NINA SCRAPBOOK

Kevin Connell

'NINA SCRAPBOOK'

A collection of talks given at the
10th Birthday Celebrations of NINA
on
22nd December 1976
and at the Closing Ceremony
on
1st April 1977

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FOREWORD

The tenth anniversary of the first accelerated beam in NINA, the 5 GeV electron synchrotron at the Daresbury Laboratory of the Science Research Council, was celebrated in December 1976 with an afternoon of lectures by scientists who had played a leading role in the project. For those who attended the lectures and for many others associated with NINA it was felt valuable to reproduce them in a more permanent form. They also form an interesting record of events in the story of high energy physics in this country as seen by the lecturers who are responsible for the written versions presented here.

On 1st April 1977 NINA was closed after ten years of research in particle physics and five years of research with synchrotron radiation. At an informal open ceremony in the Experimental Hall speeches were made by staff members of the Laboratory and university users which are recorded here. During the period of refreshment which followed there were many reminiscences of personalities and events connected with NINA. These of course are, perhaps fortunately, not available for the record.

A. Ashmore

INTRODUCTION

Professor J.R. Holt, FRS
Department of Physics, University of Liverpool

We are here today to celebrate the tenth anniversary of the first accelerated beam in the electron synchrotron NINA. This was obtained on the 2nd December 1966, although the full energy was not reached on that day. However, on the following day, the design energy of 4 GeV was exceeded. A beam current of 10 μA was obtained on that occasion and the machine really got off to a very good start. This was just 4½ years after financial approval had been given, which was rather a good construction record. However, we are all aware of the fact that NINA will cease to run in March next year, and so we are also commemorating ten years of active life of the accelerator and the important research which has been carried out with it during that time.

The closure of NINA marks the end of a long period of work in this part of the country, with accelerators of steadily increasing energy, which began 40 years ago in 1936. This was about the time that the construction of a small cyclotron in Liverpool was started by Sir James Chadwick, and NINA, I think it is true to say, is a direct descendant of that small machine which came into operation just before the beginning of the War.

However, the local tradition of research in nuclear physics goes back much further than this since we remember that the nucleus itself was discovered in Manchester by Rutherford in 1911 and that Chadwick began as a student there and carried out his first research under Rutherford. Going on many years to 1947, also in Manchester, we remember that Rochester and Butler discovered the V-particles in the cosmic radiation and opened the door to the study of the new phenomena associated with the property that later came to take the name of 'strangeness'. This discovery, together with the discovery of the pion by Powell about the same time, really marks the beginning of the modern study of particle physics.

This was towards the end of the period during which the cosmic radiation provided a sufficient source of particles for high energy research, and the period during which comparatively small scale and inexpensive equipment could be used to make significant discoveries. As an example of this perhaps I can quote something else close to home. In 1938, during the time when the small cyclotron was under construction in Liverpool, E.J. Williams who was a brilliant theoretician as well as an experimentalist, built a cloud chamber there with which he was able to produce powerful evidence for the existence in the cosmic radiation of particles having mass intermediate between those of the electron and proton. Other people had obtained similar evidence, and, in those days, these particles were called 'mesotrons'. Of course they are the muons with which we are now so familiar.

The small cyclotron which produced deuterons with an energy of 8 MeV were succeeded after the War by a much larger synchrocyclotron giving protons of 380 MeV energy, for which Chadwick was also responsible. Both these machines in their day were world-class accelerators, as was the 340 MeV electron synchrotron built in Glasgow after the War under the direction of Professor Dee.

A number of people who played a prominent part in the planning of the Daresbury Laboratory, or in the work of the Laboratory itself, were associated with one or other of these accelerators. It is not possible to mention them all by name, but a few have devoted themselves continuously to the work of the Laboratory from the earliest planning days onwards. Of these, I would particularly like to remember John Rutherglen, whose untimely death in a car accident last summer was a great shock to all his friends as well as a serious blow to high energy physics. He was a member of some of the early working parties making plans for the new accelerator and made an outstanding contribution to the experimental programme of the Laboratory.

Someone with a very long record is my old colleague, M.J. Moore, who from the early days of the small cyclotron at Liverpool, was responsible for the construction and maintenance, and contributed in many ways to the planning of the synchrocyclotron and to

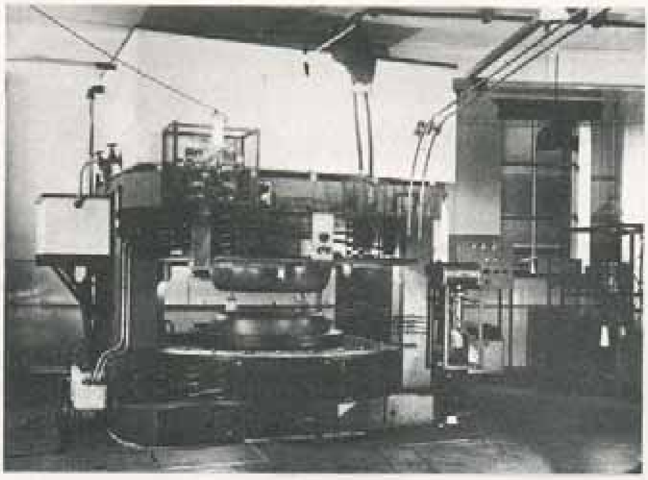

Fig.1 Magnet for the small cyclotron at Liverpool.

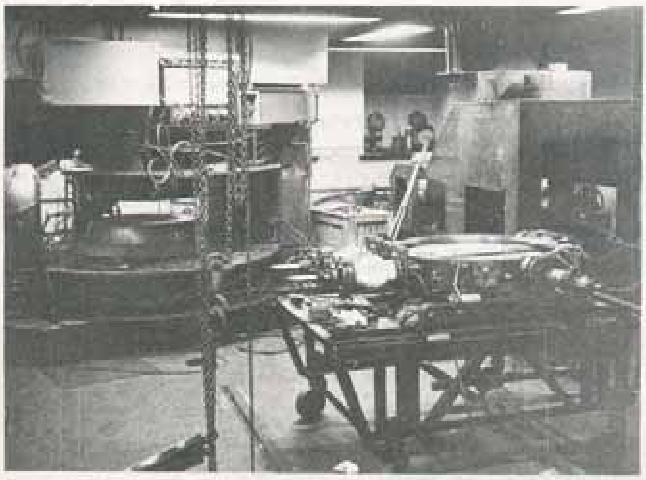

Fig.2 The Liverpool cyclotron in a dismantled state just after the War.

Fig.3 Mike Moore at the control desk of the Liverpool cyclotron.

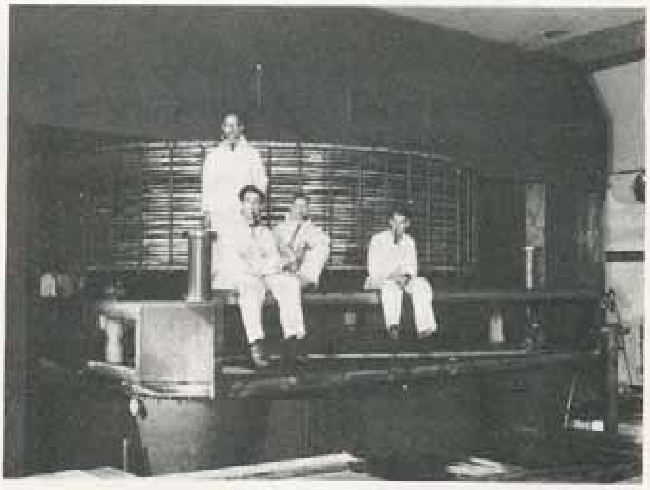

Fig.4 Magnet of the synchrocyclotron at Liverpool.

the smooth running of the research programmes at both these accelerators. He also played a large part in the early stages of the Daresbury Laboratory and recently retired as head of the Engineering Division here.

Perhaps I could show one or two photographs to illustrate some of these remarks. The first is a very old one (fig.1), of the magnet for the small cyclotron at Liverpool, taken I am sure, soon after it was assembled because the space around it is remarkably clean and uncluttered. The next, fig.2, shows the cyclotron after the War, on one of the many occasions when it was in a dismantled state. During that period it had been rebuilt and refurbished, and fig.3 shows Mike Moore seated at the rather beautiful control desk which had been newly acquired. Figure 4 shows the magnet of the synchrocyclotron and illustrates the step-up in size to the new machine. Most of you will recognise John Gregory on the right of the group in the photograph. Albert Crewe, seated on the left, is now well known in the field of electron microscopy.

If I may be permitted a personal reference, my own involvement with NINA began in November 1960 when Jim Cassels had returned from a sabbatical year at Cornell University to take over as Head of the Physics Department in Liverpool following the death of Herbert Skinner. Suggestions for a new accelerator had been in the air for several years, and he approached Harry Newns and myself with a proposal that we might consider giving up our respective researches for a couple of years to devote ourselves to the design of the magnet ring for the proposed electron synchrotron. This we agreed to do, and after educating ourselves in the basic principles of strong focusing accelerators, about which we knew absolutely nothing at the time, enjoyed a most interesting and rewarding interlude. We were later joined by an enthusiastic young group of post-graduates, and perhaps I could name them, as most of them will be known to you. They were Brian Couchman, Vince Hatton, Neil Marks, Robin Tait and Ian Rabinowitz, and they were installed in the old cyclotron building at Liverpool and set to work to design and test model magnets. Meanwhile, John Fox and Jerry Thompson at the Rutherford Laboratory were busy on the magnet power supply and radio frequency system.

I will conclude by telling you how NINA came to be christened. One day one of our little group of magnet designers came into the laboratory (it was soon after financial approval had been given for the project in July 1962) with the information that the Rutherford house journal was proposing a competition to find a name for the new accelerator. Well, it was generally agreed that no southerner should be allowed to name our machine, so half an hour was set aside to consider the matter. Various suggestions were made, mostly related to Lewis Carroll's connection with Daresbury, but these were soon rejected. Then Brian Couchman walked over to the blackboard, wrote down 'National Institute' and after a moment's hesitation, 'Northern Accelerator' -- NINA. It was so obviously right that I immediately went to tell Alec Merrison, and he readily agreed.

To turn now to matters of more importance; our first speaker is Professor Cassels, and he is going to talk about the background and history of the Daresbury Laboratory. Jim Cassels is the Lyon Jones Professor of Physics at Liverpool University and was involved from the very earliest days with plans for a new national accelerator; so who better than he to tell us about the history of events leading up to the establishment of the Laboratory.

THE BACKGROUND OF NINA

Professor J. M. Cassels, FRS
Lyon Jones Professor of Physics
University of Liverpool

Before lunch I was reading the official history of NINA as described on the screens outside this lecture theatre, and I believe the talk I am about to give should be called the 'alternative history' of NINA. Since most of the events I shall be describing occurred 15, 20, or more years ago, a modicum of candour is perhaps permissible.

To understand the history of high energy physics in this country after the war we have to start by going back to the war itself. As Sir Brian Flowers has remarked, it was a physicists' war, and British physicists did extraordinarily well. There were the triumphs of radar, scientific intelligence, and operational research. The future of military and civil nuclear power was diagnosed with extraordinary accuracy when the slender resources deployed are considered. The names of Watson-Watt, Randall, Boot, Cockcroft, Jones, Blackett, Chadwick, Peierls, and Frisch may be mentioned, but there were many others. It is quite amazing how consistently right these men were, and our country certainly owes its very survival to them.

After the war there was a general return of the physicists to the universities, and political attitudes to their work were very favourable, naturally. Cockcroft[+] did not return to Cambridge but set up AERE, Harwell, and brought Big Science to British soil for the first time. There are many of my generation, like myself, who really learned what Big Science was all about in the exciting days at Harwell in the late 40s and early 50s.

Thus we started off after the war in a bright glow of enthusiasm and hope, but by 1950-51 things did not look at all good. Let us examine the particle accelerators of those days and see what had happened. At Harwell a 170 MeV synchrocyclotron had been built at great speed by Pickavance[++], who also had quite a promising young lad called Adams[*] working for him. This machine had originally been intended for making radioactive isotopes, and the modifications to make it suitable for high energy physics were only introduced later. By a tragic error of planning, for which Pickavance was not responsible, its energy was too low to allow pion production. Moving north up the country, we find a 50 MeV electron synchrotron being built in Oxford: this was too timid a project to have the slightest chance of influencing the course of physics, and it did not. At Birmingham Oliphant[**] had made the marvellous invention of the proton synchrotron, which has come to dominate particle physics in the last two decades; but he and his colleagues set their faces against using money and manpower in the way that they have to be used if Big Science is to be successful. That was a great pity. In Liverpool Chadwick[†] with his usual sagacity had chosen absolutely the right machine, the 380 MeV synchrocyclotron. But it was ordered as a whole from Metropolitan Vickers and not assembled by a local team in the modern style, and so it was very late in spite of the high words that frequently passed between Moore[††] and the contractors. It was a good machine when it was finished in 1954, and the people who worked on it have spread through Europe as the founders of the whole continent's work in high energy physics. But, to repeat, it was very late. Finally, going even further north to Glasgow, we note the 330 MeV electron synchrotron which was also late. Machines of that size and type were notoriously difficult to use, but some good work was done: it was not helped by the minute size of the experimental room which made it difficult to swing a spectrometer arm, let alone a cat.

[+] Sir John Cockcroft, FRS, first Director of AERE, Harwell, and later Master of Churchill College, Cambridge.

[++] Dr. T. G. Pickavance, FRS.

[*] Dr. J.B. Adams, FRS, now Joint Director General of CERN.

[**] Professor M.L. Oliphant, FRS.

[†] Sir James Chadwick, FRS, OM, (Lyon Jones Professor of Physics, as I am always extremely proud to mention), and later Master of Gonville and Caius College, Cambridge.

[††] Mr. M.J. Moore.

The British situation in 1951 was particularly disappointing because such rapid progress had been made in the USA. The Berkeley synchrocyclotron, a conversion of a visionary but unsound pre-war project, had been followed by the commissioning of the Illinois electron synchrotron and the Chicago and Columbia synchrocyclotrons. More significantly still, the proton synchrotrons called the Cosmotron and Bevatron were coming along at great speed, and confidence in the operation of these audacious projects had reached a high level. I well remember seeing the Cosmotron in an advanced state of construction during my first visit to the USA in 1951.

It was then that Cockcroft realised that something had to be done, and set out to see that it was done.[+] He initiated a series of conferences at a rather agreeable country seat, Buckland House, near Harwell. At one of these I remember giving the advice, based on hardly any evidence, that 15 GeV would be a good energy to adopt for a new project. Lord Cherwell remarked that he thought that the British taxpayer would sooner have an extra shilling's worth of meat on his weekly ration.

About this time Rabi[++] came to this country to press for the idea of the future CERN, as a means to resurrect European science, which in most countries had hardly begun to rise from the ruins of the war. I was fired by this idea and, together with Pickavance, toured the country advocating British participation. Looking back, I can see that these efforts had the strong if tacit approval of our joint chief, Cockcroft. We encountered strong resistance from Skinner[*], who in turn had persuaded Dee[**] to join the opposition camp, but we had a much more favourable reception by Powell[†]. All that culminated in an historic meeting[††] in Cambridge of a Royal Society Advisory Committee, chaired by Chadwick. A motion to recommend joining CERN was carried with only Dee dissenting, since Skinner withdrew his adverse vote literally in the last ten milliseconds.

