig 3, we show this expectation for the first-excited configuration, which has orbital angular momentum L = 1 and negative parity and is 70-dimensional with respect to SU(6). Five N* and two Δ* states were predicted, in three clusters associated with the three (Spin)×SU(3) configurations which have a non-strange member, the mass separations within each cluster being due to spin-orbit couplings. All of the negative-parity N* and Δ* states below mass 2000 MeV, reported from this phase-shift analysis, have been plotted on Fig 3.

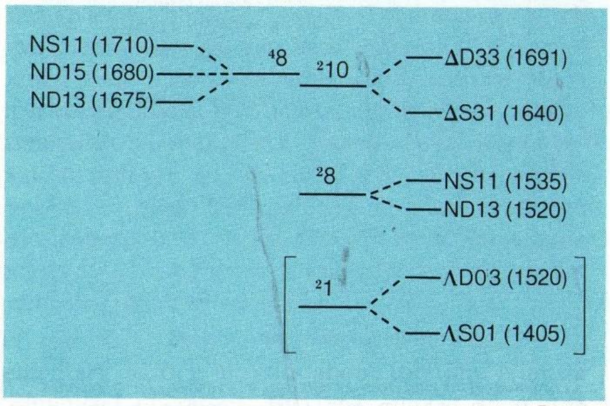


Fig 3. The nine SU(3) multiplets contained within the 70–(L^P = 1$^-$) representation of SU(6)×O(3) symmetry are plotted, indicating the five N* and two Δ* states which belong to them, together with the N* and Δ* mass values determined from experiment, through the CERN phase-shift analysis. In the resonance notation, the capital letter signifies ℓ_π, the orbital angular momentum of the pion emitted, and the last two numbers give 2I (or I, if I is integral) and 2J. The two Λ states which belong to the SU(3)-singlet state in the 70 representation of SU(6) are shown in parenthesis.

These five N* and two Δ* states cluster in the manner expected and they correspond precisely, in spin and isospin, with the expectation for the configuration SU(6)-70 with L^P = 1$^-$. We note that this configuration also predicts a Λ*-doublet, which is SU(3)-singlet and has no N* or Δ* counterpart; these Λ* states, shown in parenthesis on Fig 3, were already known from other work elsewhere.

A similar situation held for the positive-parity N* and Δ* states reported from this phase-shift analysis, as is shown on Fig 4. The doubly-excited three-quark configurations are predicted to have positive parity and include two SU(6)-56 representations, with L^P = 2$^+$ and 0$^+$, respectively; the latter is often referred to as the first radial excitation of the baryon octet and decuplet. The L = 2$^+$ configuration predicts two N* and four Δ* states, occurring in two clusters; the L = $0^{\star+}$ configuration predicts one N* and one Δ* state. The N* and Δ* states reported from this phase-shift analysis, which have positive parity and mass below 2000 MeV, fit these expectations well, except that there was one Δ* state still to be found and two N* states, NF17(1983) and NP11(1751), in excess. (Today, there is still doubt about the existence of the missing ΔP33 state, and the two N* states are assigned to two SU(6)-70 representations with L^P = 2$^+$ and 0$^+$, respectively, which can also occur for a doubly-excited three-quark system).

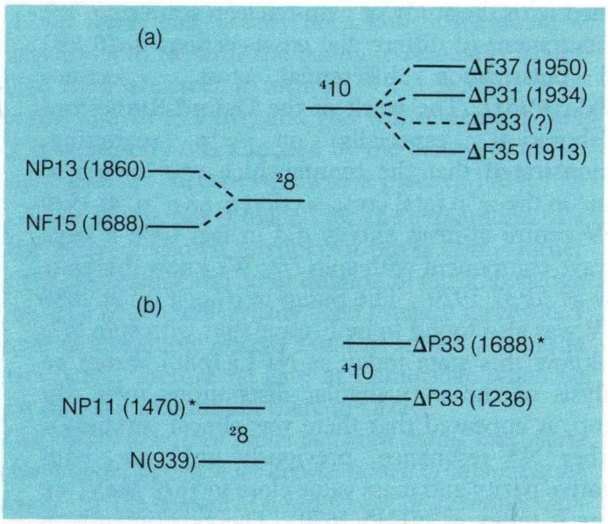


Fig 4. The SU(3) multiplets contained within the 56–(L^P = 2$^+$) and 56–(L^P = 0$^+$)* representations of SU(6)×O(3) symmetry are plotted in Figs (a) and (b) respectively, together with the N* and Δ* mass values determined from experiment, through the CERN phase-shift analysis.

After such an initial success, it was natural for Nimrod's users to turn to the study of the corresponding hyperonic resonances, the Λ* and Σ* states, using K$^-$ beams incident on proton and deuteron targets. We have already mentioned above the early K$^-$p total cross-section data which showed up the ΛD03(1690) state. The corresponding K$^-$d total cross-section measurements are shown on Fig 5, where they are compared with the K$^-$p data.

Survey experiments were carried out using the Saclay 80 cm hydrogen bubble chamber, by a CEN Saclay-Collège de France-Rutherford collaboration for the K$^-$p interaction processes for K$^-$ momenta from 1250 to 1850 MeV/c, and by a Birmingham-Edinburgh-Glasgow-Imperial College collaboration for the K$^-$d interaction processes for the K$^-$ momentum values 1450 and 1650 MeV/c. This K$^-$p work established the spin-parity assignment of 7/2$^+$ for the Σ(2030) resonance and gave the first evidence for the ΣD13(1940) state (now well-established) and the ΣP11(1880) state (still not finally established today). The K$^-$d work confirmed the ΣF17(2030) and ΣF15(1915) states and established the decay mode ΣF17(2030) → πΣP13(1385).

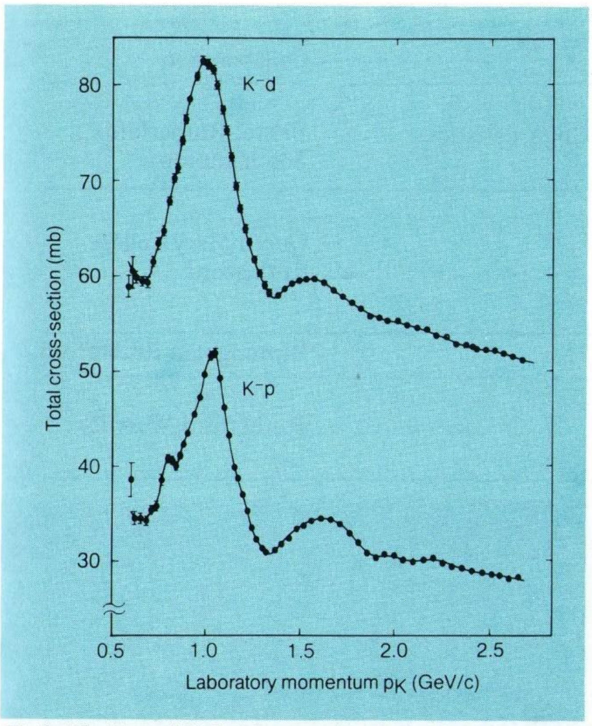


Fig 5. The K^-p and K^-d total cross-sections obtained by the Birmingham-Cambridge-Rutherford group for laboratory momenta p_K from 600 to 2650 MeV/c are plotted and compared.

The Second Generation Experiments (from about 1967)

As listed in Table II, the majority of the second generation experiments were concerned with the $K^{\pm}p$ interactions, on a variety of questions. The Rutherford-University College collaboration made measurements of the $K^{\pm}p$ differential elastic scattering cross-sections over the laboratory momentum range p_K from 1.0 to 2.5 GeV/c, where, as we have seen in Fig 5, there was a great deal of unknown structure found for $\sigma_{tot}(K^-p)$. The Birmingham-Rutherford team concentrated on the momentum range below this, covering p_K values from 1000 to 450 MeV/c, pushing down to momenta where the Coulomb-nuclear interference effects could be observed and measured accurately, to give a new kind of information about the $K^{\pm}p$ hadronic interactions.

The Oxford group made use of their expertise in photon detection to study the inelastic K^-p reactions leading to neutral final states, such as $\Lambda\pi^0$, $\Sigma^0\pi^0$, $\Lambda\eta$ and $\Lambda\pi^0\pi^0$. Thus, the groups with experience from the first generation work turned their attention to these new areas, while new groups, such as the Bristol-Rutherford collaboration, started up with work in the less-demanding area of the $d\sigma/d\Omega$ measurements for the pion-nucleon system, benefitting from the experience of the first generation groups who had now moved on.

The K^+-proton interaction now received considerable attention, owing to the observation of some small bumps in $\sigma_{tot}(E)$ for K^+p and K^+d, which happened to lie close to the threshold for $K\Delta$ or $K^\star N$ excitation. Much painstaking measurement was done to explore the phenomena occurring around these small bumps, in order to determine whether or not they resulted from the occurrence of resonant states (generically named $Z^\star$) in the KN systems. The net outcome of the Nimrod work was that there was no evidence for the existence of an $I = 1$ state $Z_1^\star$ in the mass range studied. Although this conclusion is perhaps less exciting than would have been the discovery of a $Z_1^\star$ state, it is certainly no less important for our understanding of the baryonic states than was the observation of $\Lambda^\star$ and $\Sigma^\star$ resonances in the $\bar{K}N$ system. The origin of the small bumps found in the total K^+p cross-section must be sought elsewhere, most probably as dynamical effects associated with the threshold excitations mentioned above.

The Third Generation Experiments (from about 1970)

The characteristic of these experiments, listed in Table III, is their increased sophistication. The improvements are of different kinds, such as:

(i) *Increased statistical accuracy.* For example, the Bristol-Rutherford-Southampton measurements on $d\sigma/d\Omega$ for $\pi^{\pm}p$ elastic scattering involved the observation of almost two million events distributed over 51 values for the laboratory momentum p_π. The nature of their data is shown in Fig 6, which gives a summary picture of their extremely detailed coverage of pion-

Table II. Some "second generation" experiments at Nimrod.

Experiment	Details	Collaboration
K^+p, K^-p $\quad d\sigma(\theta)/d\Omega$	$p_K = 1000\text{-}2500$ MeV/c	Rutherford-University College
K^+p, K^-p $\quad d\sigma(\theta)/d\Omega$	$p_K = 450\text{-}1000$ MeV/c (including Coulomb- nuclear interference effects)	Birmingham-Rutherford
$K^-p \rightarrow \Lambda\pi^0, \Sigma^0\pi^0, \Lambda\eta, \Lambda\pi^0\pi^0$	$p_K = 865\text{-}990$ MeV/c	Oxford
π^-p $\quad d\sigma(\theta)/d\Omega$	$p_\pi = 1200\text{-}2500$ MeV/c	Bristol-Rutherford

Table III. Some "third generation" experiments at Nimrod.

Experiment	Details	Collaboration
K^+p $d\sigma(\theta)/d\Omega$ $\pi^{\pm}p$ $d\sigma(\theta)/d\Omega$ K^-p $d\sigma(\theta)/d\Omega$	$p_K = 900\text{-}2000$ MeV/c $p_\pi = 400\text{-}2150$ MeV/c (high statistics) $p_K = 950\text{-}2000$ MeV/c	Bristol-Rutherford-Southampton
$K^-p \rightarrow K^-p$ $P(\theta)d\sigma(\theta)/d\Omega$ $K^+n \rightarrow K^+n$ and K^0p $P(\theta)d\sigma(\theta)/d\Omega$	$p_K = 965\text{-}1285$ MeV/c $p_K = 860\text{-}1365$ MeV/c	Queen Mary College-Rutherford
$K^+n \rightarrow K^0p$ $d\sigma(\theta)/d\Omega$	$p_K = 450\text{-}950$ MeV/c	Birmingham-Rutherford
$\pi^-p \rightarrow \pi^0n$ and ηn $d\sigma(\theta)/d\Omega$ and $P(\theta)$	$p_\pi = 600\text{-}4000$ MeV/c	Rutherford-Warwick

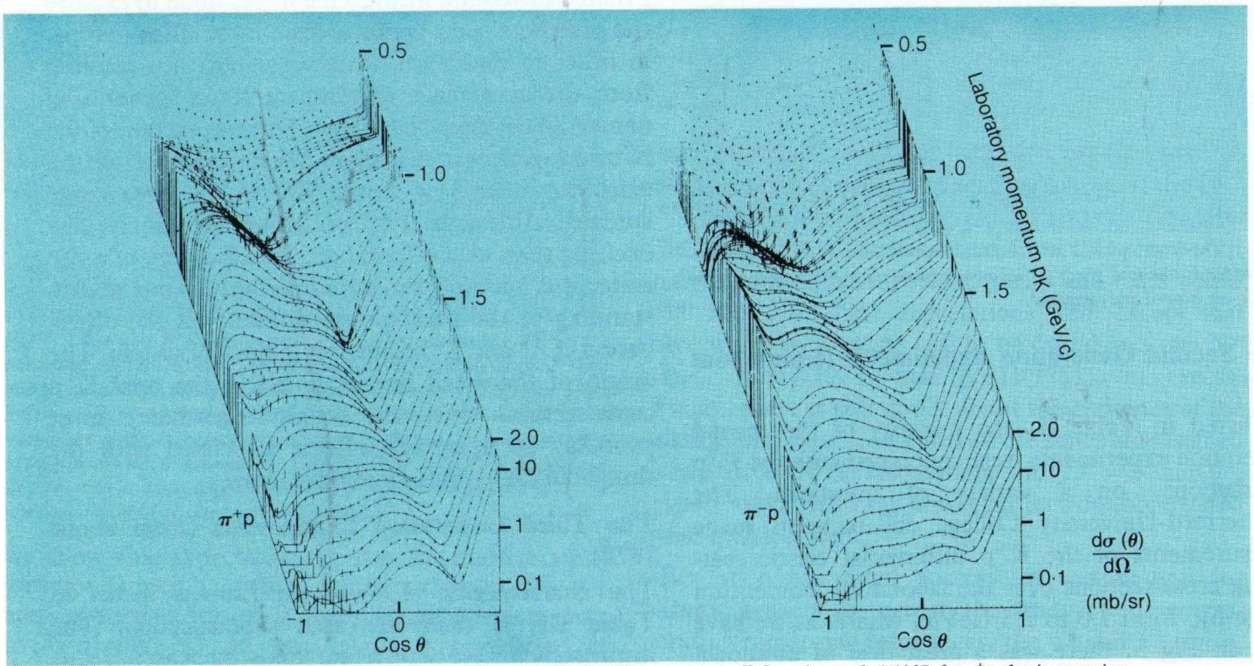


Fig 6. A summary plot of the data obtained by the Bristol-Rutherford-Southampton collaboration on $d\sigma(\theta)/d\Omega$ for $\pi^{\pm}p$ elastic scattering.

nucleon elastic scattering over the whole resonance region.

(ii) *Improved measurement accuracy*. Momentum measurement was improved so that the data could be meaningfully specified over smaller momentum intervals. Beam intensities were improved, and better K/π separation was achieved. Backgrounds and their sources became better understood, and so on. Thus, the data available for analysis became more accurate, more precisely specified and more reliable.