That decided, the next question was the accelerator policy for the UK itself. It was then that the oscillation between a European high energy physics policy and a nationalist policy began. Looking back, I think we were foolish not to come to terms with the European policy once and for all[§] but at least we had the excuse in those days that the scales of universities and science generally were expanding at 15% p.a. for year after year. It was possible to think that the final size of the physics establishment would be very large indeed. The bubble did not burst until the late 60s.

I had been struck by the favourable properties of the $p + p \rightarrow d + \pi^+$ reaction and suggested that a 600 MeV proton linear accelerator (PLA) should be built as a pion factory; it was the first time that sort of industry was proposed. If it had actually been built we would have had a fine time with it, in the late fifties and early sixties, after the non-conservation of parity had been discovered and when precise work was done with pionic and muonic x-rays[§§]. However the PLA was never taken beyond 50 MeV. Many were worried at the idea of doing something that was not being done in the USA, and

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[+] His efforts extended over two decades, and were all the more remarkable because his main responsibility was nuclear energy, which had nothing to do with high energy physics, at least in any direct sense. He got very little thanks for his leadership of the policy side of high energy physics in his lifetime, and I hope this lecture will make some small amends.

[++] Professor I. I. Rabi, Columbia University.

[*] Professor H. W. B. Skinner, FRS, Liverpool University.

[**] Professor P. I. Dee, FRS, Glasgow University.

[†] Professor C. F. Powell, FRS, Bristol University.

[††] It was typical of Cockcroft that he should see to it that such a young (29) enthusiast as myself should be present at the final count.

[§] Chadwick told me in 1972 that he had been opposed to the building of Nimrod on precisely these grounds. As always, he was right about an important matter of long term policy.

[§§] The original idea of pionic therapy for cancer formed part of the case for the machine. I well remember the uselessness of this being argued at a conference in Cockcroft's office, but Cockcroft told the sceptic (Cockburn) that he would soon change his mind if he were to discover that he had cancer himself. Pickavance, Tait (AERE), and I were much amused.

Skinner was one of these. When the beam was extracted from the Liverpool synchrocyclotron by the clever magnetic resonance method (Tuck and Teng, Le Couteur, Crewe) he was able to get the PLA stopped, in spite of the fact that the 380 MeV at Liverpool was too low an energy for the $p + p \rightarrow d + \pi^+$ reaction, and that the experimental room was too small unless the Roman Catholic Cathedral were abandoned, an unlikely eventuality.

At that point, then, the discussion in the UK went back to the idea of building another PS, and a 12 GeV alternating gradient synchrotron was proposed. By democratic and not always wise processes this became NIMROD as we know it.

Administrative machinery was set up in 1957 to enable the universities to make use of the machine, and this was the National Institute for Research in Nuclear Science (NIRNS). There were at least two surprising things about the membership of the first NIRNS Board. Cockcroft, although he had carried the proposal all the way, was not the Chairman. In fact Lord Bridges, formerly in charge of the Treasury and later Secretary to the Cabinet, was the first and only Chairman of NIRNS, and a more distinguished one could not be imagined. Further, one of the Board was Sir James Mountford, making his first personal venture into nuclear physics, after a previous career as a Professor of Classics, and while continuing to be Vice-Chancellor of Liverpool University. Although he is the first to make pleasantries about his talents as a nuclear physicist, his was a very important appointment; he was the doyen of Vice-Chancellors, and his university was really the only one in the UK where the peculiar needs of Big Science were adequately recognised at the time. His presence on the Board was a signal that co-operation with the universities was seriously intended, and I shall come back to this. However one of the consequences was that Skinner, also at Liverpool, could not be on the Board without destroying its geographical balance. Since he was in charge of much the most successful high energy physics group at that time, this was a terrible blow to him. I am sure that the frustration and worry that followed led directly to his death three years later[+]. At that time I was still very young, and it did not occur to me that people could be killed in that way: I thought it only happened in Victorian novels, but let me assure you that it happens in real life too.

The NIRNS Board appointed Pickavance to be the first Director of the Rutherford Laboratory, and construction of Nimrod began. At Liverpool, where I now was, I had many doubts about the situation. Having been at Harwell I had seen the Royal and Ancient game of empire building expertly played by some, and I thought it was possible that real service to the universities would fail to materialise, and indeed that they might be treated with contempt. My worries were wrong for the next 15 years or so. The high quality of the NIRNS Board meant that it kept its primary aim always in view, and Pickavance himself consistently took the attitude that the universities came first and all the way.

As it happens another critic was Devons[++] who wrote a witty memorandum about the setting up of NIRNS. I shall always remember his summary:- 'plus, solves some administrative problems; minus, creates new administrative problems'.

We met and talked about the situation, and took note of another point. I have never myself seen a copy of the letter that Cockcroft sent to Ministers about the need for NIRNS, but we were credibly informed that it spoke of two accelerators costing M£4 each. Why not, we thought, capture the second for the universities in the north? We found a site for this northern laboratory under the second arch of the southern approach ramp to Thelwall Viaduct on the M6. You may think that was an odd place to have an accelerator, but actually the M6 had not been built at that time (1958). I was still keen on pion factories, and we discussed building a sector-focusing cyclotron from which we could extract essentially all the proton beam. Devons had the exciting idea that the pions produced could be accelerated

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[+] After this lecture Sir James Mountford told me that he consulted Skinner about all important points that came up at the NIRNS Board. I have not the slightest doubt of that, but then Skinner always liked to interact personally and immediately, at all levels from Cabinet Minister to research student, when something was on his mind.

[++] Professor S. Devons, FRS, then Langworthy Professor of Physics at Manchester University, now at Columbia University.

further in a linear accelerator[+], the lifetime of the pions being stretched relativistically. I studied the theory of the sector-focused cyclotron and gradually came to the conclusion that it would be a horribly difficult machine to build, and eventually I felt it would be too much for me. That meant that the professionals would have to be brought in, a process that had failed to produce results with the 600 MeV PLA. So Devons and I started to talk about another and much easier machine, which would also be complementary to NIMROD and the CERN PS. This was an electron synchrotron, eventually to be realised as NINA.

Meanwhile, back at Liverpool, Skinner was not pleased that I had been talking to Devons, and he decided to promote an electron synchrotron himself. Another source of confusion was that Mott[++] at Cambridge had hired a promising young lad called Wilson[*] to show him how to build an electron synchrotron for next to nothing.

The situation was confused and so Devons and I both emigrated to the USA, an action which was regarded as the solution to all problems in those days. Devons is still there, but then he always was the more intelligent of the two of us.

The ante-natal development of NINA continued while I was at Cornell, although I was quite out of touch with it. I assume that Cockcroft was pushing it along all the while. There was some noise in the system because Glasgow, having been refused a low energy electron linear accelerator, courageously doubled the odds and bid for a high energy electron linear accelerator instead. This was killed eventually on the grounds that the duty cycle would be too low.

At this point Skinner died and I was appointed his successor within a few hours by Sir James Mountford, quite unconstitutionally but completely authoritatively. I was flown back to Liverpool for a conference at Liverpool chaired by Cockcroft: experts said (wrongly) that a 10 GeV electron synchrotron was impossible so the energy was reduced to a worryingly low 4 GeV.

In the autumn of 1960 both Merrison[**] and I returned to Liverpool, and began the search for a site. The University owned Wood Park Farm, next to its Veterinary Field Station in the Wirral. This had a delightful situation and suitable geology, but would not in fact have been sufficiently accessible for other universities. Officers of the Cheshire County Council were invited to a lunch at the University, and some fruitful horse trading took place over the coffee. Those were the days when every pleasant town in England was seeking to have a university, and Chester was no exception. However it was feared that considerable resistance would be offered by Liverpool University, which draws many of its students from that direction. They need not have worried, because Sir James was never a man for petty thinking, but they were not to know that. Once this point was cleared, the Council officers told us that an accelerator in the Wirral could not be contemplated, but they pledged every assistance to find a site in north Cheshire, and they were as good as their word.

The next possible site was Stretton airfield, formerly a RNAS station. I well remember trundling out there with Merrison, Moore, and Fox, the engineering genius who designed the NINA choke. We stopped on the way and sat in a field with our backs against a hedge, in the sunshine, listening on a portable radio to a test match commentary. England lost. Then we went on to Stretton, which looked a possibility. Then the Daresbury site came up, and it seemed (and has proved to be) quite ideal. So that was the site settled, at the fourth attempt.

From then on the project settled down and Holt[†] and Newns[††] designed the magnet lattice. Merrison took charge of the venture and away it went.

[+] This would not in fact have been a very fruitful operation, but it seemed a good idea at the time.

[++] Professor Sir Nevill Mott, FRS.

[*] Professor R. R. Wilson, then at Cornell University and now Director of Fermilab.

[**] Sir Alec Merrison, then Professor of Experimental Physics at Liverpool and now Vice-Chancellor of Bristol University.

[†] Professor J.R. Holt, FRS, Liverpool University.

[††] Dr. H. Newns, Liverpool University.

I believe that I have now discharged my remit to recall the background to NINA, but I would like to add a few remarks about subsequent events.

One thing that worried me greatly for years was the fear that 4 GeV was too small. I remember mentioning this to Wilkinson[+] after the NIRNS Board meeting at which NINA was approved. "Well, you can't go back now" he said, comfortingly. Actually I feel now that it was quite a good size: the physics was there.

Then I remember how impressed I felt as Merrison and his team completed the machine at lightning speed, the more remarkable because NIRNS was liquidated and the new and very inexperienced SRC was born. Actually the speed of building NINA was almost too fast, since nobody was really ready to use it when it was finished. This unfortunately gave NINA the reputation of being a machine which was not being used, especially among those who did not leave London very often. There was a Committee set up during the building period to look into the future needs for experimental apparatus; I may even have been Chairman of it. The most important piece of experimental apparatus, we decided, was a theoretical physicist who would point out interesting things to do. It took a seemingly interminable time to find him, but eventually Donnachie[++] was appointed. The experimental programme burst into furious life, which has continued unabated ever since.

Then there was a proposal to build a much larger ring, Frederick. You will remember that NINA was supposed to inject into Frederick, a technical problem. If implemented this might have grown in the way that the Hamburg complex has, but in my opinion a credible proposition including proper experimental facilities should have been put forward. However I do not think the lack of that has made any tangible difference, since the venture would have foundered anyway in the present economic crisis.

The thing that has pleased me most is that NINA has fulfilled its highest purpose. It has attracted extremely bright young men in all the universities around: I shall not make them blush by naming them. A real physics community has been built up whose members have learnt to work together in the exacting conditions that high energy physics imposes. I have only to look at the schedule of the machine to see mention of the Glasgow-Liverpool-Sheffield collaboration, or the Mancaster[*] group. That community is the ultimate product of NINA, and personally I am and always shall be extremely proud of my part in creating it. I am sure it will go on to do great things at the CERN SPS after NINA is closed.

Unfortunately the end of my lecture must be clouded by a reference to recent attempts of the SRC to destroy the community of which I have spoken, in particular by refusing northern universities the right of assembly at Daresbury to prepare their work at CERN. This has been a cardinal error, the stupidest I have ever seen. As the full story becomes known, and nearly all of it is widely known by now, the reputations of the perpetrators will be reassessed appropriately. They have sown the wind, and they shall reap the whirlwind.

+ Sir Denys Wilkinson, FRS, then Professor of Nuclear Physics at Oxford and now Vice-Chancellor at Sussex University.

++ Professor A. Donnachie, Manchester University.

* i.e. Manchester-Lancaster.

INTRODUCTION TO SIR ALEC MERRISON

Professor J.R. Holt

Our next speaker is Sir Alec Merrison, our first Director, whom it is a great pleasure to welcome back to Daresbury. The success of the Laboratory depends very much on the scientific and administrative skill with which he got everything going in a grand style. He was determined right from the start that things should be put in train exactly as he wanted them to be and I think the results of this forethought and planning are plain to see. Soon after he took over as Director in 1962 he fixed the date for the start of the synchrotron as around the end of 1966 and as we all know that is exactly what happened. I believe he built the machine within his original financial estimates as well which is quite an achievement. After proving himself a physicist of the first rank by research on the Liverpool synchrocyclotron and later at CERN his great abilities as an administrator were realised here at Daresbury. He's now at Bristol, a member of that growing band of physicists who are taking over the Vice-Chancellorships of the universities of this country.

THE EARLY YEARS OF NINA

Sir Alec Merrison, FRS
Vice-Chancellor
University of Bristol

I won't bore you with the details of the design and planning of NINA but will, as Jim Cassels has done, try to tell you some of the things which I think are important. I think Jim missed out one really crucial meeting in March 1960 when we were both committed to coming back to Liverpool. He was in Cornell at that time and I was in Geneva, but we were invited back for this slightly odd meeting in the Physics Department in Liverpool run by Sir John Cockcroft. At that time I still hankered after building a pion factory of some kind - one had got used to pions over the years and one knew how to handle them. Jim had become very much wedded, particularly with his experience working with Bob Wilson, to the electron synchrotron. We had amiable discussions, as indeed throughout our lives we have had amiable discussions on many subjects, and as we usually do, reached an agreeable compromise rather quickly - to build an electron synchrotron.

Edward Bridges was then Chairman of NIRNS which was a remarkable outfit and it is a pity that it survived for only a short time. It was perfectly right that it should be subsumed into a greater body when that had to come but while it lasted NIRNS did do remarkable things largely because of two people - Edward Bridges and Gerry Pickavance. This was from the late 1950's up to 1965 when the Science and Technology Act came along. The Daresbury Laboratory with the synchrotron, NINA, was authorised in July 1962. I remember it was a Friday and I remember too that it was exactly the same day when MacMillan sacked thirteen of his cabinet ministers, including the Chancellor of the Exchequer. I don't think, in spite of the sterling work that Mike Moore had been putting in on the Leader of the Opposition at that time, that the two events were connected, but nonetheless they were both equally agreeable. However, NINA, like all good projects, went through its bad periods and it wasn't long after that there were financial problems in NIRNS and quite the most logical way of solving them was to stop the Northern project. It hadn't been started, no real money spent on it by that time, and so it would have been very easy to have killed it off. I remember Gerry Pickavance and I constructing perfectly logical arguments as to why you shouldn't do this and talking with Edward Bridges about it. I think you really have to understand that he was a giant in the world of the Civil Service. He had been Secretary of the War Cabinet, then Permanent Secretary to the Treasury and Head of the Civil Service and retired from that. NIRNS was one of his retirement jobs. Consequently civil servants in general treated him with extraordinary respect. I was quite interested to see how he would play all this when we went over to the office of Sir Frank Turnbull who was then Permanent Secretary of the Ministry of Science and how he would use all the really quite convincing, but slightly recherché arguments, that we had constructed for him. However he would have none of them. He simply said to Sir Frank Turnbull "Well of course you could stop this Northern project but your masters would then have to go back to Parliament and say they had made a mistake - and they wouldn't like that". It was the finest example of a knock-down argument since Humpty Dumpty and it just finished the conversation!

If I can digress from NINA for a moment it was an interesting time in the science world too. The Trend report which created three research councils, or created two and acknowledged the existence of a third, was published in 1963. This meant that there was an awful lot of political science, or scientific politics, which had to be done on behalf of NIRNS which Gerry Pickavance and I did, but which mostly fell on Gerry. We were trying to get the position of nuclear physics established in the SRC, which was to be formed at that time, and we used to go to meetings in London where one discussed with civil servants the shape of things to come. One of the things I remember most clearly about those meetings was that a young civil servant, Geoffrey Caston, started to appear. We didn't know who he was, he used to drift in and we hadn't any idea of what sort of role he was playing. It wasn't until after the meetings were over that we found out he'd actually been drafting the bill the whole time and that he played a pretty important part in all of this. So it was quite easy for people at our level to influence the course of quite high events of the time -

which I think had a great deal to do with getting the SRC off to a good start. One of the things which Bridges did, almost single handed, was to see that there was adequate capital delegation. He was extremely worried about the various layers between the chap at the bottom, who wanted to do something, and the chap at the top who might say 'no'. So he arranged a capital delegation of £100,000 to the Council which was a huge amount in those days. Of course £50,000 of that capital delegation was carried by the Nuclear Physics Board which shows how important it was that there was a Nuclear Physics Board - the original proposals hadn't included anything like that. So you see that, quite apart from what was going on here at Daresbury, we had to look after all these other things to ensure that there was a decent framework within which to work.

You have heard already how Jim Cassels asked John Holt and Harry Newns to look into the design of the magnet lattice of NINA and this they did. In fact, I think one of the nicest things which happened was that they very quickly found out how to incorporate long straight sections in machines of this kind. Unfortunately their public relations weren't quite so good as those of other people and so these long straight sections got to be known by the nasty name of "Collins' insertions". Nonetheless it was their doing and from that time it was fairly easy to see how the magnet lattice of NINA should go. John Holt has already mentioned all the people who were then working on these problems.

Since one was thinking of a site between Liverpool and Manchester an area about 30 miles square was under consideration. It is very difficult to look at a map and decide which site to choose. Then we remembered geological maps which show the top and what goes on underneath. The answer then became rather clear since a large part of the area was not good whereas in the North Cheshire plain there are large blocks of sandstone sticking out of the layer of mud. You do not have to build on a good foundation. As Ted Ashley, one of the UKAEA engineers on the project once said, you can build these things on chewing gum if you want to but the cost is higher. You would then have to provide very elaborate foundations, as with the proton synchrotron at CERN where the material underneath is essentially chewing gum! It is actually called molasses. However our review of the area produced three sites which seemed particularly suitable. Jim Cassels has already mentioned the site at Wood Park, Neston, and we also considered a site at Mickledale, on the top of the cliffs at Frodsham, and this particular site here at Daresbury. So that was the way the site was chosen. Figure 1 shows how it looked from the top of the hill. On the Laboratory side of the canal it is all good sandstone, within 30-40 feet of the surface at the worst point, and so one had something fairly easy to work with. Figure 2 shows the model we used to hawk around to planning authorities. As you can see it is not quite the laboratory as it is today but this is the way we first conceived it and it was built more or less like that at the beginning. It seemed to me that it was absolutely essential to get onto the site as quickly as we possibly could. Once you start actually to walk around a site in gumboots you become convinced you're committed to doing something about it, and so we moved onto the site in 1963 and started to put things together.

We were very fortunate in that the UKAEA Engineering Group at Risley were pretty well out of work at that time. We rapidly established that on the strict engineering side they could not be of too much use to us, to be perfectly candid about it, but on the civil engineering side they were absolutely first class and they in fact did undertake the civil engineering work for us. This was terribly important because the civil engineering is half the cost of a project of this kind. Mike Moore took on the job of looking after it. I think he was somewhat unwilling to be limited in this sort of way but you all know Mike, he can turn his hand to anything and does so with extreme fluency and skill. It did strike me very seriously at the beginning that we had to get the civil engineering right in order that the machine should work successfully. Not only that, it was clear that since half our money was tied up in that way it had to be somebody fairly rough, if I can put it so, who was going to see this thing along and that was how we got the civil engineering done.

Figure 3 shows how it looked when we started drilling into the ground, looking rather like Stonehenge. In fact it was said that we were giving employment to the archaeologists 2,000 years hence if to no one else! There are columns which go down to the sandstone. The tops of these are tied together with a ring and the pillars which can be seen in the photograph are those on which the magnets are sited. The

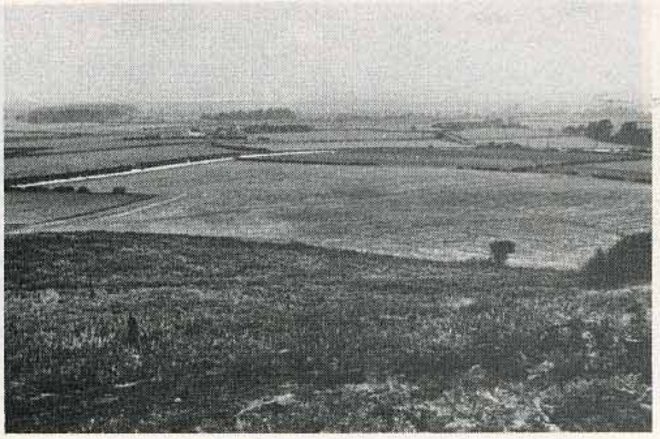


Fig.1 The site at Daresbury chosen for the Laboratory.

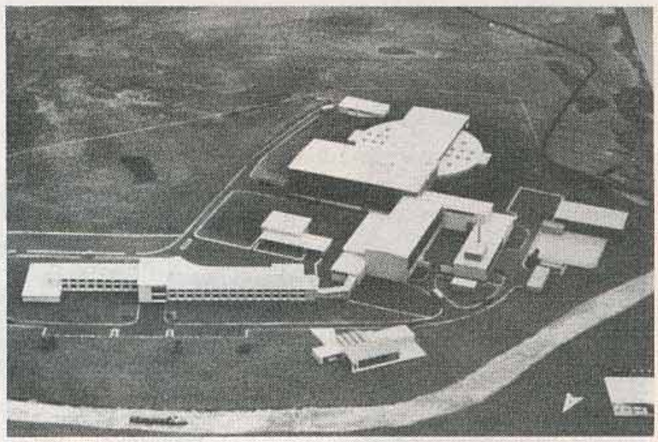


Fig.2 The model of the site used in discussions with local planning authorities.

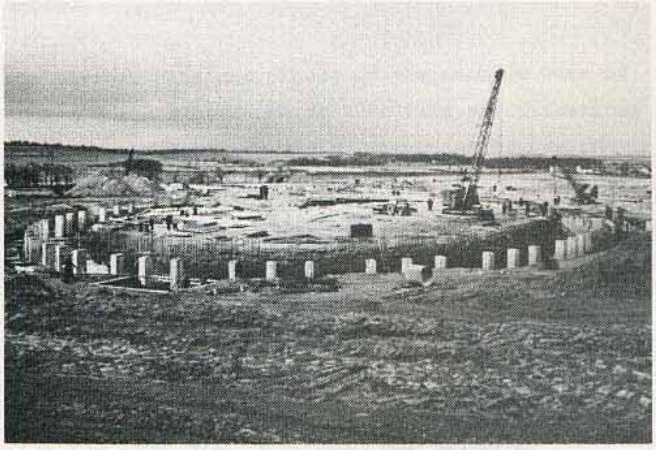


Fig.3 Foundations for NINA.

Fig.4 Magnet blocks for NINA.

Fig.5 A 'Laboratory Meeting' in progress. Left to right Michael Crowley-Milling, Harold Rothwell, Basil Zacharov, Alec Merrison, Tony Egginton, Mike Moore and Bob Voss.

Fig.6 An RAF helicopter about to give the Director a 'lift'.

Fig.7 The day we burst the banks of the Bridgewater Canal. Ted Ashley looking on.

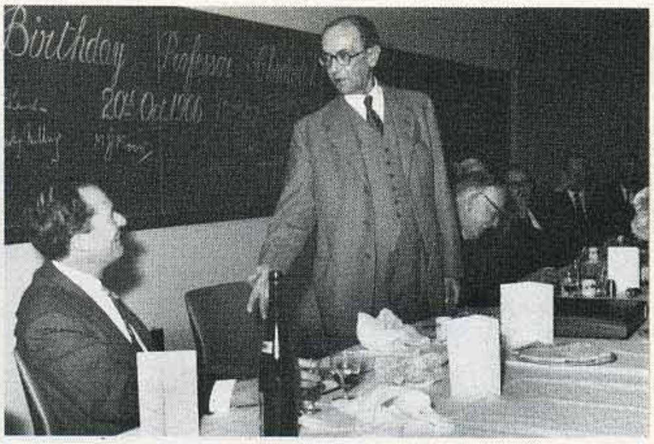


Fig.8 Professor Chadwick at the lunch given on the occasion of his 75th Birthday remonstrating with the Director.

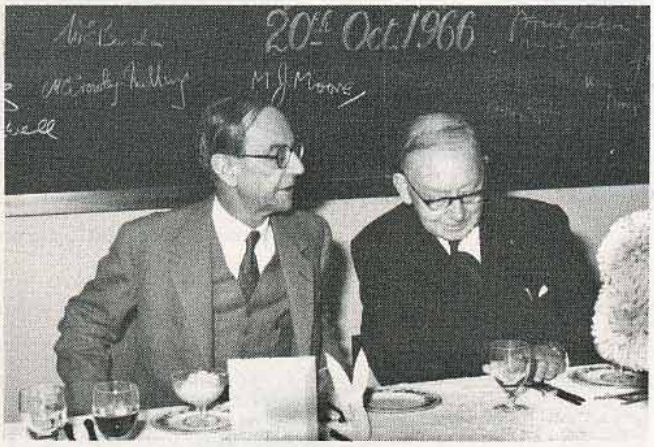


Fig.9 Professors Chadwick and Cockcroft at the 75th Birthday Celebration.

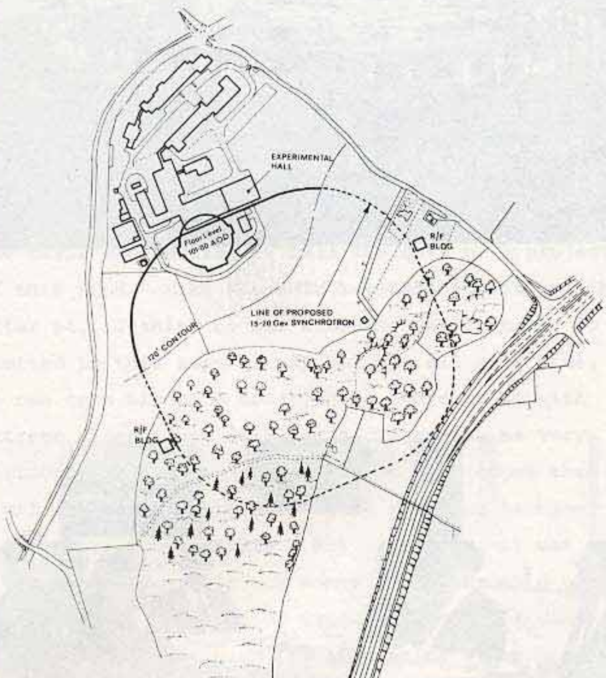


Fig.10 'Frederick' - the proposed booster for NINA.

choice of a sandstone site turned out to be a perfectly good one and we had no problems at all with movement. Even when we were moving 30,000 tons of concrete about there were no serious worries so far as movements of the machine were concerned.

The great virtue of a strong focusing synchrotron is that everything doesn't interact with everything else. You really can design a magnet, an r.f. system, a vacuum system and so on without too much interaction between the parts and this means that the whole team working on such a project can be split into units which are rather independent of each other and happily we had a number of extremely competent and independent people who got on with this job very well. It was easy to keep the thing together by having a project meeting about once a month.

The various bits and pieces of the machine were of course quite a headache to turn into specifications as they were designed and it was here that we put in the maximum of effort and this was probably the reason why the project sailed along pretty quickly, as indeed it did. The whole writing of specifications and tenders and the negotiating of a contract is a very much underrated pastime. It calls for a great deal of thought, work and skill. One of the most difficult things is of course being absolutely sure that the firm that is bidding for a job has actually read the tender. John Fox to whom reference was made in the previous lecture, hit upon rather a good solution to this in that, quite apart from sending out an invitation to tender which would be really rather detailed, he would always put at the back a sort of examination paper on which the tenderers had to answer the questions. It became fairly clear when you looked at the answers whether they had understood the really tricky things in the tender. I do remember that John at one time wanted to put as one of the questions 'Do you supply green shield stamps?" which caused at least one contract man sitting in this room today to be quite unhappy about the whole deal. I'm afraid we never actually managed to get that into the contract although we tried hard! We of course let many contracts and naturally tried to let as many in this country as we could. However we were not very successful in this. In fact it must be said that the tenders put to us by firms on the continent were really very good - and, of course, the negotiations were most enjoyable. Bob Voss will remember the arguments we had with people like Siemens and getting another 1% out of the Swiss is an occasion when you really see men sweating blood! There is a remark of Ian Fleming's to the effect that if ever you see a Genevoise leaping out of the window leap after him as you may be sure that there is 1.5% to be gained. At the tender stage you see them trying to find out whether 1% would do them any good, or 2% or whatever. It is certainly rather an amusing game to play.

Anyway the pieces started to come along and fig. 4 shows the magnet blocks - the first of which British Rail lost for us - of course! I think it got as far as Warrington and then it disappeared on a branch line somehwere. Eventually it turned up. They were shipped over to Harwich and then came by train from there, but it was a very difficult operation. The photograph shows them sitting in the experimental room. All good experimental rooms are full of magnet blocks at some time and there you see a necessary stage in the construction of an accelerator. I remember taking Cockcroft around CERN at the time when John Adams was putting together the proton synchrotron there and the experimental hall was absolutely full of magnet blocks as far as the eye could see. We were both looking down at these and I said to Cockcroft 'You know, I think Adams is really committed to building this accelerator' and he rather gloomily replied 'he is committed to making it work'. However all good machines go through that sort of stage.

The administration at the Laboratory was very simple too, and run on a very light rein. We had something called a 'Laboratory Meeting' which used to meet once a fortnight. Figure 5 shows such a meeting in progress - all extremely powerful and intelligent men with myself, looking serious, at the end of the table. We didn't find this very difficult and at one time we were running entirely without a laboratory secretary. This was long before Harold Rothwell appeared on the scene and showed us how to do it of course.

To digress, fig. 6 shows you the daring sort of thing the Director of a Laboratory has to do from time to time. We needed an aerial picture of the site and the firm we had asked simply did not seem to be able to turn out. Don Braben, always a resourceful chap, suggested asking the RAF at Valley in Anglesey to send a helicopter. They not only agreed but made no charge, regarding it is a training flight! Anyway, they sent over this helicopter and there you see me sitting in it. It is an interesting experience to sit a thousand feet above the Laboratory with one's feet dangling out like that.