(iii) *Complexity*. Much more work was done with deuterium targets in order to study the $\pi^{\pm}n$ and $K^{\pm}n$ interactions. There was less emphasis on elastic scattering processes, and interest turned to the study of charge-exchange processes and other reactions leading to two-body final states. The use of polarized targets became almost routine, and the design of these targets was considerably developed during this period. For example, a Queen Mary College-Rutherford collaboration has been measuring K^+n differential elastic and charge-exchange scattering using a polarized deuterium target. Their experiment was completed just before the Nimrod shut-down, so there are no physics results available yet. However, a neutron polarization of 30 % was achieved, so that this experiment has been extremely successful in relation to its aims.

One experiment of particular importance for baryonic resonance physics has been the massive study of the polarization differential cross-section for the exchange reaction $\pi^-p \rightarrow \pi^0n$ over the laboratory momentum range from 600 to 2700 MeV/c. Some of these data are shown in Figs 7 and 8. In Fig 7 the polarization data are compared with earlier data from the Lawrence Berkeley Laboratory. The Rutherford-Warwick data are given at closely spaced angles and, even so, have higher statistical accuracy at each data point than did the pre-existing data. In some cases, eg see Fig 7(a), the new data show substantial and systematic differences from the earlier data. In Fig 8, we show a comparison

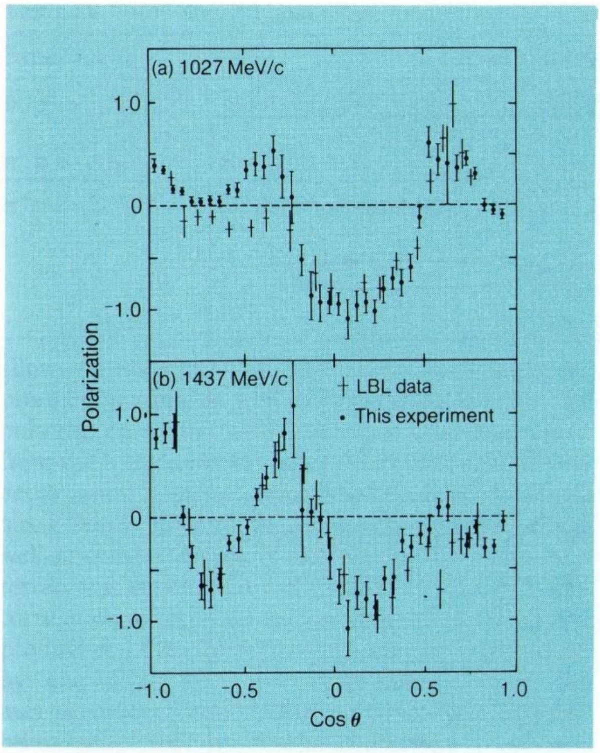


Fig 7. Some $\pi^- p \to \pi^0 n$ polarization angular distribution data obtained by the Rutherford-Warwick Group at Nimrod are compared with the earlier data from experiments at the Lawrence Berkeley Laboratory, for pion momenta p_π values of 1027 and 1437 MeV/c.

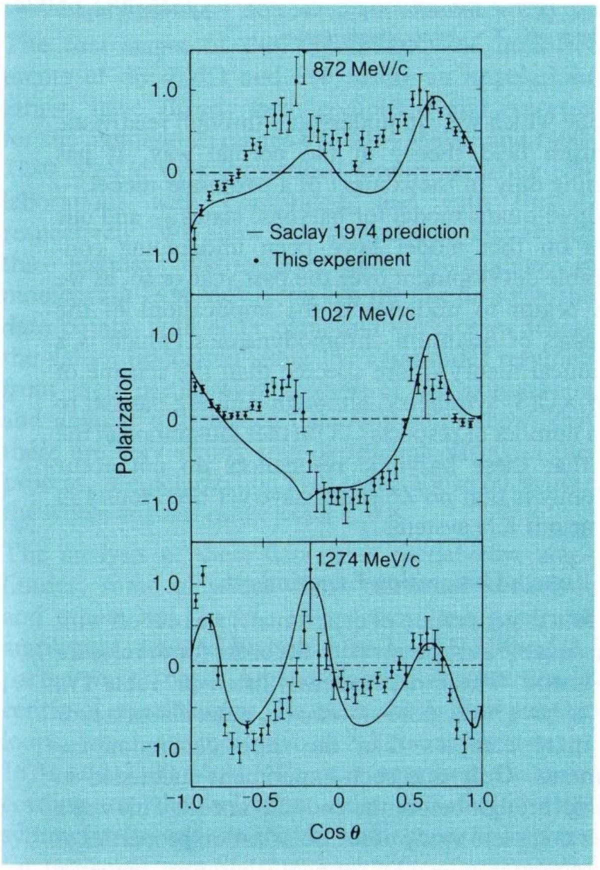


Fig 8. Some $\pi^- p \to \pi^0 n$ polarization angular distribution data obtained with Nimrod are compared with prediction on the basis of the Saclay 1974 phase-shift analysis.

with the polarization predicted using the Saclay 1974 phase-shifts. Their determination did not use any empirical information on the polarization properties of the π^-p charge-exchange process, so that this comparison provides a very significant test of these phase-shifts. We see that the qualitative features of the data are fairly well represented by their use, for example at 872 and 1274 MeV/c, but that the quantitative agreement is generally poor with some quite major discrepancies, for example as at 1027 MeV/c. It is not at all surprising that there should exist such discrepancies between predictions and data for a new experimental quantity. All that I want to point out is that the inclusion of these new data in the input for a new phase-shift analysis will certainly have a significant effect on our knowledge of pion-nucleon resonances.

In this third phase, the contribution of Nimrod to baryon resonance physics has continued to be very great; in pion-nucleon scattering alone, Nimrod has done more than keep up the pace. In 1974, Kelly collected all the available pion-nucleon scattering data for a new phase-shift analysis which he carried out jointly with Cutkosky and his group at Carnegie-Mellon University. In his report on this work to the 1974 "Rochester" Conference, held in London, Kelly remarked that, in his analysis of the relative weights of the various data contributions in determining the final fit achieved, 55 % of this weight came from the data provided by the experimental work with Nimrod. Today, this fraction would be substantially higher, I believe. This is well illustrated by Fig 9. For each kind of pion-proton scattering experiment, an arrow is placed at each centre-of-mass energy value where data are available. The arrows pointing *downwards* indicate the measurements which were performed at Nimrod, while those pointing *upwards* indicate measurements carried out elsewhere. The preponderance of the experimental data from Nimrod is thus made very apparent.

In $\Lambda^\star$ and $\Sigma^\star$ resonance physics, Nimrod has not played such a dominating role as it has for $N^\star$ and $\Delta^\star$ physics, because much more intense $K^\pm$ beams have been available at higher-energy accelerators such as the PS at CERN or the AGS at Brookhaven National Laboratory. Nevertheless, its contribution to KN and $\bar{K}N$ physics will be seen as substantial. For example, the polarization data obtained recently by the Queen Mary College-Rutherford collaboration on the K^+n interactions will have great importance for the outstanding question of the existence of a $Z_0^\star$ resonance, and the high-statistics high-quality data obtained recently by the Bristol-Rutherford-Southampton collaboration on K^-p elastic scattering has provided information important for partial-wave analyses seeking to settle our picture of the $\Lambda^\star$ and $\Sigma^\star$ resonances.

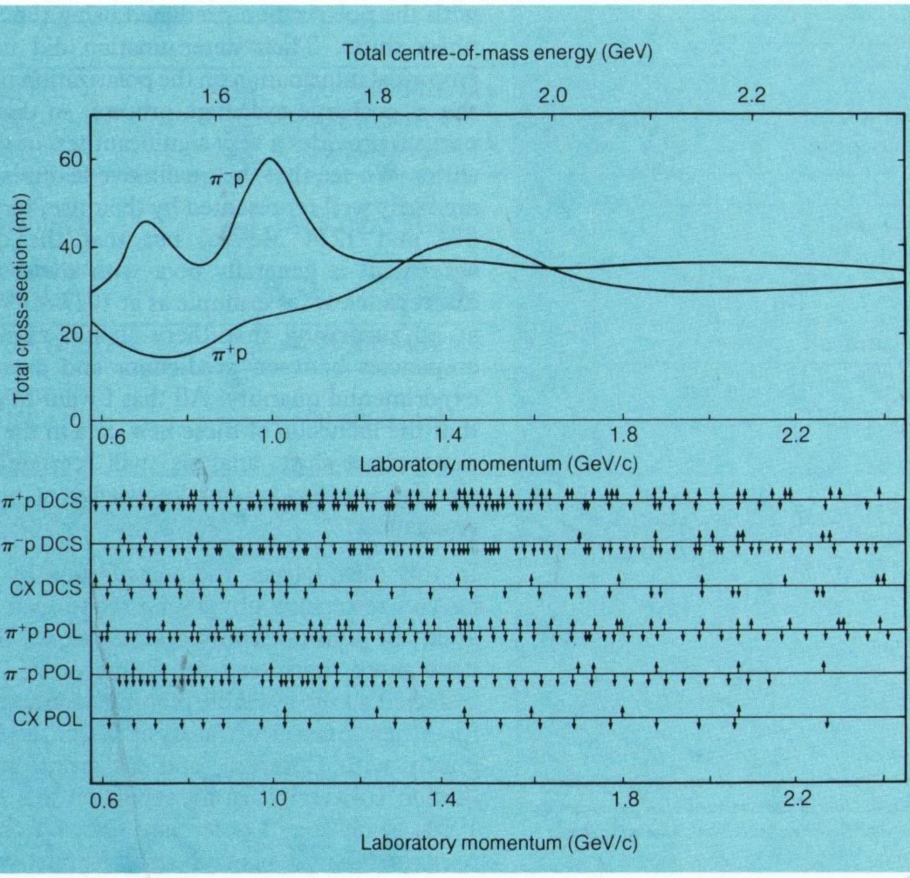


Fig 9. A record of all pion-nucleon scattering experiments in the centre-of-mass energy range from 1450 MeV to 2350 MeV. Down arrows indicate experiments performed with Nimrod, up *arrows indicate experiments performed elsewhere. DCS = differential cross-section, POL = polarization angular distribution and CX = charge exchange reaction.*

The whole story of baryonic resonance phenomena is a very complex one. It has become quite clear in the course of this work with Nimrod that such resonances do not occur randomly — their occurrence in different partial waves or in different channels is rather coherent. This has been expressed in one sense by the notion of *duality* which requires that there should be an equivalence between the description of reaction processes as being due to the transfer of mesonic systems virtually between one particle and another, and the description of these same processes as being due to the formation and decay of resonance states coupled with the ingoing particles and outgoing particles in these two steps. Another cause for correlations between the various resonance states is the existence of the symmetry SU(6)×O(3), mentioned above. If this symmetry were exact, then all of the substates within one particular SU(6)-L^P representation would have the same mass. In practice, there is generally quite strong symmetry-breaking which separates states with different strangeness, or belonging to different SU(3)-multiplets, or with different J, although approximate mass degeneracy still persists for some configurations (eg note the cluster of states NS11(1710), ND13(1675) and ND15(1680) on Fig 3, or the incomplete cluster of states ΔP31(1934), ΔF35(1913) and ΔF37(1950) on Fig 4a). Incidentally, this SU(6)×O(3) symmetry is not one which we can understand through relativistic quantum field theory in the normal way. It has meaning only in the context of a particular model — the three-quark model for baryonic states — and our views on this model have been undergoing considerable development over the past year or so, as we have begun to understand the implications of the successes of quantum chromodynamics (which is a dynamical theory, more specific than the scheme of SU(6)×O(3) symmetry, although running parallel to it in a number of respects). A further illustration of the fact that these baryonic resonances do not occur randomly is that no Z* states have yet been found in the various KN systems.

The Fourth Generation Experiments

The fourth generation of experiments was cut off with unreasonable abruptness with the order for the closure of Nimrod. These experiments, listed in Table IV, were to have been more systematic, more elaborate, or still more complex than the third generation experiments. Only one such experiment succeeded in getting through before the end of Nimrod. This was a rather complete study of the polarization properties of the reaction $\pi^- p \rightarrow \Lambda K^0$ for pion laboratory momenta from threshold (897 MeV/c) up to 2380 MeV/c. This experiment ran using a polarized target, which

Table IV. Some "fourth generation" experiments at Nimrod.

Experiment		Details	Collaboration
$\pi^-p \rightarrow \Lambda K^0, \Sigma^0 K^0$	$d\sigma(\theta)/d\Omega$ and $P(\theta)$,	$p = 897$-2380 MeV/c	Bristol-Cambridge-Rutherford
$\pi^-p \rightarrow \Lambda K^0$	$R(\theta)$ and $A(\theta)$	$p = 1340$-2240 MeV/c	
$\pi^+p \rightarrow \Sigma^+K^+$	(a) $d\sigma(\theta)/d\Omega$ and $P(\theta)$,	$p = 1300$-2500 MeV/c	Edinburgh-Rutherford-Westfield (Rutherford Multiparticle Spectrometer)
	(b) $R(\theta)$ and $A(\theta)$	(No data taken before Nimrod closure: will transfer to CERN)	

allowed measurements for the proton spin lying in the production plane as well as perpendicular to it. The polarization of the final Λ particle was determined from the measurements on its decay products, for the decay mode $\Lambda \rightarrow p\pi^-$, since the angular distribution has a strong dependence on the Λ spin due to the well-known strong parity violation for this mode. As a result, this experiment will provide us with spin-spin correlation effects, as a function of the angle of production, the effects of which may be summarized by the $R(\theta)$ and $A(\theta)$ parameters. Since the measurements using a polarized target have only just been completed, no results concerning these new parameters are available yet. The final products of these experiments will be additional information concerning the N* resonance states in the mass range from 1600 to 2300 MeV.

The first stages of this experiment, the measurements of $d\sigma(\theta)/d\Omega$ and $P(\theta)$ using an unpolarized target, have already been completed and reported for the momentum ranges 897-1334 MeV/c and 1400-2380 MeV/c by a Rutherford Group involving collaborations with Cambridge, and with Bristol, respectively. We show two typical data curves, for the angular distribution and polarization at momentum 1334 MeV/c, on Fig 10. The dots give the data points, with error bars, and the crosses denote the values calculated using the amplitudes resulting from their partial-wave analysis of this polarization and angular distribution data. Fig 10(b) shows that there are very strong spin-dependent effects in this process, the polarization $P(\theta)$ being above 60 % over the whole angular range $|\cos\theta| < 0.8$.

The analysis of these data gives partial-wave amplitudes, some of which show resonance behaviour and others not. In Fig 11 we show one particular amplitude, denoted by ND15 (an $I = 1/2$, $J^P = 5/2^-$ partial wave), on an Argand diagram as a function of centre-of-mass energy. Its path shows two circular loops, which are indicative of N* resonance states at 1670 and 2100 MeV. We may note here that, according to the selection rules for SU(6)×O(3) symmetry (which are for baryons the same as those for the relativistic $SU(6)_W$ symmetry often referred to in this field of work), the process ND15(1670) → ΛK is forbidden. However, we draw attention to the

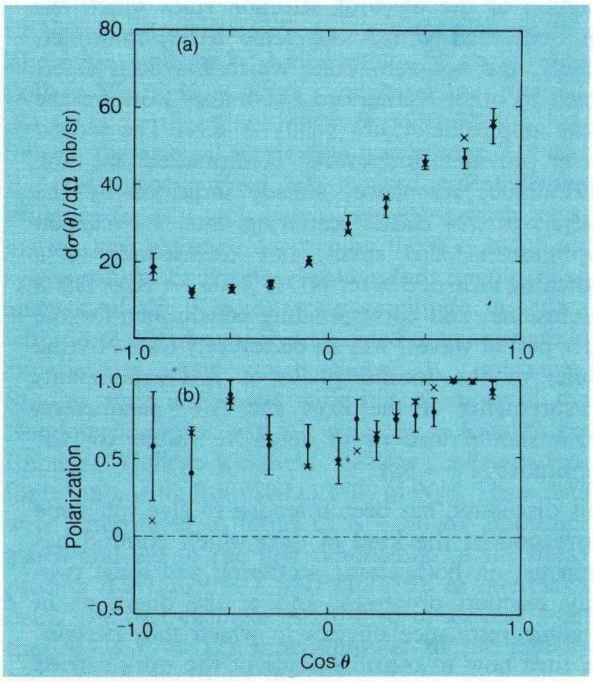


Fig 10. A plot of the angular distribution and polarization data for the reaction $\pi^-p \rightarrow \Lambda K^0$ at a laboratory momentum p_π value of 1334 MeV/c, obtained with Nimrod. The dots specify the experimental values, with error bars; the crosses denote the cross-sections calculated from the partial-wave amplitudes giving the best fit to all of the data at this momentum.