We were not successful with everything we did and believe it or not fig.7 shows the Bridgewater Canal the other side of the bridge. We had made a hole in the wall of the canal, as you can see, and that is the great Ted Ashley standing looking at this miserable sight. It was a huge hole, we flooded most of the ground north of here and had all sorts of curious insurance claims. These included one from a gentleman whose goldfish had been washed into the Manchester Ship Canal from his garden. However there was one good thing about that accident. We had been looked on with some suspicion by the locals until that point, but when we did something as crass as that they, of course, took a considerable rise out of us and felt that much better

There were a number of people who although they did not work in the Laboratory made an enormous contribution to it nonetheless. One of those was certainly Chadwick, who has been mentioned several times already. He in a sense was responsible for the high standard of nuclear physics in this area. When he moved up from Cambridge in the 1930's he attracted around him a very considerable gang of people, including John Holt, Mike Moore and people of that kind. Figure 8 is a picture which has recently achieved a considerable amount of fame. This is Chadwick telling me off, actually, on the somewhat insubstantial grounds that I didn't know how many buckets there were in the Laboratory. This was a birthday party we gave for him on his 75th birthday and of course beneath that cold exterior there beat a heart of pure stone and he could never resist the opportunity for being severe in, I suppose, the most attractive way. I say it has achieved fame recently because, as you know, the Royal Society issues biographical memoirs of its fellows who have died, and they published this in their most recent volume. There are, therefore, quite a number of people in that photograph who've managed to get their photographs into the biographical memoirs of the Royal Society before their time has come upon them, so to speak, and long may it be so. Chadwick had a scale of values of judging people which was rather peculiar. The prime accolade that Chadwick could bestow was a 'bloody fool' and Cockcroft was in the 'bloody fool' class. In fact I remember him saying to me once that when the first HT set was being built at Cambridge he walked into the laboratory and there were those bloody fools, Cockcroft and Walton who were measuring the ranges of protons in gases rather than getting on and seeing if atoms will split. I suppose the worst thing that Chadwick could say about anyone was that he had 'no damned sense'. If he said that then you knew he was really being very cutting. Cockcroft, as I think you can see from his demeanour in fig. 9, was always very humble in Chadwick's presence and it was clear that this was a relationship which had been established over the years. Of course Chadwick did preserve a somewhat austere appearance, and it was not until quite late on in his life I learned from Denys Wilkinson that when he had been Assistant Director of Research at the Cavendish Laboratory he had, in fact, had a considerable reputation as a curser. His oaths were apparently something to be wondered at. The story that Denys tells was of his coming in upon two luckless research students who were constructing a rather complicated piece of glass apparatus, in the days when you made glassware out of soft glass, and making an absolute botch of it. Chadwick elbowed them out of the way and of course as he was an extremely skilled glass blower, made this thing beautifully, annealed it carefully with a soft flame and then sort of stood back, put his spectacles up on his forehead and admired his handiwork. Then the thing went "ping-g-g-g" - just shattered in fact. The two luckless youths looked at him and dared not breathe let alone speak. Chadwick said not a word. He just looked at it for a second or two and then padded quietly to the door. He opened the door and he looked back on the mess that was there and said "Christ's bleeding teeth". So he really was rather a human sort of man.

I would like to close by showing you a picture of Frederick because, like Jim, I suppose you know that I feel it is a pity it wasn't built but perhaps not a tragedy. Figure 10 shows what it would have looked like. This is the 15-20 GeV version with injection from NINA into a big ring. The first estimates of the cost that I made, which I know some people in this room disagreed with, were conveniently £πM. In fact we could have built the machine at that time out of the underspends in two successive years of the SRC as it happens, such was the state of planning in those days and Mike Moore and I did seriously consider for a while taking six Welsh miners on the site and quietly burrowing away without telling anyone. But we did not do so and this really was my last contact with NINA. I was sitting in my office quitely minding my own business one day when Cecil Powell rang up and asked me if I would like to be the Vice-Chancellor of Bristol, which offer, mistakenly, I accepted.

INTRODUCTION TO PROFESSOR DONNACHIE

Professor J.R. Holt

We go onto the next talk which deals with the products of all the work and effort that has been put into the Laboratory over the years. Professor Sandy Donnachie, who was mentioned by Jim Cassels, came to Daresbury from Glasgow in 1969 holding a joint appointment with Manchester University to set up a theoretical physics group. This he did very successfully and quickly established a keen gang of young theoreticians who provided a most important stimulus to the experimenters working on NINA. He himself always took a very close interest in the experimental programme and he had remarkable understanding, for a theoretician, of the practical difficulties faced by experimenters. He played a most important part in the development of the programme by suggesting lines of experiments and by interpreting the results of the measurements. Never has a group of experimenters had a more understanding and helpful theoretician. Professor Donnachie.

PARTICLE PHYSICS FROM NINA

Professor A. Donnachie
Department of Theoretical Physics, University of Manchester

My task this afternoon is to discuss the physics which has resulted from all the efforts about which we have heard earlier today. This is no simple task because the sheer volume of data that has been produced from this Laboratory is really quite considerable, as is the scientific achievement. It is surprising to realise that this success was obtained in a comparatively short time, and is a tribute to the user physicists from Daresbury and from the Universities. I personally was greatly impressed by their enthusiasm, dedication and professionalism and I believe that this transmitted itself to and inspired the Laboratory staff who also gave of their best. The rapport between the "external" users and the "in-house" staff was quite remarkable and generated a tangible atmosphere in the "good old days" in the Laboratory.

Many people, over many years, have contributed to the success of the NINA programme. In retrospect, it is clear that the arrival of NINA on the stage ten years ago was very timely, although I am not sure if it looked so to everybody at the time. The credit for this goes clearly to the founding fathers. The physics was aided by a series of Selection Committees which behaved with remarkable commonsense. Selection Committees do not always do this, but under the able chairmanship of the two directors the NINA Selection Committees behaved with restraint and guided the physics without trying to impose their own ideas, which I think is extremely important and very valuable. Of course from time to time they did make mistakes, and we will come across one or two of these on our way through this talk. To be fair, they were quickly recognised and corrected, and did not upset the programme.

A meeting such as this is an occasion for nostalgia and personal reminiscence, not just for physics, and I would like to give my impressions of some of the individuals who have had a part in the making of the Laboratory.

There was very strong support for the physics throughout the Laboratory, even in administration. I stress this because the impression is sometimes given that administrators exist to say "No" and do little else. I must confess that this was my first impression of Harold Rothwell who could say "No" most effectively, and I thought that he was one of those typical bureaucrats to whom one can apply the saying: "It's a poor bureaucrat who can't stall a good idea until even its sponsor is glad to see it dead and buried". I was completely wrong, and quickly discovered that Harold was of a very different stamp from this. His "No" meant "I must be convinced", and once confronted with a properly presented and sufficiently strong case he would provide complete and enthusiastic support. I found him a tower of strength and an invaluable ally: he was very knowledgeable about SRC rules and regulations and although he would never break any (being a good civil servant) he knew an awful lot of loopholes!

The availability of support from within Engineering Division went without saying. Being a Celt, Mike Moore didn't like to hurt anyone's feelings by saying "No", and would usually say "Yes" even to some ridiculous suggestion, and then gently persuade the progenitor of the error of his ways. Of course when real need was demonstrated the necessary effort always became available. Stories about Mike are legion, and I will restrict myself to two for this afternoon.

One of them arose in the early days of LAMP, just at the time of the switch from Alec to Alick. For some reason, which I can't remember, effort was rather scarce in the Laboratory and the LAMP group were impatient to get their cables in (some 600, all told). Being a rather impetuous lot they decided to do the job themselves and they were down in the depths pulling cable when the new Director came along, complete with Lab. Management. Someone, my records don't say who, asked "What are you doing down there, Erwin?" Back came the answer "We're pulling these bloody cables ourselves because there is no effort available from the engineers". I'll draw a discreet veil over the consequences: I think the charge was providing misleading information to the Director. Be that as it may, the cables were pulled (by members of Mike's division) within a week.

The other was the occasion of the first contretemps I had with Mike. Again a temporary shortage of effort was the cause and I wrote a memo in which I complained about this, saying that the engineers were dissipating their energies elsewhere than in high energy physics. Now Mike didn't mind my complaining about engineers doing other things, but he didn't like the word "dissipated" which he felt was a most unsuitable description of Mike Morris and Jim O'Gara and Don Taylor and all the rest. I duly had a lecture from Mike on how to write memos more conducive to securing his co-operation. But of course, words never lasted very long with Mike and very soon he was reaching for his equivalent of the peace-pipe which he kept in the bottom right-hand drawer of his desk. Once he went that way, forgiveness was in the air.

One of the outstanding areas of support was in the field of computing and electronics, where the benefits of having an international authority like Basil Zacharov in charge were very obvious. High energy physics at Daresbury (and I include here the Daresbury supported programme at CERN) owes an incalculable debt to Basil and his staff for their support, encouragement and inventiveness over the years.

A physics programme, of course, is nothing without the accelerator. It is notable that NINA's performance improved steadily, starting well and becoming better and better. This high level of performance has been maintained despite the impending closure and the pressure on the machine group from the new projects. I have a particular personal debt to Jerry Thompson and Godfrey Saxon, who cheerfully accepted a theoretical physicist as their boss. Perhaps the moral of this story is that it pays to let the experts get on with the job and not interfere too much from above. The machine crew merit a special mention, not only for their very obvious competence, but also for their cheerfulness and unflappability at all times of day and night, and in very trying circumstances. I very much enjoyed my contacts with them, and if anyone ever feels like having a free and frank discussion then I recommend having tea in the control room at one o'clock in the morning.

Since time is limited and there is a lot of physics which I am supposed to discuss, I will be able to do no more than take a rather random sample. In this, I want to try and follow the example of the way in which the Laboratory worked, and which was stated explicitly in the 1969 Annual Report. This was the year of the Alec-Alick switch, so I don't know which of the two was responsible for the director's comments. It could have been written by either, and it says this:

"There has been no attempt in what follows to divide the subject matter on a group basis. This is because there is one main purpose for the Laboratory, the preparation and carrying out of experimental physics, and all groups co-operate in the furtherance of this aim".

The best place to start the physics discussion is in 1969, with a look at the experimental area (fig.1) which shows very clearly, even at this stage, the areas in which NINA was to make a significant impact. Starting with the top beam line (beam line 2), we first come to the Liverpool experiment, and I guess that the spectrometer here must hold an all-time record for continuous use. The particular experiment at this time was precise measurement of the differential cross sections for π^0 photoproduction. Behind this is the Mancaster experiment, at the start of the long successful run on pion electroproduction. There is an amusing comment in the papers of the Selection Committee of 16th May 1968, which discusses in turn the various proposals. As far as Mancaster is concerned, it is in a very non-commital mood saying only:

"Mancaster - no account has been taken of this team as yet".

The Mancaster group certainly turned that position around very quickly. Behind Mancaster we find the Mark I tagging system, and the σ_T experiment. The use of tagged photons is one of the areas in which Daresbury has excelled, and the total cross section measurement was one of the first significant experiments to come out of the NINA programme.

On the lower beam line (beam line 1) there is first of all the wide-angle pair production experiment (WAPP), followed by a Glasgow-Sheffield experiment on recoil proton polarisation measurements in electron scattering and then in π^0 photoproduction. The subsequent linking of this group with Liverpool led to one of the most prolific and most significant

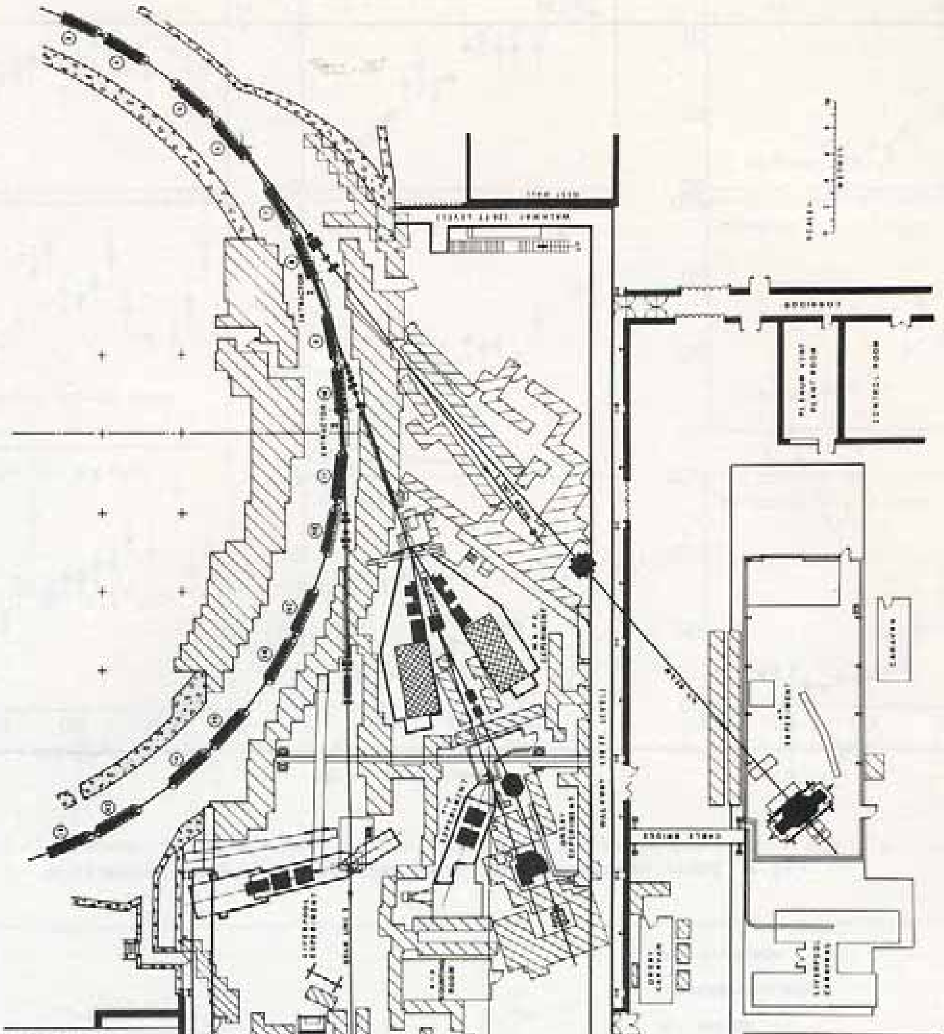


Fig.1 Layout of Experimental Hall in 1969.

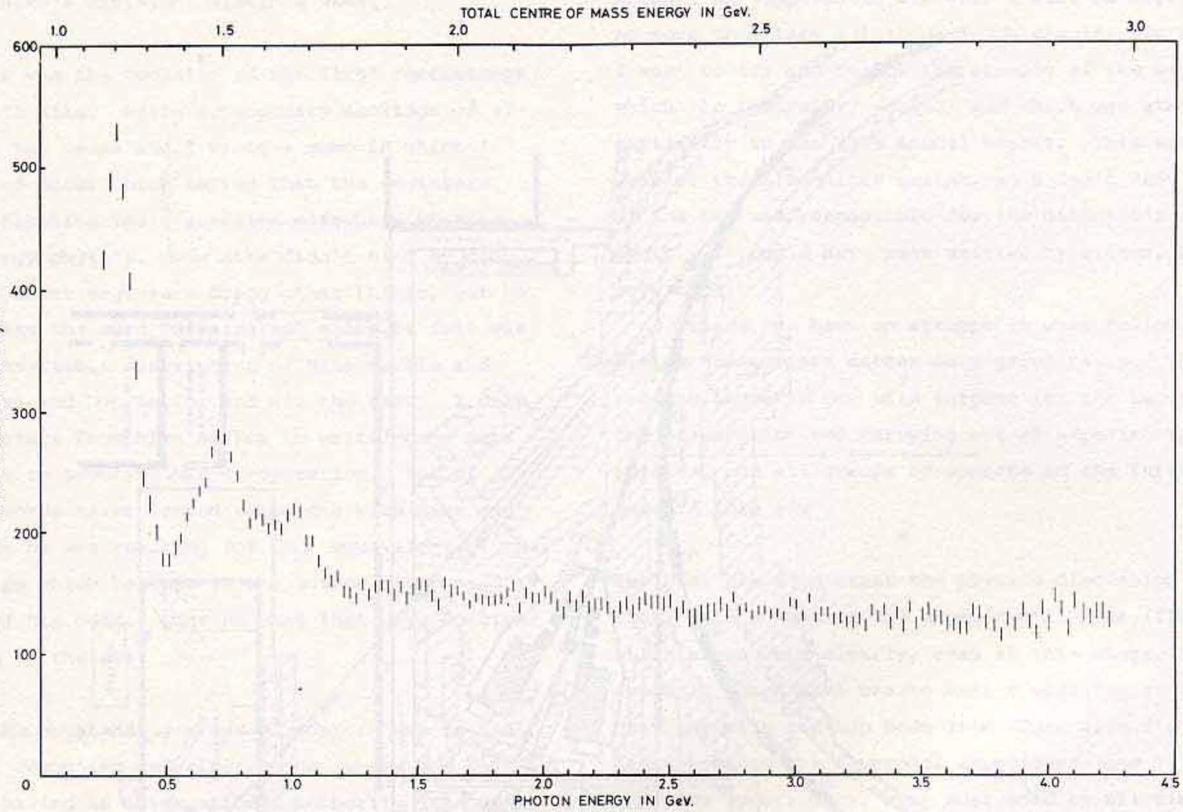


Fig.2 Total photon-proton cross section for hadron production.

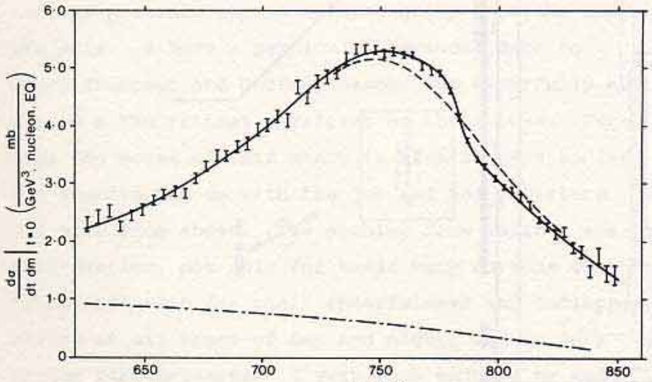


Fig.3 Photoproduction of pion pairs demonstrating ρ-ω interference.

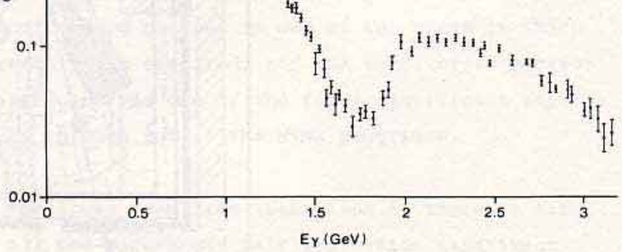


Fig.4 Backward cross section for π^+ photoproduction.

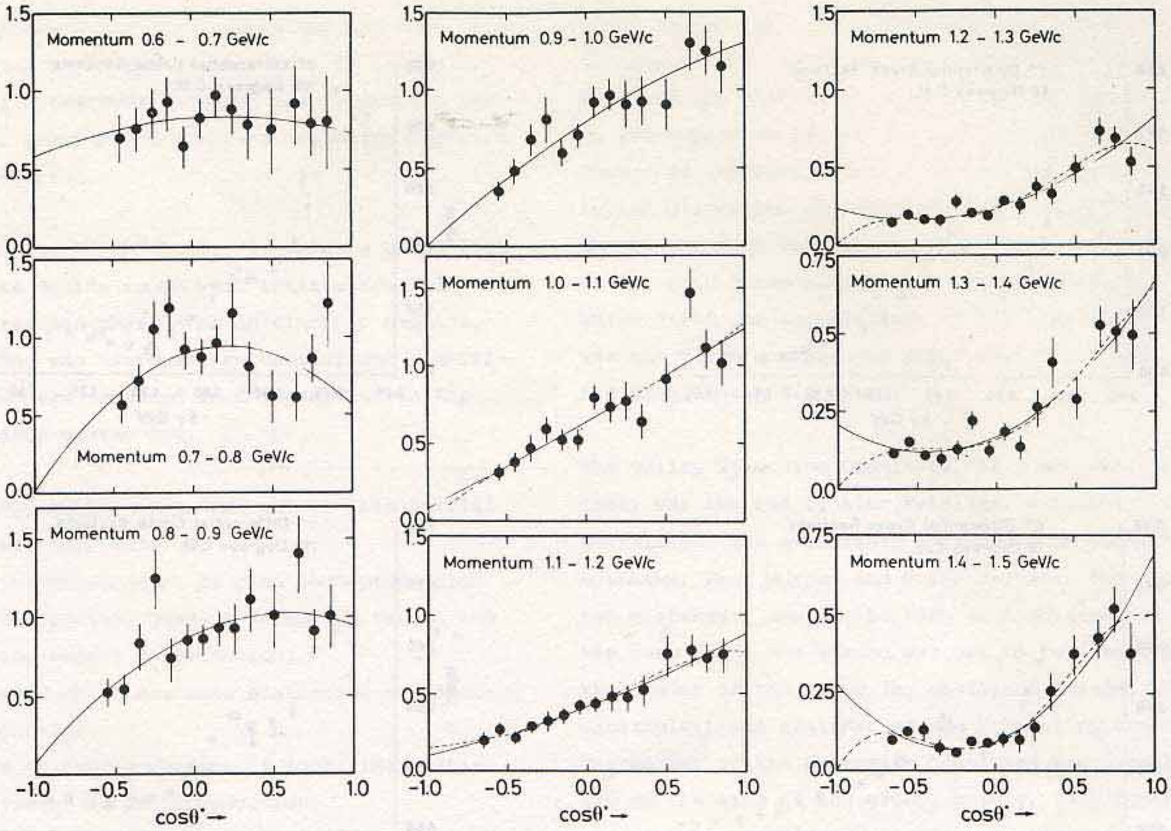


Fig.5 Differential cross sections for the charge exchange reaction $K^0p \rightarrow K^+n$.

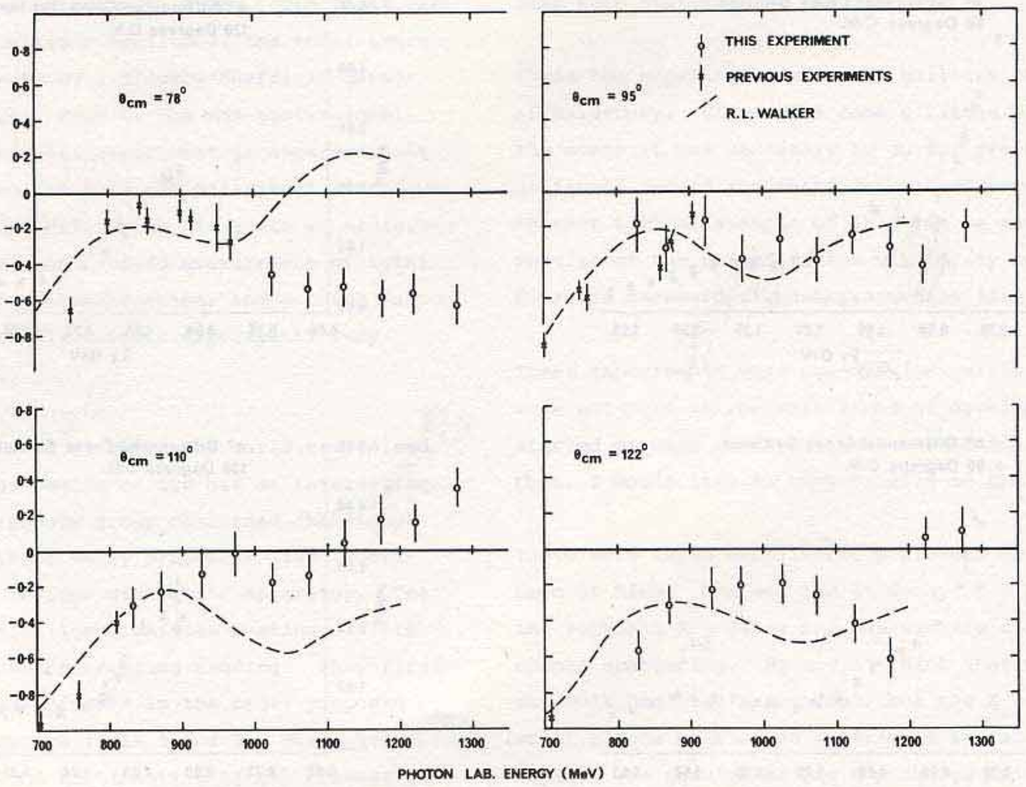


Fig.6 Recoil proton polarisation for $\gamma p \rightarrow p\pi^0$

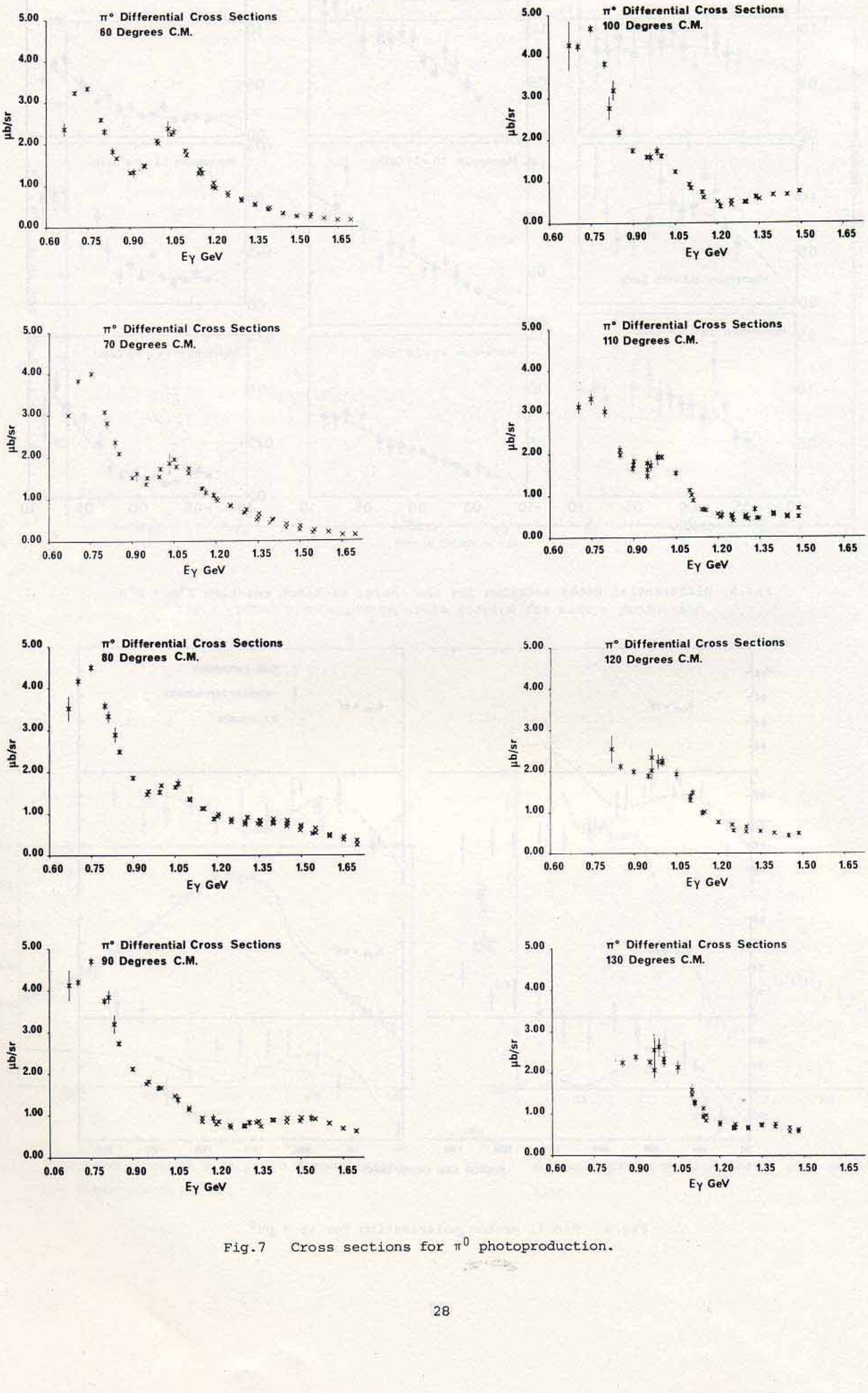


Fig.7 Cross sections for π^0 photoproduction.

outputs of science that the Laboratory has seen. At the end of the beam line is the first of the international collaborations in which the Laboratory indulged, with Orsay and Strasbourg measuring backward π^+ photoproduction.

Finally there is the K^0 beam. It takes a group like Manchester to decide to do weak interaction and strong interaction physics on an electron machine. This programme was extremely successful and significant results were obtained in K^0 decay and in K^0p charge exchange scattering.

The main developments that came out of this initial programme were threefold:

1. Precision measurement of pion photoproduction using a polarised beam, a polarised target and obtaining recoil polarisation.
2. Detailed work on electron scattering and electroproduction.
3. The use of tagged photons to look like multi-body systems in photoproduction.

Before considering these in detail, I would like to mention the two experiments which were the first proof that NINA was coming of age and was a scientific force at the international level. The first was one which I have already mentioned, the total cross section measurements by a Glasgow-Sheffield-Daresbury group (fig.2). Even to the non-professional eye, the beauty of this experiment is apparent. It is interesting to note that the experiment went down to an energy of 200 MeV, which was quite an achievement. This is the outstanding measurement of total cross sections in photoproduction, and nothing has bettered it: a really classic experiment, very beautifully done.