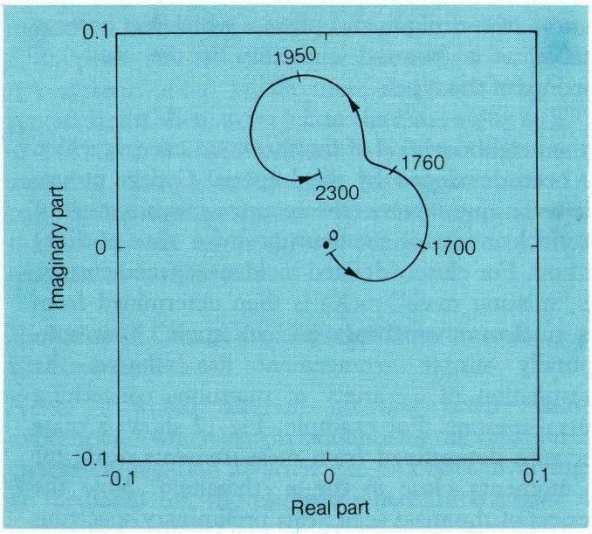


Fig 11. The ND15 amplitude obtained by analysis of the Nimrod data for the reaction $\pi^-p \rightarrow \Lambda K^0$ is plotted on an Argand diagram, as a function of the centre-of-mass energy. The two circular loops correspond to the resonances ND15(1670) and ND15(2100).

scale with which this Argand diagram is drawn; the unitarity circle, within which the amplitude must lie, has a radius of 1.0 with centre 0, so that the amplitude for the decay process ND15(1670) → ΛK is indeed quite small. This experiment determines its branching ratio to be 0.5 %, the full width for ND15(1670) being obtained as 50 MeV (significantly smaller than the value of 155 MeV determined from elastic scattering data). Although the amplitude is small, it does represent a definite violation of the selection rules expected, and this matches another violation of the SU(6)$_W$ selection rules which has been observed within the same SU(3) multiplet, namely, the non-zero value which has been determined by other Rutherford Laboratory work for the decay amplitude ΛD03 (1830) → $\bar{K}N$. The second circle provides interesting confirmation for the ND15(2100) resonance, already indicated by the analysis of πN elastic scattering data. Its reaction amplitude is also small, but corresponds to a branching ratio of 3 % for ND15(2100) → $K\Lambda$. These conclusions, and corresponding conclusions for the other partial waves, will all be severely tested by the results of the second phase of this experiment, measurements of the $R(\theta)$ and $A(\theta)$ parameters, which should lead us to definitive results for the amplitudes $N^\star \rightarrow K\Lambda$.

This discussion has been intended to give you some impression of the kind of data which Nimrod has given us, on both elastic scattering and some two-body reaction processes, and of the questions in baryonic resonance physics to which they pertain. We turn now to consider some of the work carried out with Nimrod in other areas of elementary particle physics.

Threshold Production of Mesons

An area of technique and physics which has become notable as a Nimrod speciality is the study of reactions of the types

$$\pi^- p \rightarrow X^0 n \quad \text{and} \quad \pi^- p \rightarrow X^- p$$

in the neighbourhood of the threshold energy, which has been developed by the Imperial College group. The technique involves the accurate measurement of the nucleon recoil momentum by a time-of-flight method. For closely defined incident π^- momentum, the "missing mass" m(X) is then determined from this nucleon momentum and its angle. This conceptually simple arrangement has allowed the investigation of a variety of questions concerning neutral mesons. For example, Fig 12 shows a mass spectrum determined from measurements made for π^- momenta close to the η' threshold. Note the fineness of the mass scale; this preliminary spectrum already demonstrates that the mass of the η' meson is 957.6 ± 0.3 MeV/c^2 and its width less than 0.8 MeV/c^2. In due course, it is hoped that careful analysis of these experimental data may yield a definite value for the full width $\Gamma(\eta')$.

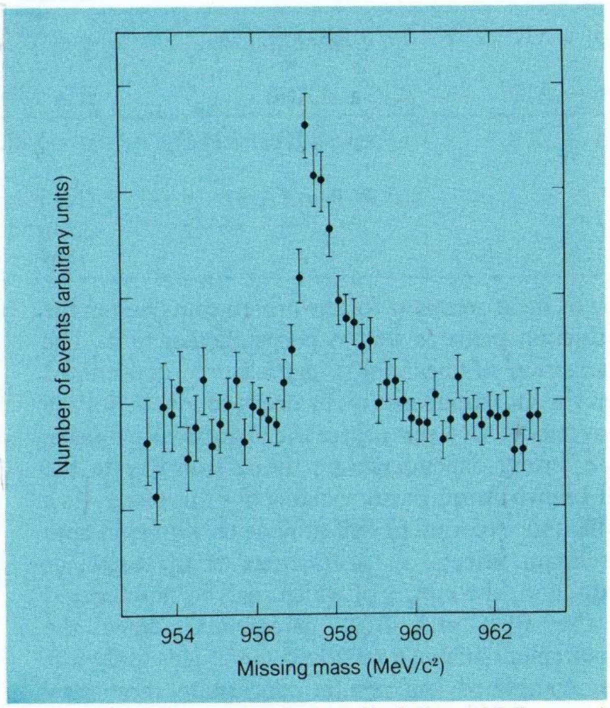


Fig 12. The missing mass spectrum observed by the Imperial College group in the neighbourhood of the η' (958) meson, for the reaction $\pi^- p \rightarrow n +$ "missing mass".

This measurement technique has been particularly productive in the determination of meson production cross-sections near threshold. This has been particularly interesting for η-meson production, where the threshold cross-section is unusually large, and for ω-meson production, where the energy dependence near threshold has been shown to be anomalous, for reasons not yet understood. These experiments have, of course, required a very fine resolution in momentum and very large statistics. The technique has also been extended, by requiring the observation of charged particle pairs in coincidence with the production event, for the demonstration of the existence of a new meson state S*(987) which has decay modes $\pi\pi$ and $K\bar{K}$.

An interesting, related experiment has been the observation by this group of *cusp effects* in $\pi^- p$ elastic scattering at the threshold for the strong η production process $\pi^- p \rightarrow \eta n$. That such cusps should exist in nuclear data as a function of the energy, marking the onset of a new competing channel, was recognized by Wigner and others in the 1930s, but it has proved very difficult to demonstrate their occurrence clearly in low-energy nuclear physics. The data obtained by the Imperial College group on $d\sigma/d\Omega$ for backward $\pi^- p$ elastic scattering are shown on Fig 13 as a function of the incident laboratory momentum p_π. Note the closely spaced data points, all with small statistical error, and the sharpness of the upward peak, centred at the value of p_π appropriate for the

ηn threshold. This is the clearest example of a cusp in any two-body scattering or reaction process to date.

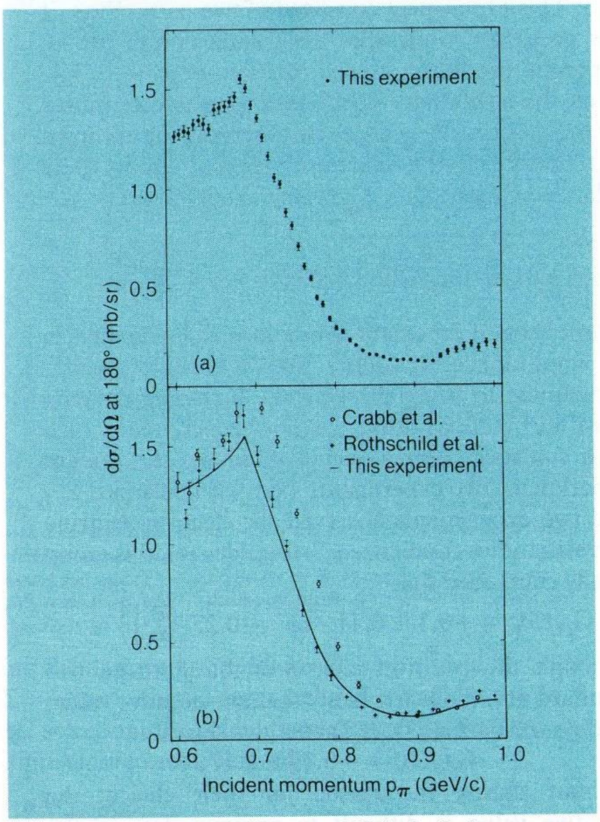


Fig 13. The cross-section $d\sigma/d\Omega$ at $\theta = 180°$ for the elastic process $\pi^-p \rightarrow \pi^-p$ is plotted as a function of incident laboratory momentum p_π in small bins across the threshold for the process $\pi^-p \rightarrow \eta n$. Fig (a) shows the data obtained by the Imperial College group with Nimrod, and (b) compares the Nimrod data (given by the solid line) with the data available from two earlier experiments elsewhere.

Nucleon-Nucleon Interactions

Some of the earliest experiments with Nimrod were concerned with the nucleon-nucleon system. Already in 1964, there was completed a measurement of the differential elastic scattering cross-section for neutrons of momentum 8 GeV/c incident on a hydrogen target, by an AERE-Birmingham-Bristol-Rutherford collaboration, which showed considerable structure for scattering angles near 180°, indicative of the important role played by one-pion-exchange between the proton and neutron in this configuration. Proton-proton and proton-neutron total cross-section measurements were made for incident proton momenta from 1 to 8 GeV/c, using hydrogen and deuterium targets, by a Cambridge-Rutherford collaboration, and were reported in 1965. Their aim had been to measure these cross-sections to an accuracy of ± 0.1 % and this was achieved; some interesting minor structure was found, associated with onset of identifiable inelastic processes. Some of the inelastic processes were studied for proton-proton collisions at 8 GeV/c, by an AERE-Queen Mary College-Rutherford collaboration, by measuring the spectrum of inelastically-scattered protons as a function of the centre-of-mass scattering angle. When these cross-sections were plotted against "missing mass" $m(X^+)$ for the process $pp \rightarrow pX^+$, peaks were clearly seen corresponding to production of the nucleonic resonance states N★(1470) and N★(1688).

The British Hydrogen Chamber

This 1.5 m chamber was used for a number of experiments at Nimrod after its return from CERN in 1967. We mention particularly a series of experiments carried out on K^+p interactions by the CEN Saclay-Imperial College-Westfield College collaboration, and on K^+d interactions by an Imperial College-Westfield College collaboration, for K^+ momenta in the range 2000 to 3000 MeV/c. Although the original motivation was to seek Z_1★ and Z_0★ resonances, these data showed essentially no evidence for any such states. However, much information was obtained concerning the $K\pi$ and $K\pi\pi$ systems in the final states, leading, for example, to an analysis of the $K\pi$ scattering amplitude in accord with quite different analyses carried out elsewhere in experiments using much higher K^+ momenta. Studies were also made with this chamber of the reaction processes $\pi^+p \rightarrow \pi\pi N$ by a Cambridge-Imperial College-Westfield College collaboration, for π^+ momenta from 800 to 1500 MeV/c. These data were interpreted in terms of N★ and Δ★ resonance states and led to the first observation of the decay process $\Delta S31(1650) \rightarrow \pi NP11(1470)$, of interest as being the first example established of a decay from the $70-(L^P = 1^-)$ supermultiplet to the radially excited supermultiplet $56-(L^P = 0^+)$★.

The Heavy Liquid Chamber

This was used for hadronic experiments particularly by the University College group, using incident K^- mesons. Their object was to measure properties of the Ξ^0 hyperon and to look for possible resonances in the $\Lambda\Lambda$ system, about which there had been frequent speculation. Their work led to the mass value $m(\Xi^0)$ of 1315.2 ± 0.9 MeV, which is still the best value available. They studied more than a thousand $\Lambda\Lambda$ pairs in this experiment, but in the end they concluded that there was no positive evidence for any such resonances in their data.

Violation of Charge-Conjugation Invariance in Electromagnetism?

Next, I would like to mention briefly those experiments which sought evidence for the violation of charge-conjugation invariance in processes involving, or mediated by, the electromagnetic interaction. A number of bubble chamber experiments were carried out using the Nimrod beams in the earlier period, but these experiments were superseded by the spark chamber work of the Rutherford-Westfield College collaboration who studied the three η-decay processes:

$$\eta \rightarrow \pi^0 e^+ e^- \qquad (1)$$
$$\eta \rightarrow \pi^0 \pi^+ \pi^- \qquad (2)$$
$$\eta \rightarrow \gamma \pi^+ \pi^- \qquad (3)$$

The first of these processes should not occur if charge-conjugation invariance holds, at least not through one photon exchange. The branching ratio was determined to be $< 1.9 \times 10^{-4}$, relative to the decay process $\eta \rightarrow \pi^0 \pi^+ \pi^-$, which placed a severe upper limit on the C-violation present in this process. The other two decay processes, which produce a $\pi^+\pi^-$ pair together with either π^0 or γ, would generally show an asymmetry between π^+ and π^- emission if charge-conjugation invariance were violated in the electromagnetic interaction. (We should remember here that the decay process $\eta \rightarrow \pi^0\pi^+\pi^-$ strongly violates isospin conservation and is considered to be mediated by virtual electromagnetic interactions). The asymmetries measured for these processes (2) and (3) were $(2.8 \pm 2.6) \times 10^{-3}$ and $(1.2 \pm 0.6) \times 10^{-3}$, respectively. To judge the contribution made by Nimrod, they may be compared with the present asymmetry values of $(1.2 \pm 1.7) \times 10^{-3}$ and $(0.88 \pm 0.40) \times 10^{-3}$ obtained from the total world data to date.

Weak Interaction Processes

Nimrod has not really been a major contributor to this area of elementary particle physics. However, we shall discuss briefly four of the experiments carried out and their significance:

(a) The measurement of the K^+ lifetime. By 1968, there were large discrepancies, far outside the experimental errors, between the lifetime values which had been reported in the literature. A Queen Mary College group proposed an experiment aiming at an accuracy of 0.1 % in order to clarify the situation. Their final lifetime value of 12.380 ± 0.016 nsec compares favourably with the accepted present-day value of 12.37 ± 0.02 nsec.

(b) An early measurement of the branching ratio $(e^+\nu_e)/(\mu^+\nu_\mu)$ for K^+ decay. This was particularly interesting since it concerned the nature of the weak interactions effective in the decay of the K^+ meson. With $e-\mu$ universality, it was interesting that the value obtained, namely $(1.9\,^{+0.7}_{-0.5}) \times 10^{-5}$, was a very small number. It was already known that this ratio was of order 10^{-4} for π^+ decay and that this was a consequence of the axial vector character of the strangeness-conserving hadronic weak current. Since K^+ decay is due to the strangeness-changing hadronic weak current, it was of obvious importance to check this branching ratio for the K^+ meson. This value obtained in 1967 is quite compatible with the present-day value of $(1.54 \pm 0.09) \times 10^{-5}$.

(c) A measurement of polarized Σ^- beta decay, by an AERE-Queen Mary College-Rutherford collaboration. This experiment consisted of two parts. First, it was necessary to measure the polarization of the Σ^- hyperons produced in the reaction $\pi^- p \rightarrow \Sigma^- K^+$. Then the asymmetry coefficient α_e was determined for the process $\Sigma^- \rightarrow n\bar{\nu}_e e^-$ by observing the up-down asymmetry of the electrons emitted from these polarized Σ^- particles. The value obtained was

$$\alpha_e = +0.39\,^{+1.9}_{-0.5}$$

which leads to the value $G_A/G_V = -0.4\,^{+0.5}_{-1.5}$

for the ratio of the axial vector and vector couplings in the amplitude for this decay. This was the first counter experiment to study this process. Including the data from four earlier bubble chamber experiments, each with statistics comparable to those for the present experiment (two experiments were with polarized Σ^-, and two experiments observed the electron-neutrino correlation for unpolarized Σ^- beta decay), this group finally concluded that

$$G_A/G_V = +0.1 \pm 0.11 \quad \text{or} \quad -0.27\,^{+0.13}_{-0.17}$$

although the positive value was found to be two standard deviations less likely than the negative value. Today, the Particle Data Tables give the value

$$G_A/G_V = +0.13 \pm 0.17,$$

without stating the reason for their choice; the negative value is difficult to accommodate in the analysis of all baryon beta-decay data according to the Cabibbo theory, whereas the positive value is acceptable. It is worth mentioning that two subsequent experiments with high statistics have been carried out at Brookhaven and at CERN for unpolarized Σ^- hyperons. These determine only $|G_A/G_V|$, giving together the value 0.39 ± 0.07, which fits best the negative value of G_A/G_V, although not really inconsistent with the positive value given in the Particle Data Tables. Clearly, even now, the matter of the amplitude empirically appropriate to the beta decay of the Σ^- hyperon is far from being satisfactorily settled. The necessary experiments have proved to be very difficult and have not yet been tackled at other accelerators.

(d) The amplitude for $\Sigma^+ \rightarrow p\pi^0$ decay. A knowledge of this amplitude was needed in order to provide an accurate test for the selection rule $\Delta I = 1/2$ for the non-leptonic decay processes for hyperons. The experiment, carried out by a Westfield College-Rutherford collaboration, involved the measurement of the polarization of the final proton for initially polarized Σ^+ hyperons. This leads to the determination of two parameters, denoted by $\alpha(\Sigma_0^+)$ and $\gamma(\Sigma_0^+)$, from which the amplitude can be deduced. This was the first experiment to determine the sign of the parameter $\gamma(\Sigma_0^+)$. The final product of the experiment is the plot shown on Fig 14. The three vectors shown, represent-

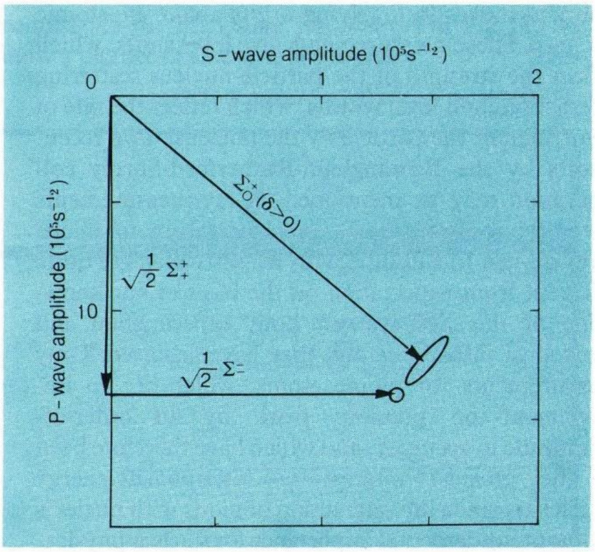


Fig 14. Test of the $\Delta I = 1/2$ rule for $\Sigma \rightarrow N\pi$ decays. With parity violation, each decay amplitude has an S-wave and P-wave component and is therefore plotted as a vector on an (S,P) plane. The $\Delta I = 1/2$ rule requires that the three vectors indicated (Σ^+_+ and Σ^+_0 denote the decay amplitudes for $\Sigma^+ \rightarrow n\pi^+$ and $p\pi^0$, respectively, and Σ^-_- denotes the amplitude for $\Sigma^- \rightarrow n\pi^-$) should form a closed triangle.

ing the decay amplitudes for $\Sigma^+ \rightarrow p\pi^0$ and $n\pi^+$, and $\Sigma^- \rightarrow n\pi^-$, should form a closed triangle if the $\Delta I = 1/2$ selection rule holds; the experimental results come quite close to achieving this.

CP Violation

Another early experiment at Nimrod, with fundamental importance, was the study of the decay process $K^0_L \rightarrow \pi^+\pi^-$ for the long-lived neutral K-meson. The branching ratio measured was in accord with that determined in the pioneering experiment of Fitch and Cronin at Brookhaven. However, the important question at that time was whether or not these observations necessarily implied a failure of CP-invariance at the microscopic level. It was suggested that there might perhaps be some interaction involved on a cosmological scale which acted differently on particle and antiparticle. Such an interaction could induce transitions between the two states K^0_+ and K^0_-, where $K^0_\pm$ denotes the neutral kaon states with CP = ±1, respectively. A beam initially of K^0_- would then gain a component of the K^0_+ state, as time progressed, and the CP-allowed decay $K^0_+ \rightarrow \pi^+\pi^-$ of this component could then account for the observed $\pi^+\pi^-$ pairs. For example, a vector field coupled with strangeness would have this property. If this vector field V_μ were cosmologically determined by the rest of the Universe, then there would be a local inertial frame in which the field is static, ie the space components $\underline{V}$ are zero and the V_0 is time-independent. For neutral K-mesons stationary in this frame, the energy difference required between the K^0 and $\bar{K}^0$ interactions in order to account for the observed rate is very small, being of the order-of-magnitude $\sim 10^{-8}$ eV. However, for neutral K-mesons moving with a velocity βc relative to this frame, the interaction again involves a coupling only with the V'_0 component of this vector field V'_μ, as seen in the rest-frame for these K-mesons. The magnitude of this field V'_0 is dependent on the K-meson velocity, being given by $V'_0 = \gamma V_0$, where $\gamma = (1-\beta^2)^{-1/2}$. As a result, the apparent branching ratio for $K^0_- \rightarrow \pi^+\pi^-$, due to the admixture of K^0_+ state through this interaction, would be expected to vary strongly with K-meson momentum, like $\gamma^2 = 1 + p^2_K/m^2_K$. The particular merit of the Nimrod experiment was that this branching ratio was measured for a number of different K-meson momenta using the same apparatus. The values obtained, as shown in Fig 15, were consistent with the Princeton value and showed no sign of any such dependence upon K-momentum. The conclusion was that the observed $K^0_L \rightarrow \pi^+\pi^-$ process was intrinsic to the K^0_L meson and not the product of its cosmological environment, so that these $K^0_L \rightarrow \pi^+\pi^-$ events were indeed direct evidence for a very small CP-violation in the Weak Interaction.

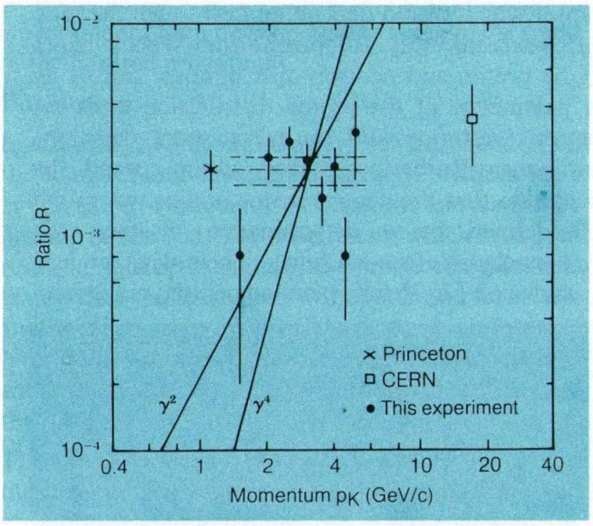


Fig 15. Results from Nimrod of the branching ratio $R = (K^0_L \rightarrow \pi^+\pi^-)/(K^0_L \rightarrow$ all charged modes) are plotted as a function of the K^0_L momentum p_K.

Nuclear and Intermediate Energy Physics

We shall give two examples of the impact of work with Nimrod on nuclear physics:

(i) The accurate measurement of the total cross-sections of nuclei for incident π^+ and π^- mesons, as a function of their momentum p_π, by a Birmingham-Rutherford-Surrey collaboration. These data bear directly on the question of whether or not the distribution of neutrons in a nucleus differs from that for the protons. The accurate measurements which have been made for nuclear size and shape, from the study of the elastic scattering of high-energy electrons on nuclei and from the observations on the energies of the X-rays emitted from mu-mesic atoms, are all sensitive to the charge distribution in the nucleus, which reflects the distribution of the protons within it. For ^{208}Pb, the electron scattering data lead to the value

$R_p = 5.42 \pm 0.07$ fm for the r.m.s. radius of the proton distribution, while the mu-mesic atom data lead to the value $R_p = 5.42 \pm 0.02$ fm, so that these two types of experiment give quite consistent results.

For a long time, people have talked of determining the neutron distribution in a nucleus by using the fact that the π^+p $(=\pi^-n)$ and π^-p $(=\pi^+n)$ reaction cross-sections are often widely different. For example, for $p_\pi = 1$ GeV/c, we have $\sigma(\pi^+p) = \sigma(\pi^-n) = 26$ mb, whereas $\sigma(\pi^-p) = \sigma(\pi^+n) = 60$ mb. If the neutron distribution within a nucleus had a radius R_n greater than R_p, then the π^+-nucleus cross-section would be greater than the π^--nucleus cross-section by an amount which would depend on how much the ratio R_n/R_p exceeds unity. Adopting Saxon-Woods shapes for the proton and neutron distributions, and fixing the parameters of the proton distribution to fit the electron scattering and mu-mesic atom data, the Birmingham-Rutherford-Surrey group found by calculation that the neutron r.m.s. radius was rather well defined by a determination of the ratio $\sigma(\pi^-\text{-nucleus})/\sigma(\pi^+\text{-nucleus})$. Their data for lead are shown on Fig 16 for a pion momentum range from 700 to 2000 MeV/c, where they are compared with several theoretical estimates. In the figure, SW refers to the Saxon-Woods shapes with $R_n = R_p$, ZD are density distributions from a single particle model of the nucleus, and NEG are density distributions from a Hartree-Fock calculation by Negele. Using the Saxon-Woods shape with R_p fixed and R_n free, they obtained the best fit to their data for $R_n = 5.47 \pm 0.07$ fm. This experiment indicated that the neutron distribution within a heavy nucleus must be quite similar to that for the protons.

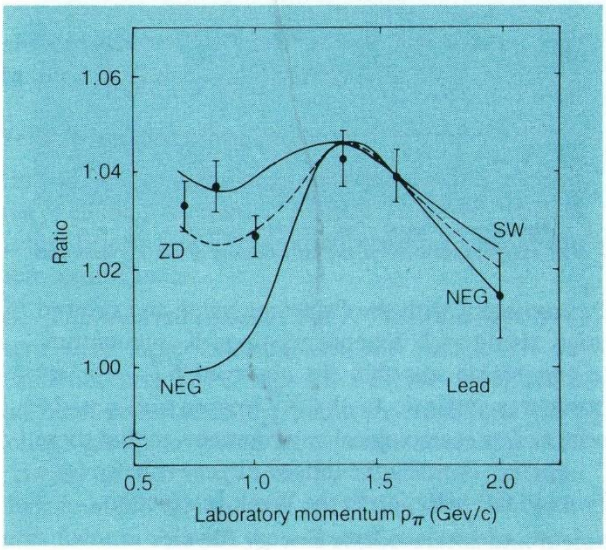


Fig 16. The ratio of π^- to π^+ total cross-sections on lead as determined by the Birmingham-Rutherford-Surrey collaboration is plotted as a function of the laboratory pion momentum p_π, and compared with theoretical calculations, as mentioned in the text.

(ii) The X-rays emitted from *exotic atoms*, in which a negatively-charged heavy particle has replaced an electron and moves in low-lying orbits about the atomic nucleus. These measurements give level shifts, which reflect the strength of the particle-nucleus scattering interaction, and level widths, which reflect the rate of absorption of the particle by the nucleus. Two recent results by the Birmingham-Rutherford-Surrey collaboration may be mentioned. X-ray measurements have been made for the Σ^--nucleus system, for nuclei from oxygen to sulphur, with results which are quite different from expectation on the basis of our knowledge of the ΣN system, both experimental and theoretical. Most recently, they have achieved X-ray measurements for pionic atoms, which are an improvement on previous work by an order-of-magnitude in accuracy, and which have therefore been of the greatest interest to intermediate-energy physicists, since the interaction of pions with nuclei is a topic of fundamental importance for nuclear physics.

The Development of Techniques

The work at Nimrod has also given rise to the development of a number of new techniques of value for experimental high energy physics. The most striking example is that of the track sensitive target for use inside a bubble chamber. This target contains hydrogen, on which the interactions to be studied occur, whereas the surrounding volume of the bubble chamber is filled with a neon-hydrogen mixture, whose higher density gives it a short mean free path for gamma-rays. With this gamma-sensitivity, this system is of particular value for the study of multi-π^0 events.

A track sensitive target (TST) system is shown in Fig 17, which gives a view of the TST container in place within the British 1.5 m bubble chamber vessel. The TST technique was used in three experiments at Nimrod, as follows:

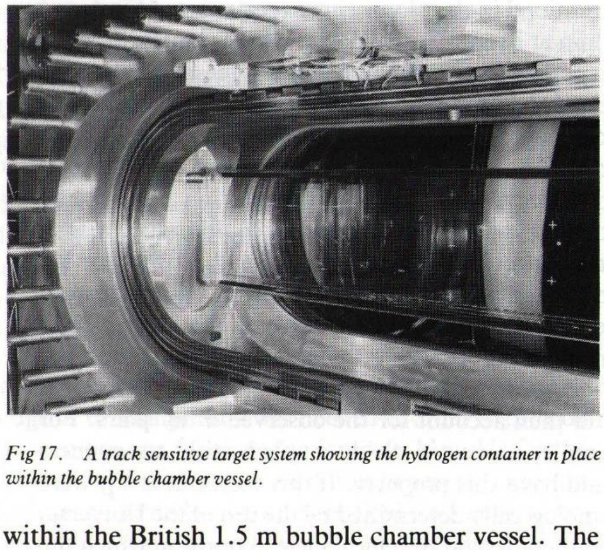


Fig 17. A track sensitive target system showing the hydrogen container in place within the bubble chamber vessel.