The second experiment by which the world recognised that Daresbury was coming of age has an interesting history. The Daresbury group concerned (WAPP) outlined in one of their early proposals five experiments that could be done with their apparatus. The minutes of the Selection Committee Meeting of 26th September 1968 make interesting reading. They first of all list the experiments in the order proposed by the group, and approve items 1 and 2. They then point out: "That the continuation of the present experiment implies a fairly long time scale, consequently there is no question of approving experiments further down the list". Figure 3 shows the result of an experiment they did not approve and which, as far as I know, never was approved. The experiment measured the cross section for pion pair production in the region of the rho meson, and the dotted line shows the expected result. The actual result is rather different, the very obvious squiggle being due to the fact that the omega meson, which normally decays into three pions, also decays into two pions which gives the interference effect observed. This was the first unambiguous proof that this decay of the omega meson did exist.

The guilty Selection Committee, if I can call them that, was chaired by Alec Merrison, with Bob Voss as secretary. The membership included John Gunn, Paul Matthews, Paul Murphy and Franz Heymann. They made two mistakes: one was to turn down physics which was very good; the second was not to realise that the leader of the group (my ebullient friend, Erwin Gabathuler) was chairman of the Scheduling Committee. One member of the Selection Committee was sympathetic to the aims of the group, namely, Tony Egginton, and with his connivance machine physics was allocated an awful lot of time in the next cycle, which turned into the results of fig.3. The moral here is plain: if you are going to break the rules then make sure that you are right.

These two experiments show the hallmark of the work at Daresbury. Since NINA came a little bit late on the scene it was necessary to go for precision, for difficult second and third generation experiments. Another typical example of this can be seen in the results of the collaboration with Orsay and Strasbourg on backward π^+ photoproduction (fig.4)

These experiments were one-off (or nearly so) and were not part of the main lines of development which started growing at this time. Before dealing with them, I would like to turn briefly to the K^0 physics.

There were three experiments performed on the K^0 beam at NINA. One was the 3π decay of K^0, one was the leptonic $K_{\mu 3}$ decay and one was K^0p charge exchange scattering. By now, I think that the 3π decay work has been superseded, but the $K_{\mu 3}$ decay still stands as good an experiment as any on this topic. The results of the K^0p charge exchange experiment (fig.5) are undoubtedly the best of its kind available and it is unlikely that they can be bettered.

Impressive and significant as these early experiments were, the main impact of the NINA physics has turned out to be the long-term detailed programmes in the three main areas already mentioned.

The first of these programmes started off in an apparently disjointed fashion. These were the recoil polarisation measurements in π^0 photoproduction, made by a Daresbury-Glasgow-Sheffield group, led by Bill Galbraith and the late John Rutherglen. Although their data has been overtaken by new techniques, to which I will come shortly, it is still worth showing (fig.6) because of its historical interest and because it demonstrates that even in the very early days of NINA polarisation measurements were being made. Quite distinct from these were the differential cross section measurements in π^0 photoproduction made by a Liverpool group (fig.7). These are what I call typical Liverpool results. In my young days as a high energy physicist, I remember trying to understand low energy pion nucleon scattering. There was a large quantity of rather poor data at low energies, but out of this one data set shone like a beacon. It was a π^-p charge exchange experiment at 98 MeV, with error bars so small that they could hardly be seen on a figure. Of course they were from Liverpool, and the name associated with them was that of John Holt. To me, this name has become synonymous with precision and accuracy, and a look at fig.7 conveys very vividly what I mean.

It is interesting in looking through the old papers of the Selection Committee to discover how early polarisation measurements were discussed. As we have seen, recoil polarisation measurements were there in the very beginning. A polarised target (Liverpool) was first mentioned in the Selection Committee in September 1968 and a polarised beam (Glasgow-Sheffield) in May 1969 and in an inspired moment the Selection Committee proposed that they be combined and GLS was born.

A polarised photon beam is obtained by coherent scattering of electrons off a diamond, and one would have thought that this presented no serious difficulty for the project, since diamonds are readily accessible, albeit expensive. However as far as I recall the Laboratory went through the whole output of de Beers without finding a single suitable diamond. In the end, one was obtained on loan from DESY and this got the whole project off the ground. This was a typical example of the assistance which Daresbury received from time to time from her sister electron laboratories round the world. It is a pleasure to record the help and encouragement received from them, both in the physics and the machine spheres, particularly from DESY.

The first result to appear with the Liverpool polarised target, π^0 photoproduction at 4.0 GeV, is shown in fig.8. This was another "first" in the international scene and played a significant role in our understanding of the mechanism of high energy photoproduction. Our understanding at this time is indicated by the various lines on the figure which are what various theorists thought the answer ought to be. An amusing addition to the experiment was that as a by-product of their polarisation experiment, the group were able to obtain a differential cross section which was better than the results of previous experiments which had set out to measure cross sections directly. This is shown in fig.9, the black circles being the results of the Liverpool experiment and the white diamonds those of another experiment.

At lower energies (i.e. below 2.5 GeV) the GLS collaboration cornered the market. The combination of polarised beam and polarised target is very powerful. In pion photoproduction, if the target is polarised perpendicular to the production plane (which is the conventional way) three quantities can be measured in addition to the differential cross section. These are the polarised target asymmetry T, the polarised beam asymmetry Σ and a quantity equivalent to the recoil polarisation P. If the target is polarised in the production plane (this was a considerable technical achievement on the part of the Liverpool group) two correlation quantities, G and H, can be measured. These latter two measurements are unique. The experiments undertaken are tabulated in fig.10 and typical results are given in fig.11 (Σ in $\gamma p \rightarrow \pi^0 p$), fig.12 (T in $\gamma p \rightarrow \pi^0 p$), fig.13 ($\Sigma$, P, T in $\gamma p \rightarrow \pi^+ n$) and fig.14 (H in $\gamma p \rightarrow \pi^0 p$). Where curves are shown in these figures they are the predictions from earlier analyses.

These extensive data have allowed detailed amplitude analyses to be performed, which have yielded precise information on the electromagnetic interactions of the baryon resonance. The significance and extent of the achievement is most readily seen by comparing

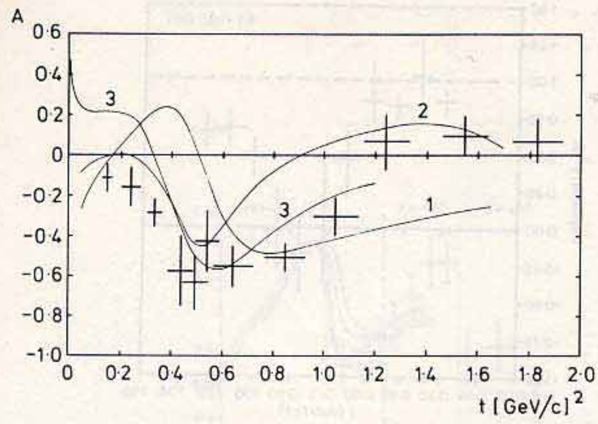


Fig.8 Polarised target asymmetry for π^0 photoproduction at 4 GeV.

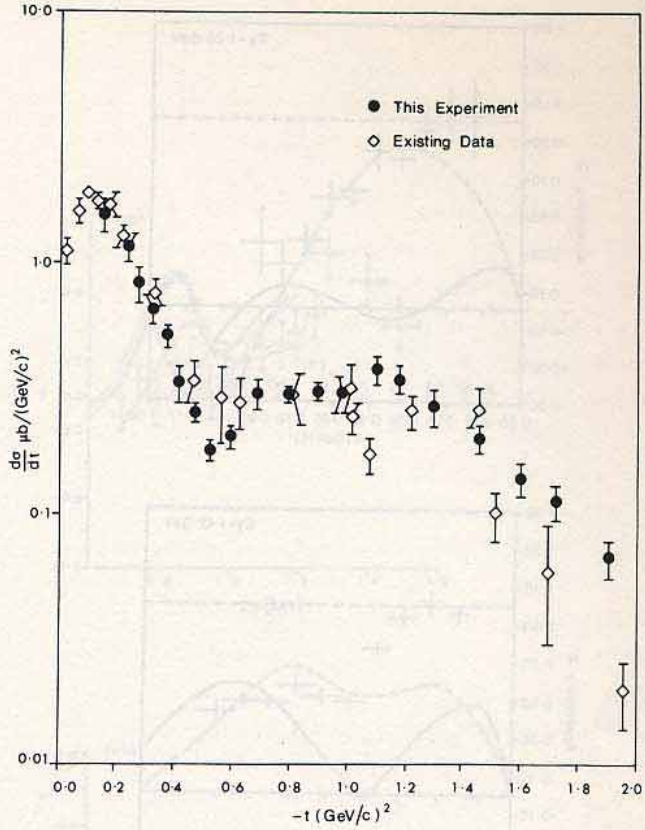
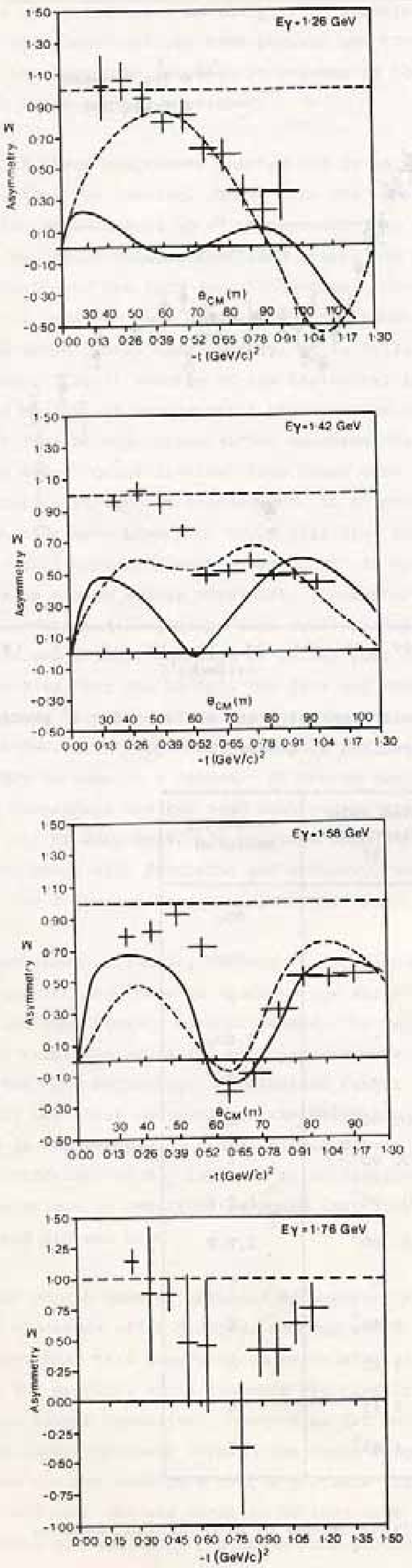


Fig.9 Differential cross sections for π^0 photoproduction at 4 GeV.

Reaction	E_γ (GeV)	Target Poln Direction	Beam Poln Direction (ϕ)	Parameter Measured
$\gamma p \to \pi^0 p$	0.7 - 1.7	-	-	$d\sigma_0$
$\gamma p \to \eta p$	0.95 - 2.8	-	-	$d\sigma_0$
$\gamma p \to \pi^0 p$	0.85 - 1.25	-	-	P
$\gamma p \to \pi^0 p$	2,4	± y	-	T,$d\sigma_0$
$\gamma p \to \pi^0 p$	0.7 - 1.5	± y	-	T
$\gamma p \to \pi^0 p$	1.3 - 2.6	-	0, 90°	Σ
$\gamma p \to \eta p$	2.3 - 3.2	-	0, 90°	Σ
$\gamma p \to \pi^+ n$	0.5 - 2.5	± y	0, 90°	Σ,T,P
$\gamma p \to \pi^0 p$	1.15 - 2.25	± y	0, 90°	Σ,T,P
$\gamma p \to \eta p$	4	± y	-	T
$\gamma p \to \pi^+ n$	0.65 - 2.1	± x	± 45°	H
$\gamma p \to \pi^+ n$	0.65 - 2.1	± z	± 45°	G
$\gamma p \to \pi^0 p$	1.4 - 2.1	± x	± 45°	H
$\gamma p \to \pi^0 p$	1.4 - 2.1	± z	± 45°	G

Fig.10 Measurements made by the Glasgow, Liverpool and Sheffield Groups in pion photoproduction.



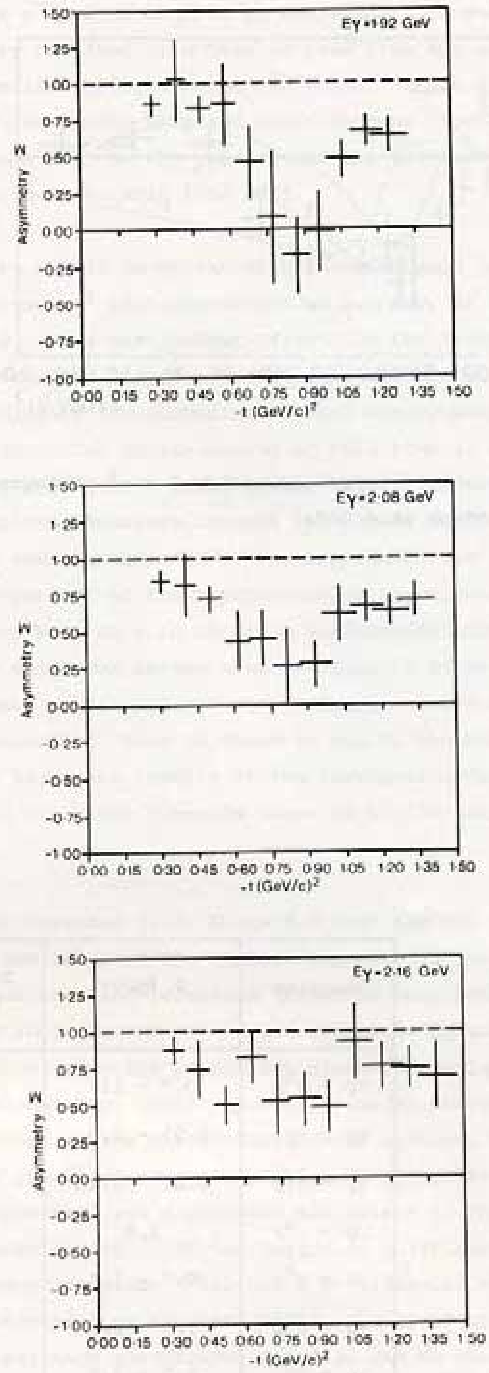


Fig.11 Results for the polarised beam asymmetry, Σ, in π^0 photoproduction.