(i) A π^+p experiment at 4 GeV/c. This was a test experiment by a Rutherford-Turin collaboration, but it showed the physics possibilities of the technique by providing data of a new kind, on the $\pi^0\pi^0$ interaction, from the observation of fully-fitted events corresponding to the final state $p\pi^+\pi^0\pi^0$. On Fig 18, we show a picture of an event of the type

$$\pi^+p \rightarrow p\pi^-\pi^+\pi^+\pi^0\pi^0.$$

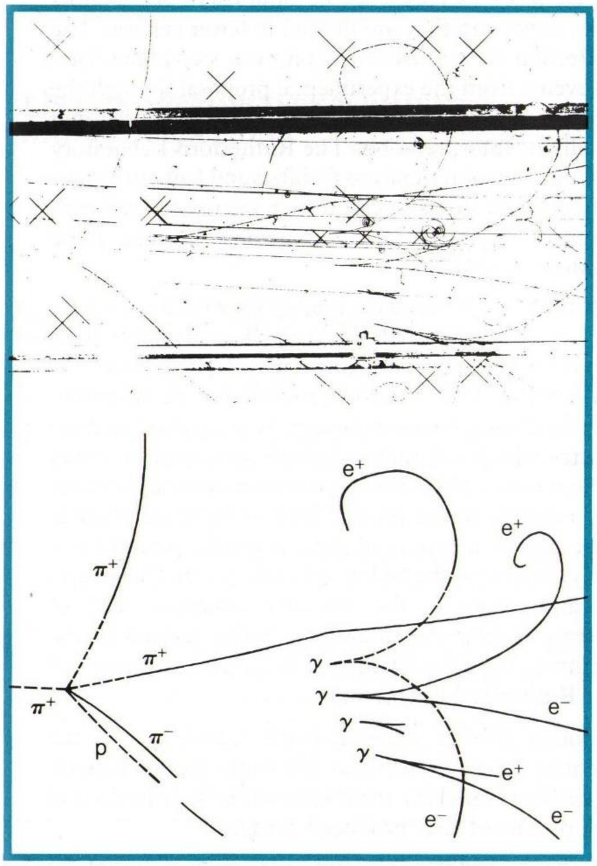


Fig 18. A picture taken using a track sensitive target exposed to a 4 GeV/c π^+ *beam. The interaction event observed took place in the hydrogen cell and is an example of the reaction* $\pi^+p \rightarrow p\pi^-\pi^+\pi^+\pi^0\pi^0$. *All of the final particles are detected, including two* π^0 *mesons each undergoing the decay mode* $\pi^0 \rightarrow \gamma\gamma$. *Three of the final* γ*-rays converted in the neon-hydrogen mixture in the bubble chamber; the fourth* γ*-ray converted within the hydrogen cell.*

Note how the track density increases for each particle as it passes from the hydrogen cell into the neon-hydrogen mixture. The curvature of the charged particle tracks is clearly visible, identifying the charge. All four γ-rays resulting from the two $\pi^0 \rightarrow \gamma\gamma$ decays convert to electron-positron pairs in the chamber — one γ-ray converts within the hydrogen cell — so that all the final particles in this process were made visible by the TST system.

(ii) A $\bar{p}p$ interaction experiment for antiproton laboratory momentum 2 GeV/c, in order to observe the π^0 production and to compare it with the $\pi^\pm$ production already known from other experiments. This analysis is being carried out by groups at the Tata Institute (Bombay) and at Melbourne University.

(iii) K^-p interactions, for a slow K^- beam, brought to stop in the hydrogen cell of the TST system. The particular purpose is to achieve a clean and reliable separation beween the final states $\Lambda\pi^0$, $\Sigma^0\pi^0$ and $\Lambda\pi^0\pi^0$. The analysis is being carried out by a Brussels-Durham-University College-Warsaw collaboration and is still in progress.

The TST technique is now being used at other laboratories, for example in BEBC at the CERN SPS.

Another example of a new technique is the rapid cycling vertex detector which was developed at Nimrod but unfortunately was not perfected in time to complete any experiment with Nimrod. However, these and other devices and techniques which have been developed around Nimrod are already well-booked for work at other accelerators. They would not have been developed at the Rutherford Laboratory without the work with Nimrod as a stimulus, and so we may count their development as being to the credit of the Nimrod programme.

Conclusion

In summary, Nimrod had a very substantial effect on the course of fundamental physics over its fifteen years of operation. However, this is not the last we will be hearing of Nimrod. The experiments which have been running right up to the end have produced a great deal of new data and its analysis will be going on for at least another three years. We shall have many reminders of Nimrod as we hear of Nimrod results being reported and see Nimrod work being published, even beyond that time.

The development of elementary particle physics in these last fifteen years has been quite remarkable and of a quite unexpected nature. After analysing the structure of nuclei, we have moved on to learn that the nucleons themselves have internal structure, a conclusion which derived much of its support from the qualitative fact attested by the Nimrod research that the nucleons had many excited states, which could only be attributed to the excitation of internal degrees of freedom within them. Today we think of all the baryons and mesons, not as simple and fundamental objects, but as composite systems made up of quarks, antiquarks and gluons, although these entities are not yet known outside these baryonic and mesonic states.

When Nimrod was being planned by Gerry Pickavance and his team, did they ever have any inkling, I wonder, that such an extraordinary picture of the so-called *elementary particles* would result from their work, and this within the lifespan of Nimrod?

THE FUTURE OF THE RUTHERFORD LABORATORY

by Dr. G. H. Stafford FRS

It is left to me to bring these proceedings to a close. I would like to do so by saying a few words about the future of the Rutherford Laboratory.

Before that, however, may I thank all of you very warmly for coming along to mark the closure of Nimrod. Incidentally this year marks the 21st anniversary of the formation of the National Institute for Research in Nuclear Science and as this was the organization which was responsible for the creation of the Rutherford Laboratory and of Nimrod it is particularly pleasing that so many of you are present who were involved in the early affairs of NIRNS. There are four Governing Board members present: Sir Harrie Massey who has presided over this gathering, Sir Denys Wilkinson who gave one of the lectures, Sir Brian Flowers and Dr John Adams. In addition there is present Sir Arthur Vick who was chairman of the Nimrod Project Committee and Professor Burcham who was on the Physics Committee. Finally I must not ignore the two past Directors, Sir Alec Merrison and Dr Gerry Pickavance.

Today's lectures were planned to commemorate the closure of Nimrod. I actually pressed the button which switched Nimrod off for good and all on the 6th June (1978). Since then I have been asked by many people whether it was a very sad occasion for me. I must confess that it was an occasion of some sadness but my overriding sentiment is one of gratitude. Gratitude that one had been so fortunate as to participate in the foundation of the Rutherford Laboratory and in the research which over the past 14 years has seen such a tremendous advance in our understanding of the nature of fundamental forces which control the behaviour of matter in the universe.

Turning now to the future; Nimrod has been switched off at a time when discoveries are being made in particle physics which are of very great and fundamental importance. At no time probably has the progress been more rapid. Fortunately the British community of high energy physicists will be able to continue to participate in these great discoveries through the superb facilities available to us at CERN (and at DESY) and I believe the Laboratory will have a continuing and an important role to play in supporting this activity. There is the obvious role in the design and construction of apparatus and in the collection and analysis of the data, but I see real problems ahead for a community which has to rely solely for their research on the accelerators in Geneva and Hamburg and we will need to put our best efforts forward into finding solutions to these problems.

It is inevitable that as the energy of particle accelerators increases so the number of accelerators goes down and they are located in fewer centres. The collection of data is however only one step in the chain of events from the experimental proposal through the preparation of apparatus to the analysis of the data and finally its interpretation. The Rutherford Laboratory has excellent data analysis facilities and I am sure that a key element in a successful high energy physics programme in the future will involve keeping these facilities up to date.

We have laid Nimrod the mighty warrior to rest where he belongs in the earth. I had a look at the Bible to see what it had to say about Nimrod's offspring but it tells us nothing. Our Nimrod certainly has an offspring: the Spallation Neutron Source. It is a pulsed neutron source which will make available slow neutron fluxes 100 or even 1,000 times greater than are available now for research in the general field of materials science. The SNS is a facility of great scientific potential not only because of the high neutron flux available but also because there is the virtually untapped field of research using the muon as a probe instead of the neutron. It is a cornerstone in the future programme of the Rutherford Laboratory.

Another project of comparable significance is the Central Laser Facility, an 800 Gigawatt neodymium glass laser aimed at a study of compression physics and the physics of laser produced plasmas.

By the end of this year there will be in operation at the Laboratory another national facility. I refer to the Electron Beam Lithography Facility which has been set up to support university research into solid state devices. This is a major facility which the SRC is providing to support Engineering Board work. The first was the Interactive Computing Facility and in addition we have a range of other projects including applied superconductivity, the development of rheometers, fluidised bed combustion, helium compressor studies and low-speed alternator development. Finally we are doing our bit to support the Council's energy research programmes.

All the work I have mentioned is done in collaboration with scientists from the universities. In one way or another there are now well over 1,000 university scientists who make use of the Laboratory. This all started with the decision to build Nimrod and to provide central support for the particle physics community in our universities.

It would be unwise to peer into the crystal ball too deeply but it is clear that the future of the Rutherford Laboratory without Nimrod will be very different from what it was with it or what it might have been if EPIC had been built. One thing that is certain is that it will be no less exciting, at least as challenging and I look forward to it with great enthusiasm.

APPENDIX 1

LIST OF APPROVED EXPERIMENTS AND PUBLICATIONS

Proposal Number	**Description and Collaboration**	**Publications**
1	Small angle proton-proton scattering at 7.85 GeV/c. The real part of the p-p scattering amplitude at 7.85 GeV/c was measured by Coulomb-nuclear interference. The ratio of the real to the imaginary part of the amplitude was found to be -0.29 ± 0.03. *AERE Harwell; Queen Mary College, London; Rutherford Laboratory.*	Phys. Lett. **14,** 54 (1965)
1a	Production of nucleon isobars in proton-proton collisions between 2.85 and 7.88 GeV/c. The production of nucleon isobars and dependence of their cross sections on the kinematical variables s and t was investigated using inelastic scattering of 2.85, 4.55, 6.06 and 7.88 GeV/c protons on hydrogen. In addition to the known isobars of masses 1236, 1518 and 1688 MeV, evidence was found for the production of an isobar of mass 1410 ± 15 MeV and width 125 ± 20 MeV. *AERE Harwell; Queen Mary College, London; Rutherford Laboratory.*	Phys. Rev. Lett. **17,** 789 (1966) Nuovo Cimento **63A,** 529 (1969)
2	π^-p polarization near 1 GeV/c. Measurements were made of the asymmetry in the scattering of π^- mesons by a polarized proton target, detecting both the scattered pions and the recoil protons. Data were obtained at 16 scattering angles at each of 8 beam momenta between 875 and 1578 MeV/c. Analysis of these data and earlier differential cross-section measurements showed that there existed at least three resonances in this region, having masses of 1920, 1682 and 1674 MeV/c^2. This experiment also provided accurate differential cross-section data for $\pi^{\pm}p$ elastic scattering over this range of beam momentum. *Oxford University; Rutherford Laboratory.*	Phys. Rev. Lett. **15,** 468 (1965) Proc. Roy. Soc. **A289,** 449 (1966) Phys. Rev. **149,** 1077 (1966) Phys. Rev. **166,** 1448 (1968)
3	Measurement of the differential cross-section for neutron-proton elastic charge exchange scattering at 8 GeV/c over the range $0 < \|t\| < 0.5$ (GeV/c)^2. The forward differential cross-section ($d\sigma/dt$) was 0.93 ± 0.28 mb/(GeV/c)^2. *AERE Harwell; Birmingham University; Bristol University; Rutherford Laboratory.*	Nuovo Cimento **41A,** 167 (1966)
5	$\pi^{\pm}p$ elastic scattering near 2 GeV/c. The cross-sections were measured at 10 incident pion momenta between 1.72 and 2.8 GeV/c. A Legendre polynomial expansion of the differential cross-sections, combined with measurements of π^-p polarization in the same momentum region, provided strong evidence for the J^P assignment of $11/2^+$ for the N^* (2420) and $7/2^-$ for the N^* (2190). *University College, London; Westfield College, London.*	Nuovo Cimento **37,** 110 (1965) Proc. Roy. Soc. **A289,** 509 (1966) Phys. Rev. Lett. **19,** 476 (1967) Nuovo Cimento **52A,** 331 (1967) Phys. Rev. **180,** 1339 (1969)

Proposal Number	Description and Collaboration	Publications
7	π^-p charge exchange scattering near 2 GeV/c. Differential cross-sections were measured in the region of the N* (2190). Using the then-popular model in which the scattering was assumed to be given by the sum of s-channel resonance and t-channel Regge exchange, it was shown that the data were inconsistent with the expected J^P assignment for the N* (2190). The anomaly was later understood in terms of dual models which implied that the resonant contributions were on average contained in the Regge exchange amplitude. An analysis of $\pi^\circ\pi^\circ$ production and other neutral final states was also made with the data from this experiment. *Oxford University; Rutherford Laboratory.*	Nuovo Cimento **39,** 979 (1965) Proc. Roy. Soc. **A289,** 513 (1966) Phys. Rev. Lett. **16,** 288 (1966) Phys. Rev. Lett. Err. **17,** 1274 (1966) Phys. Rev. **156,** 1451 (1967) Phys. Rev. **177,** 2047 (1969)
10	Search for the decay $\omega^0 \rightarrow e^+e^-$. The ω meson was produced in the reaction $\pi^-p \rightarrow \omega n$ close to threshold and was recognised by the characteristic time-of-flight of the associated neutron. Three examples of the decay were found. *Imperial College, London; Manchester University.*	Phys. Lett. **18,** 348 (1965)
11	Study of resonances near threshold for the case of two-body kinematics. Multipion resonances in π^-p scatterings were studied, and the 2π and K^+K^- decays of the f^0 meson. *AERE Harwell; Rutherford Laboratory; Southampton University; University College, London.*	Nuovo Cimento **53A,** 817 (1968) Nucl. Phys. **B48,** 365 (1972)
12	Total cross-section measurements of protons on protons and neutrons were made from 1.1 to 8 GeV/c with an absolute accuracy of ± 0.3% and relative errors between points of ± 0.1%. Structure was observed in the p-p total cross-section near a mass value of 2.75 GeV/c^2. *Cambridge University; Rutherford Laboratory.*	Phys. Rev. Lett. **15,** 214 (1965) Phys. Rev. **146,** 980 (1966) Phys. Lett. **20,** 203 (1966) Phys. Rev. **D1,** 2481 (1970)
13	Study of the leptonic decay modes of K^+ mesons. The branching ratio, positron momentum spectrum and π° energy spectrum were measured for the K^+_{e3} decay mode. *Oxford University.*	Phys. Rev. Lett. **21,** 327 (1968) Phys. Rev. Lett. **21,** 766 (1968) Phys. Rev. **174,** 1661 (1968) Phys. Lett. **31B,** 325 (1970)
13a	The branching ratio for the K_{e2} decay was measured relative to the branching ratio for $K_{\mu 2}$ decay, and found to be $1.9^{+0.7}_{-0.5} \times 10^{-5}$ in good agreement with the prediction of V–A theory. An upper limit was also determined for the branching ratio of the $K_{\pi 2}$ decay mode. *Oxford University.*	Phys. Rev. Lett. **19,** 982 (1967) Phys. Rev. **171,** 1402 (1968) Phys. Rev. Lett. **23,** 393 (1969)
17	Study of the decay modes of the η^0 meson. Data were obtained on η^0 decays and the observation of inverse electroproduction. *Oxford University; University College, London.*	Phys. Lett **24B,** 115 (1967) Phys. Lett. **25B,** 435 (1967) Phys. Lett. **27B,** 402 (1968)
18	A partial wave analysis of two-body final states produced in K^-p interactions was made. This experiment was the first in the series leading to the detailed and definitive partial wave analysis of the Imperial College and Rutherford Laboratory groups. *CEN Saclay; Collège de France; Rutherford Laboratory.*	Nucl. Phys. **B8,** 447 (1968) Nucl. Phys. **B20,** 476 (1970) Nucl. Phys. **B24,** 417 (1970) Nucl. Phys. **B30,** 125 (1971) Nuovo Cimento **7A,** 567 (1972)
—	K^-d scattering from 1.45 to 1.65 GeV/c. *Birmingham University; Edinburgh University; Glasgow University; Imperial College, London.*	Nucl. Phys. **B19,** 61 (1970) Nucl. Phys. **B25,** 75 (1971) Nucl. Phys. **B31,** 61 (1971) Phys. Rev. **D3,** 2603 (1971) Nucl. Phys. **B121,** 365 (1977) Nucl. Phys. **B125,** 61 (1977) Nucl. Phys. **B129,** 253 (1977)

Proposal Number	Description and Collaboration	Publications
18a	This experiment tested the conjecture of T. D. Lee that the CP violation observed in the decay of the K_L° arose from a C violation in the electromagnetic interaction which could thus be observed in η decay. About 1.5 million pictures were taken in the Saclay 80 cm deuterium bubble chamber exposed to a beam of 700 MeV/c π^+ mesons, yielding about 1,500 η mesons in the reaction $\pi^+ d \rightarrow pp\eta$. No C-violating effects were observed. *CEN Saclay; Rutherford Laboratory.*	Phys. Lett. **23,** 600 (1966) Phys. Lett. **24B,** 486 (1967) Nuovo Cimento **58A,** 468 (1968) Phys. Rev. **183,** 1152 (1969)
18b	Measurements of proton-deuteron scattering in the range 1.82 to 2.11 GeV/c provided data on the p-n interaction. The range was chosen to examine the onset of the rise in T=O nucleon-nucleon total cross-section between 1.5 and 3 GeV/c^2. No evidence was found for this rise to be due to N*(1470) production. *Cambridge University.*	Phys. Lett. **26B,** 317 (1968) Phys. Rev. **187,** 1856 (1969)
22	Test of CP violation in the decay $K^\circ_L \rightarrow \pi^+\pi^-$ over a range of K°_L energies. The results confirmed the two pion decay of K°_L, the rate of charged 2π mode to all charged modes of K°_L being $(2.0 \pm 0.3)\ 10^{-3}$, averaged over the K°_L momentum spectrum from 1.5 to 5.0 GeV/c. Furthermore, this rate, as compared with the results at lower and higher energies at other laboratories, shows no sign of any dependence upon kaon-momentum. *AERE Harwell; Bristol University; Rutherford Laboratory.*	Phys. Rev. Lett. **14,** 383 (1965)
23	Polarization measurements were performed in K^-p elastic scattering from a polarized proton target at 8 beam momenta between 1.08 and 1.37 GeV/c. Data were also recorded on the polarization in π^-p elastic scattering at 50 momenta between 0.64 and 2.14 GeV/c. These latter results were obtained at values of $\cos\theta$ ranging from approximately +0.9 to −0.95 in the c.m. system. The data were compared with the predictions of existing phase shift solutions. *Oxford University; Rutherford Laboratory.*	Phys. Rev. **184,** 1443 (1969) Phys. Rev. **184,** 1453 (1969)
24	Nuclear Chemistry. *Birmingham University.*	
25	High accuracy total cross-section measurements for $\pi^\pm$ and $K^\pm$ nucleon scattering from 0.6 to 2.5 GeV/c. The $\pi^\pm$ data on a deuterium target allowed a better understanding of the effects of Fermi motion and screening in deuterium, while the $K^\pm$ data provided evidence for several new resonances. The Y_0^* (1700) was discovered, and evidence for possible Z* states in the K^+p and K^+n cross-sections was confirmed. *Birmingham University; Cambridge University; Rutherford Laboratory.*	Phys. Rev. Lett. **18,** 62 (1967) Phys. Rev. **168,** 1457 (1968) Phys. Rev. **168,** 1466 (1968) Nuovo Cimento **54A,** 608 (1968)
26	Measurement of the electron asymmetry parameter in the decay of polarized Σ^- hyperons. Polarized Σ^- hyperons were produced in π^-p collisions at 1.13 GeV/c. Forty-three events of the rare beta decay of these hyperons were identified. The electron asymmetry parameter was found to be $0.39\,^{+1.9}_{-0.5}$, giving a value for the ratio of axial to vector coupling constants of $G_A/G_V = -0.4\,^{+0.5}_{-1.5}$ *AERE Harwell; Queen Mary College, London; Rutherford Laboratory.*	Nucl. Phys. **B39,** 77 (1972)
27	The polarization of Σ^- hyperons produced in π^-p collisions. The polarized target asymmetry was measured for the interaction $\pi^-p \rightarrow \Sigma^-K^+$ at 1.13 GeV/c using an LMN polarized target. The polarization parameter was found to lie between −0.1 and −0.5 in each of 7 angular bins in the range $-1.0 < \cos\theta^* < 0.3$. *AERE Harwell; Oxford University; Queen Mary College, London; Rutherford Laboratory.*	Phys. Rev. **177,** 2103 (1969)

Proposal Number	Description and Collaboration	Publications
28a	Measurement of the partial width for $\phi \to e^+e^-$. Although the cross-section for ϕ production in $\pi^-p \to \phi n$ was known to be small, clear evidence for the ϕ was found in this reaction very close to the threshold. The partial width to e^+e^- was measured to be $(7.2 \pm 3.9)10^{-4}$. *Imperial College, London; Rutherford Laboratory.*	Phys. Lett. **27B,** 106 (1968)
28b	Eta and S^0 (700) production near threshold. The reaction $\pi^-p \to \eta n$ was studied within a few MeV/c of threshold. The cross-section was found to be dominated by an S-wave and was in support of an ηn resonance a little above the threshold. The η width was found to be less than 0.9 MeV/c^2. No evidence for the S^0(700) was found. *Imperial College, London; Rutherford Laboratory.*	Phys. Lett. **23,** 597 (1966)
29	Nuclear chemistry irradiations. *Orsay Laboratory.*	
30, 74	π^-p and $K^{\pm}p$ elastic scattering between 0.8 and 2.4 GeV/c. The differential cross-section in π^-p elastic scattering was measured at 16 momenta between 1.0 and 1.34 GeV/c, the K^-p cross-section at 32 momenta between 1.09 and 2.47 GeV/c and the K^+p cross-section at 26 momenta between 1.37 and 2.26 GeV/c. The experimental data were used in a variety of energy dependent and energy independent analyses of elastic scattering phase shifts. *Rutherford Laboratory; University College, London.*	Phys. Lett. **32B,** 214 (1970) Nuc. Phys. **B61,** 125 (1973) Nucl. Phys. **B84,** 109 (1975) Nucl. Phys. **B92,** 391 (1975) Nucl. Phys. **B102,** 365 (1976)
31	The polarization of the recoil sigma in the reaction $\pi^+p \to K^+\Sigma^+$ was measured at 12 production angles and a beam momentum of 1.11 GeV/c using counters and spark chambers. The new data were compared with existing phase shift solutions in the low energy region. *Rutherford Laboratory; Sussex University; Westfield College, London.*	Phys. Lett. **39B,** 299 (1972)
33,92	A study of the production of neutral states in K^-p reactions, concentrating on pure isospin final states $\Lambda\pi^0$, $\Sigma^0\pi^0$ and detecting all the gamma rays. *Oxford University.*	Nucl. Phys. **B67,** 125 (1973)
34,47	Coherent 3π production in the 80 cm helium bubble chamber at 1.88 GeV/c. *Oxford University.*	
37	Differential cross-sections for π^-p elastic scattering were measured at 31 momenta from 1.2 to 3.0 GeV/c and a $\cos\theta$ range from 0.97 to −0.98 in the c.m. system. The results were compared with the predictions of existing phase shift analyses from CERN. *Bristol University; Rutherford Laboratory.*	Phys. Lett. **29B,** 584 (1969) Phys. Lett. **31B,** 613 (1970) Nucl. Phys. **B32,** 253 (1971)
38	A K^- beam exposure at 2.3 GeV/c with the propane bubble chamber. Two separate analyses were carried out: one on the lifetimes and other parameters of Ξ hyperons, the second on strange dibaryon states produced in K^- nucleus collisions. Limits were placed on the rate of production of low mass Λp and $\Lambda\Lambda$ resonances. *Brussels University; CERN Laboratory; Tufts University; University College, London.*	Phys. Lett. **33B,** 441 (1970) Phys. Lett. **39B,** 671 (1972) Nucl. Phys. **B47,** 333 (1972) Phys. Lett. **42B,** 372 (1972) Phys. Lett. **57B,** 97 (1975)

Proposal Number	Description and Collaboration	Publications
39, 41	Systematic study of π^+p interactions in the momentum range from 0.6 to 1.1 GeV/c using the Saclay 80 cm hydrogen bubble chamber. Phase shift analyses have been performed on the new data for elastic scattering and various inelastic channels. *Imperial College, London; Oxford University; Westfield College, London.*	Nucl. Phys. **B17**, 331 (1970) Nucl. Phys. **B18**, 29 (1970) Nucl. Phys. **B37**, 133 (1972) Nucl. Phys. **B41**, 91 (1972) Phys. Lett. **55B**, 486 (1975)
40	K^-p differential cross-sections in the range 0.61 to 0.94 GeV/c. Fourteen angular distributions were obtained with good statistics in this energy region which is densely populated with resonances. *Birmingham University; Rutherford Laboratory.*	Nucl. Phys. **B96**, 54 (1975)
43, 43a	Differential cross-section measurements for K^+p elastic scattering at 19 momenta from 0.7 to 1.9 GeV/c. The data consisted of about 20,000 elastic events at each momentum and covered the $\cos\theta$ range from -0.98 to 0.95 in the c.m. system. The results were compared with previous phase shift analyses. *Bristol University; Rutherford Laboratory; Southampton University.*	Phys. Lett. **40B**, 289 (1972) Nucl. Phys. **B131**, 7 (1977)
44	A study of the reaction $\pi^-p \to \pi\pi N$ (chiefly $\pi\pi n$) at incident momenta of 456, 505 and 552 MeV/c using the Saclay 80 cm hydrogen bubble chamber. *Lawrence Berkeley Laboratory ; Oxford University.*	Phys. Rev. **D2**, 1790 (1970)
45	Test of the $\Delta S = \Delta Q$ rule in the decays $K^\circ \to \pi^\pm e^\mp \nu$. Neutral kaons of initial strangeness $S = +1$ were produced via the reaction $\pi^-p \to \Lambda K^0$. The time dependence of the decay rates was studied, from which the amplitude ratio $\chi = [\Delta S=\Delta Q](\text{forbidden}) / [\Delta S=\Delta Q](\text{allowed})$ was calculated. The results obtained were Re $\chi = -0.03 \pm 0.07$ and Im $\chi = 0.09 \pm 0.07$, which is consistent with no violation of the $\Delta S = \Delta Q$ rule. *Cambridge University; Rutherford Laboratory.*	Nucl. Phys. **B66**, 317 (1973)
49	Hyperon-nucleon interactions in the 1.5m hydrogen bubble chamber. Data on the reactions $np \to (\Sigma K)p$ and $np \to (\Lambda K)p$ were obtained by exposing the chamber to a neutron beam. Most events occurred in the neutron momentum range from 4 to 8 GeV/c. The results were used in partial wave analyses of the basic reactions $NN \to (\Sigma K)N$ and $NN \to (\Lambda K)N$ near 6 GeV/c. *Cambridge University.*	Nucl. Phys. **B48**, 225 (1972) Nucl. Phys. **B51**, 488 (1973) Nucl. Phys. **B60**, 157 (1973)
49a	The reaction $np \to pp\pi^-$ was studied by exposing the 1.5m hydrogen bubble chamber to a neutron beam. The 3,500 events were approximately distributed uniformly over the incident momentum range from 1.0 to 7.5 GeV/c. The distributions were found to be in good agreement with the predictions of a one-pion-exchange model. No evidence was found for N*(1450) production. *Cambridge University.*	Nucl. Phys. **B63**, 93 (1973)
50, 99 & 110	Study of narrow bosons produced in π^-p collisions. In these experiments, a meson M was recognised by the missing-mass technique in the reaction $\pi^-p \to M^0n$ (or M^-p). Production cross-sections close to threshold were measured with large acceptance and high resolution. Details of the properties of the mesons were investigated, including the question of whether the proximity of the threshold was affecting the meson parameters, and a search was made for new particles. *Imperial College, London; Southampton University.*	Nucl. Instrum. Meth **77**, 329 (1970) Phys. Lett. **36B**, 257 (1971) Phys. Lett. **36B**, 537 (1971) Phys. Lett. **39B**, 275 (1972) Phys. Rev. **D8**, 2789 (1973) Phys. Rev. Lett. **31**, 1534 (1973) Phys. Rev. Lett. **32**, 392 (1974) Phys. Rev. Lett. **32**, 425 (1974) Phys. Rev. **D12**, 2545 (1975) Phys. Rev. **D14**, 28 (1976)