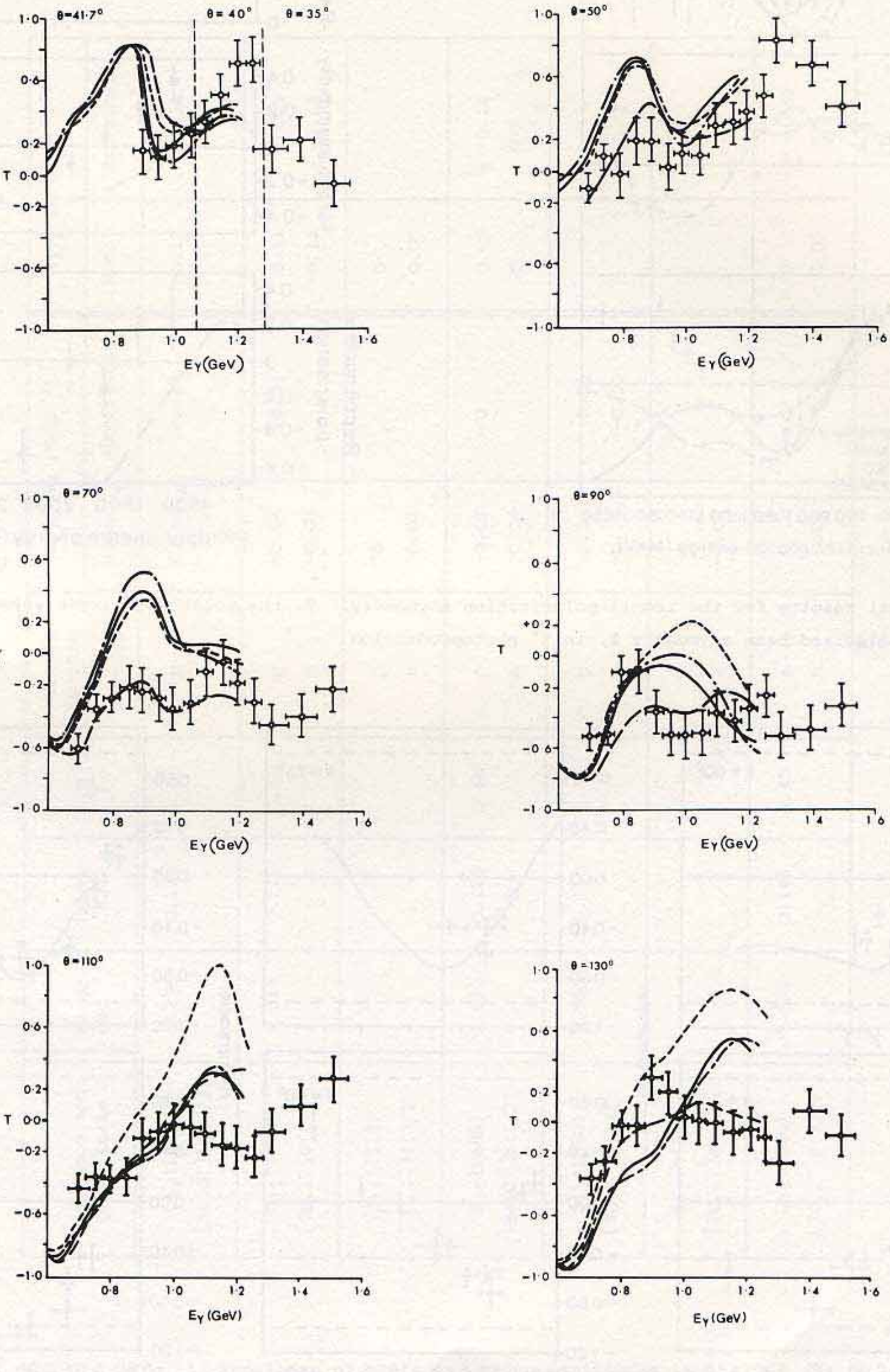


Fig.12 The polarised target asymmetry for $\gamma p \rightarrow p\pi^0$.

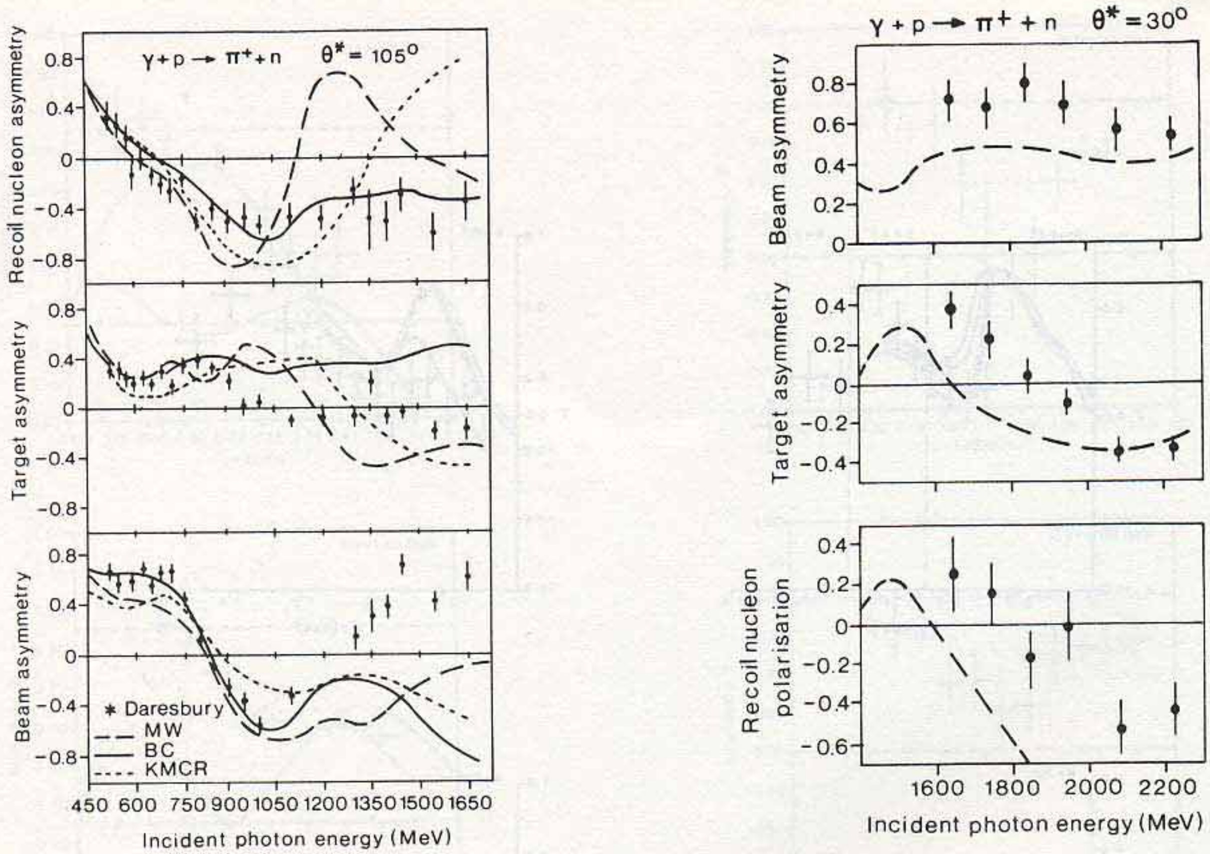


Fig.13 Typical results for the recoil polarisation asymmetry. P, the polarised target asymmetry T, and the polarised beam asymmetry Σ, in π^+ photoproduction.

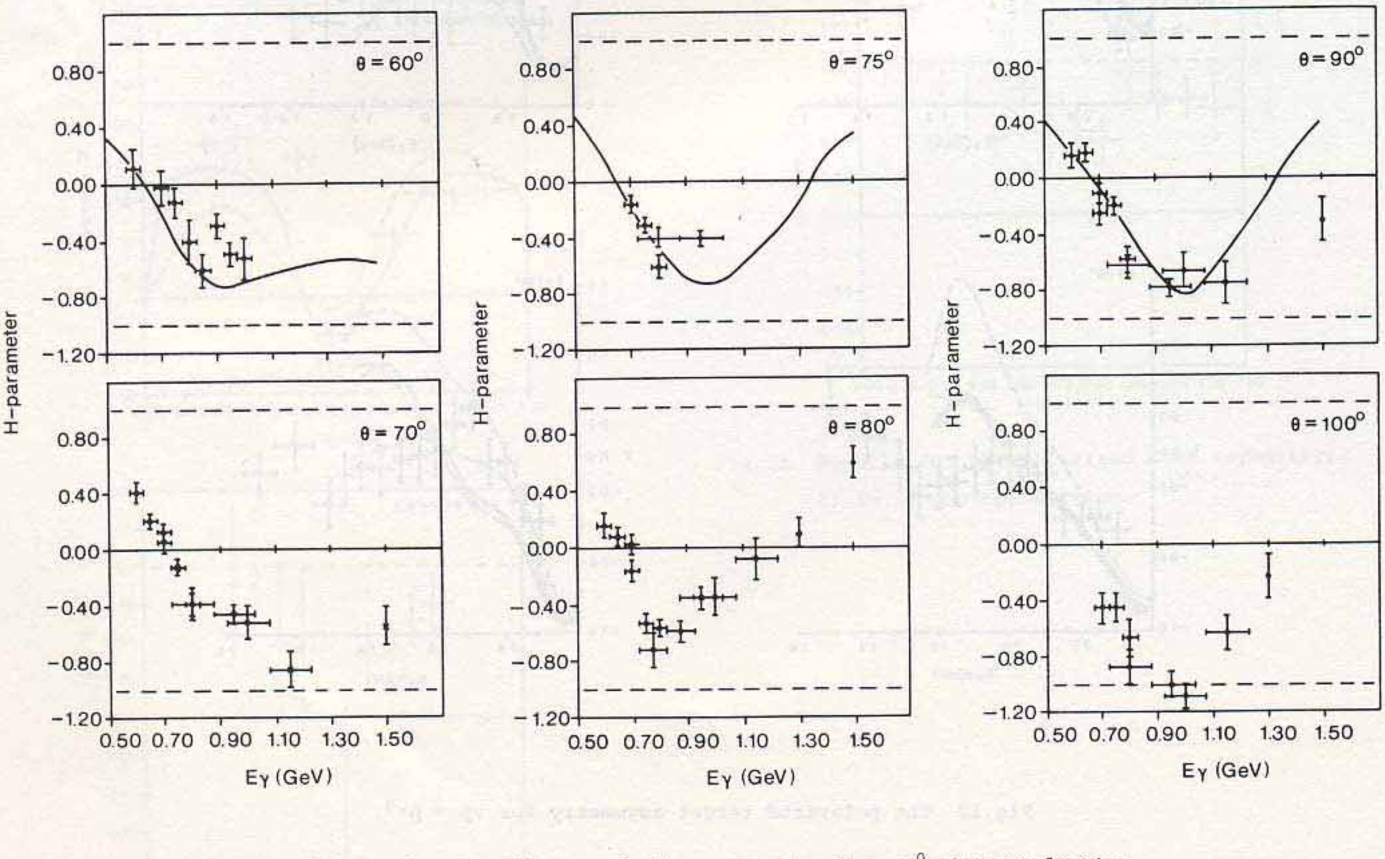


Fig.14 Results for the correlation parameter H in π^0 photoproduction.

Resonance and Quark State	Mass GeV	Width GeV	$\frac{\Gamma_\pi}{\Gamma}$	Target (proton or neutron)	$A_{1/2}$ in $(GeV)^{-1/2}$ Model	$A_{1/2}$ in $(GeV)^{-1/2}$ Experiment	$A_{3/2}$ in $(GeV)^{-1/2}$ Model	$A_{3/2}$ in $(GeV)^{-1/2}$ Experiment
$P_{33}(1236)$ $[56,0^+]10(3/2)$	1.236	0.120	1.00	p	-0.10	-0.14	-0.17	-0.24
$D_{13}(1518)$ $[70,1^-]8(1/2)$	1.515	0.115	0.50	p	-0.04	-0.03	0.11	0.15
				n	-0.03		-0.11	-0.13
$D_{13}(1518)$ $[70,1^-]8(3/2)$				p	0		0	
				n	0.01		0.05	
$F_{15}(1688)$ $[56,2^+]8(1/2)$	1.690	0.125	0.60	p	-0.01	≈ 0	0.07	0.14
				n	0.04		0	≈ 0
$S_{11}(1550)$ $[70,1^-]8(1/2)$	1.550	0.130	0.35	p	0.16	0.08		
				n	-0.11	-0.10		
$S_{11}(1550)$ $[70,1^-]8(3/2)$				p	0			
				n	-0.02			
$D_{15}(1680)$ $[70,1^-]8(3/2)$	1.675	0.145	0.45	p	0	≈ 0	0	0.04?
				n	0.04		0.05	

Fig.15 Knowledge of photoproduction amplitudes in 1969.

Resonance	Amplitude	DLR	KMORR	C	BC	$SU(6)_W$	Quark Model
$S_{11}(1535)$	$A^p_{1/2}$	78 ± 20	89 ± 21	82 ± 7	65	72	156
	$A^n_{1/2}$	5 ± 15	-52 ± 21	-88 ± 11	-106	-74	-108
$S_{11}(1700)$	$A^p_{1/2}$	29 ± 18	52 ± 11	44 ± 18	47	48	0
	$A^n_{1/2}$	-6 ± 31	-55 ± 39	-103 ± 10	-44	-53	30
$D_{13}(1510)$	$A^p_{1/2}$	-8 ± 15	-19 ± 21	-9 ± 4	-13	-36	-34
	$A^p_{3/2}$	171 ± 12	170 ± 7	162 ± 4	155	165	109
	$A^n_{1/2}$	-89 ± 19	-70 ± 21	-67 ± 4	-54	-28	-31
	$A^n_{3/2}$	-155 ± 19	-128 ± 7	-133 ± 3	-136	-125	-109
$D_{13}(1700)$	$A^p_{1/2}$	-48 ± 50	22 ± 39	-12 ± 10	-4	0	0
	$A^p_{3/2}$	-6 ± 14	61 ± 32	-12 ± 10	-27	0	0
	$A^n_{1/2}$	-21 ± 98	73 ± 67	81 ± 15	33	27	-10
	$A^n_{3/2}$	-26 ± 67	51 ± 81	107 ± 25	29	-11	-40
$D_{15}(1670)$	$A^p_{1/2}$	19 ± 21	7 ± 24	27 ± 9	10	0	0
	$A^p_{3/2}$	14 ± 4	17 ± 21	15 ± 6	17	0	0
	$A^n_{1/2}$	-29 ± 23	-43 ± 24	-52 ± 3	-62	-48	-38
	$A^n_{3/2}$	-68 ± 20	-90 ± 53	-83 ± 7	-78	-68	-53
$S_{31}(1650)$	$A_{1/2}$	-10 ± 17	27 ± 18	44 ± 27	45	99	47
$D_{33}(1685)$	$A_{1/2}$	54 ± 29	79 ± 32	101 ± 11	119	110	88
	$A_{3/2}$	72 ± 14	61 ± 39	116 ± 24	102	101	84

Resonance	Amplitude	DLR	KMORR	C	BC	$SU(6)_W$ (a)	$SU(6)_W$ (b)	Quark Model
$P_{13}(1770)$	$A^p_{1/2}$	25 ± 34	26 ± 56	22 ± 12	105	-25	39	100
	$A^p_{3/2}$	-87 ± 57	-12 ± 42	-16 ± 16	-62	38	4	-30
	$A^n_{1/2}$	13 ± 45	14 ± 49	-37 ± 22	6	-11	1	-30
	$A^n_{3/2}$	-83 ± 90	-23 ± 81	-38 ± 15	48	-9	-18	0
$F_{15}(1690)$	$A^p_{1/2}$	27 ± 19	-25 ± 11	-13 ± 2	-3	-28	-26	-10
	$A^p_{3/2}$	163 ± 11	96 ± 14	135 ± 6	132	94	98	60
	$A^n_{1/2}$	31 ± 28	33 ± 25	21 ± 1	32	40	38	30
	$A^n_{3/2}$	-21 ± 28	-15 ± 53	-15 ± 4	-30	-33	-36	0
$P_{31}(1910)$	$A_{1/2}$	0 ± 25	18 ± 18	-9 ± 13	-51	14	16	-30
$P_{33}(1900)$	$A_{1/2}$		-34 ± 28			-3	-19	30
	$A_{3/2}$		-9 ± 32			-17	-10	-50
$F_{35}(1890)$	$A_{1/2}$	19 ± 27	25 ± 35	-3 ± 9	-5	31	36	-20
	$A_{3/2}$	78 ± 20	-44 ± 63	-21 ± 36	-23	-11	-5	-90
$F_{37}(1920)$	$A_{1/2}$	-88 ± 25	-67 ± 25	-53 ± 5	-67	-50	-50	-50
	$A_{3/2}$	-80 ± 21	-80 ± 56	-38 ± 14	-70	-64	-65	-70

Fig.16 Knowledge of photoproduction amplitudes in 1975.