Proposal Number	Description and Collaboration	Publications
55	Study of the polarization effects in π^+p elastic scattering using a polarized proton target. High precision data were recorded at 64 values of the incident pion momentum over the range from 0.60 to 2.65 GeV/c. *Oxford University; Rutherford Laboratory.*	Nucl. Phys. **B89,** 253 (1975)
56	A study of K^+p and K^+d reactions in the range 2.1 to 2.7 GeV/c using the 1.5m bubble chamber exposed to an electrostatically separated K^+ beam. The exposures have yielded a variety of results, including elastic scattering, charge exchange scattering, the production of individual reaction channels and resonance production. *CEN Saclay; Collège de France; Imperial College, London; Westfield College, London.*	Nucl. Phys. **B14,** 161 (1969) Nucl. Phys. **B36,** 45 (1972) Nucl. Phys. **B37,** 114 (1972) Nucl. Phys. **B42,** 29 (1972) Nucl. Phys. **B97,** 413 (1975) Nucl. Phys. **B99,** 211 (1975) Nucl. Phys. **B105,** 431 (1976)
57	Study of π^+p scattering in the range 1.1 to 1.9 GeV/c. *Imperial College, London; Westfield College, London.*	Nucl. Phys. **B30,** 116 (1971)
60	Measurement of the lifetime of the K^+ meson, which was found to be 12.380 ±0.016 nsec. This was a more accurate result than 2 previous best values which were in disagreement by 4.5 standard deviations with each other. *Queen Mary College, London.*	Phys. Rev. **D3,** 52 (1971)
63	K^+p elastic scattering in the range 0.43 to 0.94 GeV/c. In addition to providing good angular distribution data at 13 momenta, this experiment measured the sign of the Coulomb interference at 0.43 GeV/c unambiguously, thus eliminating a class of phase shift solutions. A phase shift analysis carried out by the group using these data is regarded as a definitive piece of work. *University of Birmingham; Rutherford Laboratory.*	Phys. Rev. **D4,** 2637 (1971) Nucl. Phys. **B66,** 36 (1973)
68	Wide angle proton-proton elastic scattering from 1.3 to 3.0 GeV/c. Differential cross-sections were measured in the centre-of-mass angular range 50° to 90° at 12 incident momenta. Evidence was found for structure in the energy dependence of fixed-angle cross-sections at $/t/\sim 1\ (\mathrm{GeV/c})^2$. *Bergen University; Queen Mary College, London; Rutherford Laboratory.*	Nuovo Cimento **8A,** 447 (1972)
70	Measurement of the charge asymmetry in the decay $\eta \to \pi^+\pi^-\pi^0$. The eta mesons were produced in the process $\pi^-p \to \eta n$. The experiment produced 165,000 three-pion decays, yielding a value of 0.0028 ± 0.0026 for the charge asymmetry which indicates that there is no evidence for C-violation in this decay. The experiment also yielded 35,000 events of the decay $\eta \to \pi^+\pi^-\gamma$. The charge asymmetry was 0.012 ± 0.006, which is consistent with no C-violation in this decay mode. *Rutherford Laboratory; Sussex University; Westfield College, London.*	Phys. Lett. **48B,** 260 (1974) Phys. Lett. **48B,** 265 (1974)
73	$K^\pm n$ elastic scattering and K^+n charge exchange scattering in the range 0.43 to 0.94 GeV/c. This experiment provided a large improvement in the available data on K^+n elastic scattering and charge exchange with angular distributions at 13 momenta. They have been used in phase shift analyses bearing on the Z^* problem. The K^-n scattering data at 14 momenta confirmed a phase shift analysis carried out by an Imperial College, London – Rutherford Laboratory group which had no K^-n data as input. The data thus provided an important check of consistency. *Birmingham University; Rutherford Laboratory.*	Nucl. Instrum. Meth. **104,** 299 (1972) Nucl. Phys. **B94,** 374 (1975) Nucl. Phys. **B129,** 397 (1977)

Proposal Number	**Description and Collaboration**	**Publications**
76	A search for the decay $\eta \to \pi^0 e^+ e^-$ in an optical spark chamber experiment. The observations were consistent with no events being seen and give an upper limit for the rate $(\eta \to \pi^0 e^+ e^-)$/rate $(\eta \to \text{all})$ of less than 4.5×10^{-5} (at 95% confidence limit). The experiment also produced 80 eta Dalitz decays $(\eta \to e^+ e^- \gamma)$ which yielded a value for the electromagnetic form factor of the eta meson and the branching ratio for this decay. *Rutherford Laboratory; Westfield College, London.*	Phys. Lett. **59B,** 99 (1975) Phys. Lett. **59B,** 103 (1975)
77	The Bethe-Heitler theory of pair production by photons and of bremsstrahlung by electrons was compared with observations of these processes in the 1.5m hydrogen bubble chamber. Agreement between theory and experiment was found within the statistical limits of the experiment which were typically about ½% of the total cross-section studied. The experiment allowed limits to be set on the production of new particles of mass less than 100 MeV in high energy hadronic collisions. *Cambridge University.*	Phys. Rev. **D7,** 26 (1973)
78	Investigation of bremsstrahlung anomalies. A systematic search was made, using a Cerenkov counter in a charged particle beam of around 100 MeV/c, looking for the existence of particles with mass intermediate between the electron and the pion. No evidence for these particles was found. *Cambridge University; Rutherford Laboratory.*	
79	Total reaction cross-sections for 0.7 to 2.0 GeV/c $\pi^\pm$ were measured on a range of nuclei from carbon to lead and the radius of the neutron distribution in lead was deduced. *Birmingham University; Rutherford Laboratory; Surrey University.*	Phys. Lett. **41B,** 577 (1972) J. Phys. (Paris) Colloq. **5,** 157 (1972) Nucl. Phys. **A209,** 1 (1973)
81, 101	Measurement of the differential cross-section and polarization in the reactions $\pi^- p \to \pi^0 n$ and $\pi^- p \to \eta^0 n$ in the range 0.6 to 3.5 GeV/c. High precision measurements have been made of the charge-exchange reaction and the $\eta^0 n$ differential cross-section, and the first measurements made of the $\eta^0 n$ polarization in the resonance region. The charge-exchange results represent the most complete set of data in this channel, and provide powerful constraints to partial wave analyses of the πN system. The $\eta^0 n$ results have been used in a partial wave analysis of this channel and the couplings to $I = ½$ nucleon resonances found. *Rutherford Laboratory.*	Nucl. Instrum. Meth. **136,** 307 (1976) Nucl. Phys. **B117,** 12 (1976) Nucl. Phys. **B144,** 287 (1978) (paper in preparation)
83,105	Differential cross-sections for elastic $\pi^+ p$ and $\pi^- p$ scattering. Data were taken at 42 momenta between 0.6 and 2.1 GeV/c, yielding about 20,000 elastic events at each momenta for each beam polarity. These data have not yet been finally published, but they have been used in a preliminary form by the Berkeley Phase Shift Group in their current pion-nucleon analysis. *Bristol University; Rutherford Laboratory; Southampton University.*	(In preparation)
84, 117	Study of $K^- p$ interactions from rest to 580 MeV/c with a track-sensitive target in the 1.5m chamber. Results have been published on the charged Σ production ratios at rest, and a complete survey of all channels up to about 400 MeV/c is being made. A separate analysis above 400 MeV/c is being made by the Durham University – Rutherford Laboratory team. *Birmingham University; Brussels University; Durham University; University College, London; Warsaw.*	Nukleonika **22,** 845 (1977) Nucl. Phys. **B139,** 61 (1978)
85	Neutron-proton interactions from 1 to 3.5 GeV/c. *Cambridge University.*	

Proposal Number	**Description and Collaboration**	**Publications**
86	Study of π^+p interactions from 0.8 to 1.6 GeV/c. *Imperial College, London; Westfield College, London.*	Nucl. Phys. **B41,** 91 (1972)
87	Differential cross-section and polarization measurements up to 1.334 GeV/c on the reactions $\pi^-p\to K^0\Lambda^0$ and $\pi^-p\to K^0\Sigma^0$. The $K\Lambda$ couplings of baryon resonances in the 1600 to 1700 MeV region have been established. *Cambridge University; Rutherford Laboratory.*	Nucl. Phys. **B126,** 365 (1977) Nucl. Phys. **B141,** 29 (1978) Nucl. Phys. **B145,** 402 (1978)
91	Study of 4 GeV/c π^+p interactions using a track sensitive target in the 1.5m hydrogen bubble chamber. The experiments established the track sensitive target technique and studied electron, gamma and π^0 production and the reaction $\pi^+p\to\Delta^{++}\pi^0\pi^0$. *CERN Laboratory; Lawrence Berkeley Laboratory; Rutherford Laboratory; Turin University.*	Nucl. Instrum. Meth. **107,** 399 (1973) Nucl. Instrum. Meth. **114,** 381 (1974) Nucl. Instrum. Meth. **118,** 171 (1974) Nucl. Instrum. Meth. **133,** 29 (1976) Phys. Lett. **66B,** 300 (1977) Nucl. Instrum. Meth. **151,** 89 (1978) Nucl. Phys. **B147,** 28 (1979) Nucl. Phys. **B155,** 320 (1979)
106	Total cross-sections for $\pi^\pm$ on ^{6}Li, ^{7}Li, ^{9}Be, C and O were measured in the energy range from 90 to 860 MeV. The results were analysed using dispersion relations and with the optical model. Coulomb effects were studied and the pion-nucleus coupling constants deduced. *Birmingham University; Rutherford Laboratory; Surrey University.*	Phys. Lett. **43B,** 476 (1973) Phys. Rev. Lett. **31,** 389 (1973) Nucl. Phys. **B67,** 492 (1973) Nucl. Phys. **B76,** 15 (1974) Czech. J. Phys. **B25,** 286 (1975)
112	Investigation of the spin dependent effects in high energy proton-proton interactions at the Rutherford and CERN Laboratory accelerators. The Nimrod experiment involved the measurement of the polarization parameter for large-angle elastic scattering at 7.9 GeV/c. A comparison of these results with the data obtained at other beam momenta shows that the polarization parameter has a strong momentum dependence. *CERN Laboratory; Orsay Laboratory; Oxford University.*	Nucl. Phys. **B121,** 231 (1977) Nucl. Phys. **B125,** 349 (1977)
113	Experiments with stopping kaons. Strong interaction effects in pionic, kaonic and Σ-hyperonic atoms were measured over a wide range of nuclei. The emission of γ-rays following the capture of stopped K^- was studied. *Birmingham University; Rutherford Laboratory; Surrey University.*	Nucl. Instrum. Meth. **130,** 559 (1975) Phys. Lett. **60B,** 355 (1976) Nucl. Instrum. Meth. **137,** 139 (1976) Nucl. Phys. **A260,** 349 (1976) Nucl. Phys. **A282,** 487 (1977) Nucl. Phys. **A296,** 361 (1978) Phys. Rev. Lett. **40,** 931 (1978) Phys. Lett. **74B,** 27 (1978) Phys. Lett. **76B,** 44 (1978) Phys. Lett. **81B,** 165 (1979) Nucl. Phys. **A322,** 445 (1979) Nucl. Phys. **A329,** 407 (1979)
114	Differential cross-section and polarization parameters in the reactions $\pi^-p\to K^0\Lambda^0$ and $\pi^-p\to K^0\Sigma^0$ in the range 1.40 to 2.38 GeV/c. The phase shift analyses of the $K^0\Lambda^0$ final state have provided resonance couplings for $N_{1/2}^*$ resonances which test the quark model of the baryon spectrum. *Bristol University; Rutherford Laboratory.*	Submitted to Nucl. Phys. **B**
115	A study of gamma and π^0 production by 2 GeV/c antiproton-proton interactions in a track sensitive target and to set limits on direct electroproduction. *Melbourne University; Tata Institute, Bombay.*	Nucl. Phys. **B151,** 71 (1979)

Proposal Number	**Description and Collaboration**	**Publications**
119	Study of $S = -2$ baryon resonances using a rapid cycling vertex detector. The experiment was aimed at a high statistics study of Ξ^* states in the mass range 1.5 to 2.1 GeV/c^2 using a triggered rapid cycling hydrogen bubble chamber. Pictures were taken in test situations, however the experiment did not take the final data because of the closure of Nimrod. *CEN Saclay; Collège de France; Durham University; Oxford University; Rome University; Rutherford Laboratory.*	
120	K^-p elastic differential cross-sections in the range 1.02 to 1.95 GeV/c. Data were taken at 23 momenta, yielding about 500,000 triggers and 10,000 elastic events at each setting. The analysis of these data is still in progress. *Bristol University; Rutherford Laboratory; Southampton University.*	
128	Study of elastic scattering and meson production near threshold and a measurement of the width of the X^0 (958). The experiment has investigated the behaviour of the differential scattering cross-section $\pi^-p\rightarrow\pi^-p$ across the threshold for the production of a narrow meson, such as $\pi^-p\rightarrow\eta n$. These studies provide useful information on the production process and it is possible to directly extract the elastic scattering amplitudes. Data were recorded on the width of the X^0 (958) meson, which is expected to be about 0.3 MeV/c^2. The analysis of these data is in progress. *Imperial College, London; Rutherford Laboratory.*	Phys. Lett. **83B,** 141 (1979) Nucl. Phys. **B154,** 503 (1979) (and in preparation)
136	Polarization in K^+n elastic and charge-exchange scattering. The polarized target asymmetry was measured simultaneously for the interactions $K^+n\rightarrow K^+n$ and $K^+n\rightarrow K^0p$ using a deuterated propanediol target, at 5 momenta from 0.86 to 1.365 GeV/c to investigate the possible existence of a Z_0^* resonance. *Queen Mary College, London; Rutherford Laboratory.*	(In preparation)
136a	Polarization in elastic K^-p scattering. The polarized target asymmetry was measured for K^-p elastic scattering over the whole angular range at 9 momenta from 0.965 to 1.285 GeV/c, using a propanediol target. These results, of high statistical accuracy, will be used in a phase shift analysis of the K^-N system. *Queen Mary College, London; Rutherford Laboratory.*	(In preparation)
—	Measurements of the π^- dose-response curves and oxygen enhancement ratios for broad bean roots near the surface, in the plateau, and above the ionization peak. This experiment was performed before the rigors of formal proposals were applied to radiobiology experiments. It showed the expected enhancement at the peak region with a reduced oxygen dependence. *Churchill Hospital, Oxford; Rutherford Laboratory.*	Brit. J. Radiology **46,** 541 (1973) Brit. J. Radiology **47,** 201 (1974)
152	Measurements of π^- dose-response curves for frozen Hela cells at various positions along the depth-dose profiles. These cancer cells were grown and frozen in Glasgow, irradiated on Nimrod and returned for assay. The enhancement at the peak was confirmed in a series of experiments which provided a detailed dose-response at some 15 depths. *Glasgow Institute of Radiotherapeutics and Oncology; Rutherford Laboratory.*	Brit. J. Radiology **47,** 800 (1974) (and in preparation)
153	Measurement of π^- induced chromosome aberrations in human lymphocytes. Whole blood samples were irradiated, the white cells induced to divide and the chromosomes fixed. The response was measured as a function of depth, dose, dose rate, incident pion energy, oxygen tension and split doses. Further, the response to an extended peak obtained by superimposing beams of different energy in succession yielded 1.4 times as many dicentrics over a 7 cm peak region than in the plateau. *National Radiological Protection Board; Rutherford Laboratory.*	Int. J. Radiat. Biol. **27,** 223 (1975) Int. J. Radiat. Biol. **28,** 599 (1975) Brit. J. Radiology **51,** 41 (1978) "Mutagen-Induced Chromosome Damage in Man", Edinburgh Univ. Press, p22 (1978) (and in preparation)

Proposal Number	Description and Collaboration	Publications
154	Relation of π^- beam dosimetry and radiobiological effects in mammolian systems *in vitro* and *in vivo*. Studies were made on cancer cell reproductive integrity as a function of depth, dose and split dose. Mice were used to see effects on *in vivo* systems sensitive enough to respond to the available dose — including lens opacities, bone marrow colony forming units, thymic and testis weight loss, spermatogonial and oxyte survival, life shortening and tumour induction. No simple picture emerged but sensitive systems, in general, did not exhibit the enhancement observed in other biological material. *Medical College of St. Bartholomew's Hospital, London.*	Brit. J. Radiology **49,** 161 (1976) Brit. J. Radiology **49,** 166 (1976) Brit. J. Radiology **49,** 357 (1976) Brit. J. Radiology **50,** 658 (1977) Int. J. Radiat. Biol. **32,** 397 (1977) (and in preparation)
155	Dosimetry experiments to back up biological work with negative pions. Work involved optimising the beam for biological use; measuring the incident particle flux by counter, activation and Monte Carlo techniques, and developing profile monitors; measuring the dose with ion chambers and investigating thermoluminescent and lyoluminescent detectors; measuring the track structure, event size and linear energy transfer spectra at different positions using nuclear emulsions, solid state detectors and proportional counters; and measuring the particles emitted on pion absorption in carbon, oxygen and tissue-like compositions by counter techniques in a vacuum vessel. Design studies of possible future beamlines were also undertaken. *Leeds University; Medical College of St. Bartholomew's Hospital, London; National Radiological Protection Board; Rutherford Laboratory; Surrey University.*	Phys. Med. Biol. **20,** 918 (1975) Phys. Med. Biol. **22,** 451 (1977) Nucl. Instrum. Meth. **153,** 137 (1978) Phys. Med. Biol. **23,** 217 (1978) (and in preparation)
166	A and R polarization parameters in the reaction $\pi^-p \rightarrow K^0\Lambda^0$ between 1.34 and 2.24 GeV/c (7 momenta). Spin rotation measurements resolve the discrete ambiguities in partial wave analyses and provide a stringent test of the conventional baryon resonance scheme. *Rutherford Laboratory.*	
168	An experiment on CP violation in a high magnetic field. It is possible that the CP violating interaction, which is responsible for the decay $K^0_L \rightarrow \pi\pi$, disappears in a sufficiently high magnetic field. Evidence for such an effect has been sought, but none found. *Imperial College, London; Rutherford Laboratory.*	Phys. Lett. **86B,** 405 (1979)
181	Determination of the K^-p scattering length by observing X-rays from kaonic hydrogen. The X-ray spectra from pions and kaons stopping in liquid helium have been measured, and X-rays from kaons stopping in hydrogen have been observed. *Birmingham University; Rutherford Laboratory; Surrey University.*	Phys. Lett. **83B,** 55 (1979) Nucl. Phys. **A326,** 455 (1979) Nucl. Phys. **B160,** 492 (1979)
193	Search for exotic Δ states with a partial wave analysis of $\pi^+p \rightarrow K^+\Sigma^+$. This formation experiment using wire chamber detectors will provide data on the reaction $\pi^+p \rightarrow K^+\Sigma^+$ of considerably greater statistical precision than previous bubble chamber experiments, thus allowing a significant partial wave analysis to be performed. The channel in principle gives a clear signal for 5-quark (exotic) Δ states, which will be searched for, and should also provide a considerable amount of new information on 3-quark Δ states for comparison with models of the structure of the baryons. Data taking was completed in April 1978 and the analysis is in progress. *Edinburgh University; Rutherford Laboratory; Westfield College, London.*	(In preparation)

APPENDIX 2

LIST OF POSTGRADUATE THESES SUBMITTED FOR WORK PERFORMED ON NIMROD

1964

W Busza
PhD, University of London
The development and use of spark chambers for the study of meson production

A R Farqui
PhD, University of London
The design and use of spark chambers to distinguish energetic electrons from pions

1965

W S Chapman
PhD, University of London
Small angle elastic proton-proton scattering at 8 GeV

C J S Damerell
DPhil, University of Oxford
A study of the interactions of elementary particles

M R Jane
PhD, University of London
The electron-positron decay mode of the ω *meson*

1966

T F Buckley
PhD, University of London
The interactions of negative pions with protons at 2 GeV

R F George
PhD, University of Cambridge
Nucleon-nucleon total cross-sections

W G Jones
PhD, University of London
Eta and S° *meson production near threshold*

D B Scott
PhD, University of London
Production of nucleon isobars in proton-proton collisions

1967

R M Brown
DPhil, University of Oxford
Measurement of the decay rate of the K^+ *meson into an electron and a neutrino*

D C Brunt
PhD, University of Cambridge
High energy proton-deuteron interactions

A A Carter
PhD, University of Cambridge
Total cross-sections and forward dispersion relations

D G Crabb
PhD, University of Southampton
An on-line sonic spark chamber experiment to measure the reaction $\pi^- p \rightarrow n\pi\pi$

R J Ott
PhD, University of Southampton
Investigation of resonance production in $\pi^- p \rightarrow \pi^+\pi^- n$ *at 3.2 and 3.5 GeV/c using a sonic spark chamber/scintillation counter on-line computing system*

T W Quirk
DPhil, University of Oxford
Energy spectrum of electrons produced in Ke_3 *decay and related topics*

R J Tapper
PhD, University of Cambridge
High energy total cross-section measurements

1968

M J Clayton
PhD, University of Cambridge
Collisions of fast protons with deuterons

C R Cox
DPhil, University of Oxford
The measurement of polarization effects in the elastic scattering of K^- *mesons by protons*

R W Dobinson
PhD, University of London
Study of the $N^*_{5/2}(2420)$ *and a design study for measuring* Σ *decay parameters*

R C Field
DPhil, University of Oxford
Studies in elementary particle physics by electronic techniques

K S Heard
DPhil, University of Oxford
A study of polarization effects in $\pi^- p$ *elastic scattering*

P J Litchfield
DPhil, University of Oxford
A study of η *mesons produced in* π^+ *interactions with deuterium*

D C Mason
PhD, University of London
Spark chamber analysis of di-kaon production near threshold

M C Miller
PhD, University of London
A study of polarization effects in the reaction $\pi^- p \rightarrow \Sigma^- K^+$ *at 1 GeV*

A A Owen
PhD, University of London
Eta meson decays

V J Smith
PhD, University of Bristol
Pion-proton elastic scattering

J A Strong
PhD, University of London
The elastic scattering of pions by protons in the region of 2 GeV/c

I Ur Rahman
PhD, University of London
Time-of-flight study of the reaction $\pi^- p \rightarrow nK^+K^-$

1969

D R Botterill
DPhil, University of Oxford
Form factor measurements in K^+ *leptonic decays*

E Fleming-Tompa
PhD, University of London
Eta meson decay and electron measurements in a heavy liquid bubble chamber

J P Horsey
PhD, University of London
The electron-positron decay mode of the π meson

E N Mgbenu
PhD, University of London
The development of a wire spark chamber system for studying the elastic scattering of high energy particles

K M Potter
PhD, University of London
The use of visual spark chambers in the detection of neutrino interactions and the study of pion-proton elastic scattering

R A Rosner
PhD, University of London
The automatic acquisition and analysis of high energy elastic scattering data

D H Saxon
DPhil , University of Oxford
A study of elementary particle interactions using the bubble chamber technique

S J Sharrock
PhD, University of London
A computer-controlled kaon-proton scattering experiment

T P Swetman
PhD, University of London
High energy meson-proton elastic scattering

D L Ward
PhD, University of Bristol
A measurement of the differential cross-section for π^-p elastic scattering at 31 momenta between 1.2 and 3.0 GeV/c

E M Wilkinson
DPhil, University of Oxford
Coherent three-pion production in helium at 1.88 GeV/c

1970

R E Ansorge
PhD, University of Cambridge
A study of electromagnetic processes in a hydrogen bubble chamber

P C Barber
PhD, University of London
The elastic scattering of K mesons on protons and the capture of K mesons in heavy nuclei

R J Ellis
DPhil, University of Oxford
A study of the sigma hyperon decay

F D Fuchs
PhD, University of London
Kaon-nucleon interactions in the $T = 1$ state at 1.6 GeV/c incident momentum

G P Gopal
PhD, University of London
A study of the excitation of nucleon resonances in π^+p interactions

D Maden
PhD , University of Cambridge
The reaction $np \rightarrow pp\pi^-$ below 7.5 GeV/c

D Pierce
PhD, University of London
A study of kaon-deuteron interactions at low energy

A D Smith
PhD, University of London
A magnetostrictive readout system for wire spark chambers and the elastic scattering of positive kaons on protons

J G Wilson
PhD, University of London
A search for the A_2 meson near threshold

1971

C J Batty
DSc, University of Birmingham
Published work

D F Baxter
DPhil, University of Oxford
A study of resonances in the K-p system

B J Charles
PhD, University of Bristol
Differential cross-sections for K^+p elastic scattering

J A Charlesworth
PhD , University of Cambridge
Strange particle production in neutron-proton interactions

J F Crawford
PhD, University of London
On the non-leptonic decay of the Σ^+ hyperon

J V Guy
PhD, University of London
A study of hyperons produced by 2.2 GeV/c K^- mesons

J C Hart
PhD, University of Cambridge
An experiment to study the leptonic decay of neutral K-mesons

R J W Howells
PhD, University of Birmingham
Kaon-proton elastic scattering below 1 GeV/c

G Hughes
DPhil, University of Oxford
The formation and decay of neutral baryon resonances in K^-p interactions

C M S Jones
DPhil, University of Oxford
The formation of neutral baryon resonances of strangeness $S = -1$

A E S Krzesinski
PhD University of Cambridge
A study of electromagnetic processes in a hydrogen bubble chamber

R Maybury
DPhil, University of Oxford
A study of baryon resonances of hypercharge $Y = 0$

B K Penney
PhD, University of London
An automatic measuring machine for bubble chamber film and its use for a K^+p experiment

P R Pitts
MSc, University of London
Spark chambers, multiwire proportional chambers and streamer chambers in high energy physics

V Tayler
PhD, University of London
A study of the π^+p interactions around 1 GeV/c and a partial wave analysis of a three-body channel

G Thompson
PhD, University of London
Single pion production in intermediate energy range K^+p reactions

D W Townsend
PhD, University of London
π^+p elastic and inelastic interactions around 1 GeV/c

P N Upadhyay
PhD, University of London
A search for the $\delta^-(962)$ meson

D T Williams
PhD, University of London
Proton-proton elastic scattering at wide angles and intermediate energies

1972

R D Baker
PhD , University of Cambridge
Low energy $\pi\pi$ and πN interactions

I D Buckingham
DPhil, University of Oxford
A spark chamber study of $K^-p \rightarrow$ neutral final state from 0.69 to 1.0 GeV/c

C M Hughes
PhD, University of Bristol
π^+p elastic scattering differential cross-section for 18 pion laboratory momenta between 0.8 and 1.6 GeV/c

J S Hutton
PhD , University of Cambridge
An experimental test of the weak interaction selection rule $\Delta S = \Delta Q$

A K M A Islam
PhD, University of London
Single pion production in intermediate energy K^+d interactions

M E Kay
PhD, University of London
A decay detection system and its applications to the omega meson

M M Lewis
PhD, University of London
A study of the A_2 meson through its production threshold

P H Lewis
PhD, University of London
A study of multibody final states in K^+p reactions between 2 and 3 GeV/c

J F Martin
DPhil, University of Oxford
A study of polarization effects in π^+p elastic scattering

B McCartney
PhD, University of Bristol
Elastic scattering of positive kaons by protons

P R Norton
DPhil, University of Oxford
A study of neutral Y^* resonance formation

R S Orr
PhD, University of London
K^+n charge exchange scattering

P H Sharp
PhD, University of London
An experimental study of weak interactions using Λ^0 and K^0 decays

I Siotis
PhD, University of London
A_2 meson production at low momentum transfer

J C Sleeman
DPhil, University of Oxford
An experiment to study polarization effects in π^+p elastic scattering in the momentum range 600 to 2700 MeV/c

J W Stark
PhD, University of London
Isobar production in low energy π^+p interactions and the use of a small computer in the control of measuring machines

B C Stewart
PhD, University of London
Two and three pion production in a K^+p experiment between 2.11 and 2.72 GeV/c

R P Vickery
PhD, University of London
A measurement of decay parameters of the Σ^+ hyperon

1973

T Azemoon
PhD, University of London
The lifetime of the neutral cascade hyperon and other properties of the hyperons

T A Broome
PhD, University of London
Pion-proton elastic differential cross-sections at pion momenta near 1 GeV/c

N C Debenham
PhD, University of London
A high resolution study of backward π^-p reactions between 0.6 and 1.0 GeV/c

A Duane
PhD, University of London
A measurement of the width of the X^0 (958) meson

T R M Edwards
PhD, University of Bristol
Elastic scattering of positive pions by protons

J Keyne
PhD, University of London
A study of the ω-meson near its production threshold

J A Kirkby
PhD, University of London
A search for the C-violating decay mode $\eta\rightarrow\pi^0e^+e^-$

G May
PhD, University of London
A study of K-N, K-π and K-d interactions at intermediate energies

T A Montgomery
DPhil, University of Sussex
A measurement of the decay $\Sigma^+\rightarrow p\pi^0$ as a test of the $\Delta I = \frac{1}{2}$ rule

D J Pittuck
PhD , University of London
Double pion production in neutron-proton interactions below 3 GeV/c

C M Solomonides
DPhil, University of Sussex
Experimental measurement of the charge asymmetry in the decay $\eta\rightarrow\pi^+\pi^-\pi^0$

F J Wickens
PhD, University of Birmingham
An experiment to study the scattering of kaons from deuterium below 1 GeV/c

1974

K R Bentley
PhD, University of Birmingham
An experiment to study the elastic scattering of kaons from neutrons in the momentum range 0.4 to 0.9 GeV/c

G Hall
PhD, University of London
Double pion production in K^+d reactions between 2 and 3 GeV/c

N J D Jacobs
PhD, University of London
A study of the two- and three-body final states in K^+p interactions between 2 and 3 GeV/c

R Stevens
PhD, University of London
Partial wave analysis of $\pi\pi N$ final states from π^+p interactions around 1 GeV/c

G K Turner
PhD, University of Surrey
Pion-nucleus total cross-sections from 86 to 860 MeV

1975

S Banerjee
PhD, University of London
A study of K^+d interactions between 2 and 3 GeV/c

V J Rajaratnam
PhD, University of Surrey
Pion reaction cross-sections and nuclear sizes

P E Strickland
PhD, University of Cambridge
Associated production near threshold

1976

J N Bunch
DPhil, University of Oxford
Particle identification techniques and studies of particles using them

J Carr
PhD, University of London
Search for neutral mesons with masses near 1 GeV/c^2

A G Perris
PhD, University of London
Energy spectra of charged particles emitted at the capture of negative pions in elements and materials of relevance to radiobiology and radiotherapy

D R Perry
PhD, University of Surrey
Physical aspects of a radiobiological pion beam

H N Sarma
PhD, University of London
A cusp in pion-proton elastic scattering

1977

M M Abdulla
MSc, University of Durham
The production and decay of Σ hyperons by kaons at rest

G P Fleming
PhD, University of Durham
Production of $Y^\star$ resonances by low energy K^- mesons

A C McPherson
PhD, University of Bristol
A measurement of $\pi^\pm p$ elastic differential cross-sections for incident pion laboratory momenta between 0.4 and 2.2 GeV/c

P G Moissidis
PhD, University of London
Study of pion-proton elastic scattering in the region of the X^0 threshold

R W Pleming
DPhil, University of Oxford
The application of electronic techniques to high energy particle detection

R J Purrott
PhD, University of Reading
The effect of radiation dose rate and fractionation on the induction of chromosome aberrations in human lymphocytes

1978

M T Fallahi
PhD, University of Durham
Cross-sections for neutral channels in K^-p interactions between 1470 and 1560 MeV c.m. energy

Y A Hamam
PhD, University of Durham
Cross-sections for charged channels in K^-p interactions between 1470 an 1560 MeV c.m. energy

H Karami
PhD, University of London
The problem of the ω meson near its production threshold

W A C Mier-Jedrzejowicz
PhD, University of London
Four-variable partial wave analysis of π^+p single pion production channels from threshold to 1700 MeV

R J Nowak
PhD, University of London
The charged Σ hyperon production by K^-p interactions at rest

1979

M Gay
PhD, University of Bristol
Associated production in the secondary resonance region.

D Brenner
PhD, University of Surrey
Pion interactions with light nuclei and applications to radiotherapy

J Harvey
PhD, University of Southampton
Differential cross-sections for the elastic scattering of negative kaons by protons

C A Lewis
PhD, University of Leeds
An investigation of the linear energy transfer distribution in a beam of negative pi-mesons