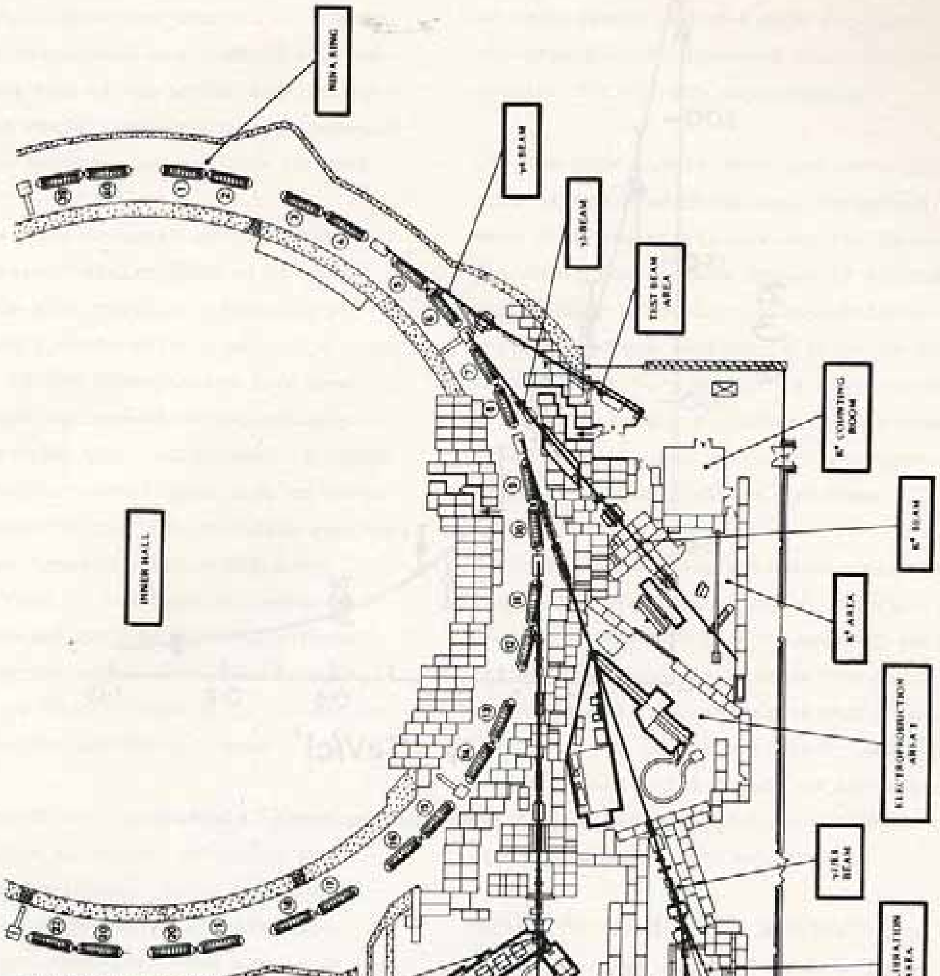


Fig.17 Layout of Experimental Hall in 1972.

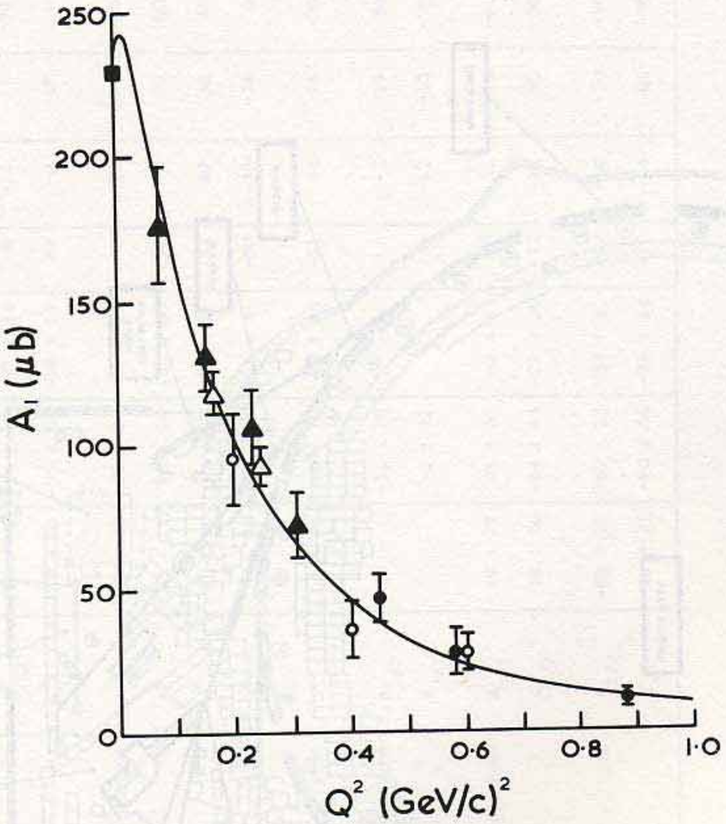


Fig.18 Threshold cross section for π^+ electroproduction.

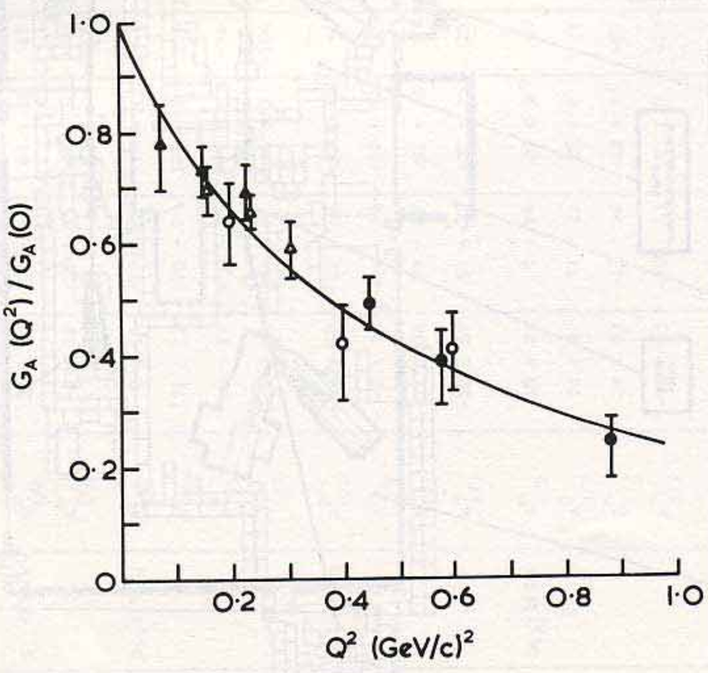


Fig.19 Axial vector from factor of the nucleon determined from π^+ electroproduction.

what was known at the time of the Liverpool Conference (1969) (fig.15) and what was known in 1975 (fig.16). These results have been crucial to the development of our understanding not just of electromagnetic interactions but of the whole idea of currents in general and their interaction with hadrons, and are based almost entirely on the work at NINA.

We should recall that the original evidence for the existence of these baryon states studied at NINA came largely from the pion nucleon scattering programme at Nimrod, and I don't think that it is going too far to say that if the laboratories had done nothing else but these two series of experiments they would have justified their existence. I think it is unfortunate that to obtain this kind of information which is crucial to our understanding requires experiments which are detailed, which are long, which are precise. They do not have the impact of sudden discovery, but are nonetheless significant. This is recognised by the experts in the field, but unfortunately since it doesn't have this instant impact it is not always recognised elsewhere.

The second major area of activity was in electroproduction, centred on the Mancaster series of experiments and that of the PEP group, which was the second international collaboration at Daresbury, this time with Pisa and Frascati. The Mancaster programme has been mainly concerned with pion electroproduction in the resonance region, to determine the transition form factors of the baryon resonances. Their work has also led to a determination of the form factor of the pion. An interesting aspect of this work was the relationship between the Mancaster group and their counterparts at DESY, a combination of mutual respect and friendly rivalry. A consequence was that the two groups worked on somewhat different kinematical regions, with enough overlap to show that they were compatible. The outcome of the two sets of measurements has been a remarkably wide coverage of the whole area of pion electroproduction, much greater than either group could have achieved on their own.

It is perhaps useful at this stage to look at the experimental hall as it was in 1972 (fig.17). The GLS experiment now occupies what was formerly the Liverpool area (now labelled the Photoproduction Area) and Mancaster are as they were in 1969, in what is now labelled Electroproduction Area 1. The main changes have taken place in the El beam, with the appearance of LAMP (large angle meson production at this stage) and the Mark II tagging system, and the replacement of one of the WAPP arms by a neutron counter for the PEP experiments.

The PEP experiments were concerned primarily with pion electroproduction near threshold. One of their more interesting results was the determination of the axial vector form factor of the nucleon (which also occurs in neutrino interactions). The basic threshold cross section is shown in fig.18 and the axial vector form factor in fig.19. The PEP collaboration was very productive and successful, although I don't think that Daresbury was quite the same again after the Italians had been.

I remember that they were inveterate smokers, which led to Don Braben putting up a notice in the control room to the effect that "Quando il pavimento e pieno, usare il porta cenere" (when the floor is full - use the ashtrays). So pervasive was this smoking among the PEP group that one of their magnets decided to get in on the act and went up in smoke early one morning. I am told that it was still possible to fry an egg on it that afternoon.

Talking of the Italians and the impact they had on Daresbury makes me think of the impact they had on Miss Miles, and vice versa. For visiting physicists (and I include in this the university community) Miss Miles was an integral part of the Daresbury scene. She was at her best when the hostel was Norton Lodge, in pre-Hinstock Mount days. One can tell many stories about the place, but there are two in particular which epitomise the way in which she looked after "her boys".

On one occasion I was returning from Italy after a visit with Don Braben to discuss some aspects of the PEP programme. I was returning on my own, coming back to Norton Lodge for a few nights, and as seems to be customary with Italian trips all the flights went wrong. I was due back in Manchester at 19.00 from London and actually arrived about midnight on a much delayed flight from Milan. To my surprise, a lab. car was waiting for me, so I asked the driver "Have you been here since seven o'clock?" "No", he replied, "Miss Miles told me when to come". Apparently, when she realised that I had missed my flight she had taken the trouble to work out how I could possibly reach Manchester that evening.

The second occasion was on one of our study weekends. As usual there was a big representation from DESY and Bonn at the meeting, which coincided with the World Cup Final between England and Germany. The game was being televised on the Sunday evening, so we 'phoned Miss Miles and enquired whether a study weekend's worth of physicists could take over Norton Lodge lounge for the match. "Come along", she said, so we did and not only was there a colour television for us, she had procured a crate or two of beer, and numerous pork pies and made a vast pile of sandwiches.

That is by way of an aside, so let us get back to physics, with the Experimental Area as it is now (fig.20) and let us restart with the Mancaster electroproduction programme. Very interesting and dramatic things can happen as soon as a photon becomes virtual. One of my favourite results is shown in fig.21, which is the forward differential cross section for π^+ production. The line is the average of the photoproduction data, and you can see the very rapid and very significant change in the cross section as the photon becomes virtual. Another intriguing one is the measurement of eta-meson production by electrons, fig.22. The various lines are theoretical guesses at what the answer ought to be: mainly wrong, as usual!

One of the difficulties in presenting electroproduction data is the sheer quantity of information. It is multidimensional, there being four variables: energy, momentum transfer and both polar and azimuthal angles. Typical experimental results for π^0 electroproduction are shown in fig.23, and the various coefficients which can be extracted from the data in fig.24.

As in photoproduction, the significance and extent of the achievement can best be gauged by comparing what was known at the Liverpool Conference with the results presented at the SLAC Conference. At Liverpool, the talk was given by Arthur Clegg (who was the initial driving force behind the electroproduction programme) and he was able to present reliable results on the form factor of only one resonance. The situation at the time of the SLAC Conference is summarised in fig.25.

The third topic which one has to mention in connection with Daresbury is the use of tagged photons. We have already seen the total cross section results obtained in the early days with the Mark I tagging system. The Mark II has led to a whole series of experiments, LAMP 1/2, LAMP 1 and LAMP 2. LAMP 1/2 was a so-called "quick experiment" which was inserted between LAMP 1 (large angle meson production) and LAMP 2 (large aperture magnet project). I don't know why it wasn't called LAMP 3/2.

LAMP 1/2 had one specific and very significant object in view. All the analyses of photoproduction and electroproduction make specific assumptions about the nature of electromagnetic interaction. Some data appeared which could be interpreted as implying that these assumptions were wrong, and that the interaction was more complex than had been supposed. The LAMP group set out to do a conclusive experiment on this, and they succeeded. They had to measure the ratio of $\gamma n \to \pi^0 n / \gamma p \to \pi^0 p$. Results are shown in fig.26 and confirm that the normal assumptions are correct to better than 3%.

The bulk of the LAMP work has been in multiparticle systems. Much of the work is still at the analysis stage, and of course LAMP 2 is still taking data. An example of the quality of the data is given in fig.27, which shows the backward production of ρ and f mesons, decaying into two pions. It is clear that these experiments will produce large quantities of high quality data, and the group is already happily preparing to upset numerous theoretical apple-carts.

It is perhaps surprising how the output of physics from NINA is still expanding, and it will certainly continue for a long time after NINA switches off. Obviously, when a machine is coming to the end of its active life there is a feeling of sadness, but in NINA's case it is more than compensated by a really positive feeling of success. The machine has done the job it was built to do, and has done it beautifully. I was discussing with Alick Ashmore today that it was a good thing in the end that NINA was limited to 5 GeV and was not built as a 15 or 20 GeV machine. This forced the programme into resonance physics which has turned out to be a real goldmine. However the energy was high enough, both in single pion and multi-meson systems, for reaction physics to be effective and valuable, and so one got the best of both worlds.

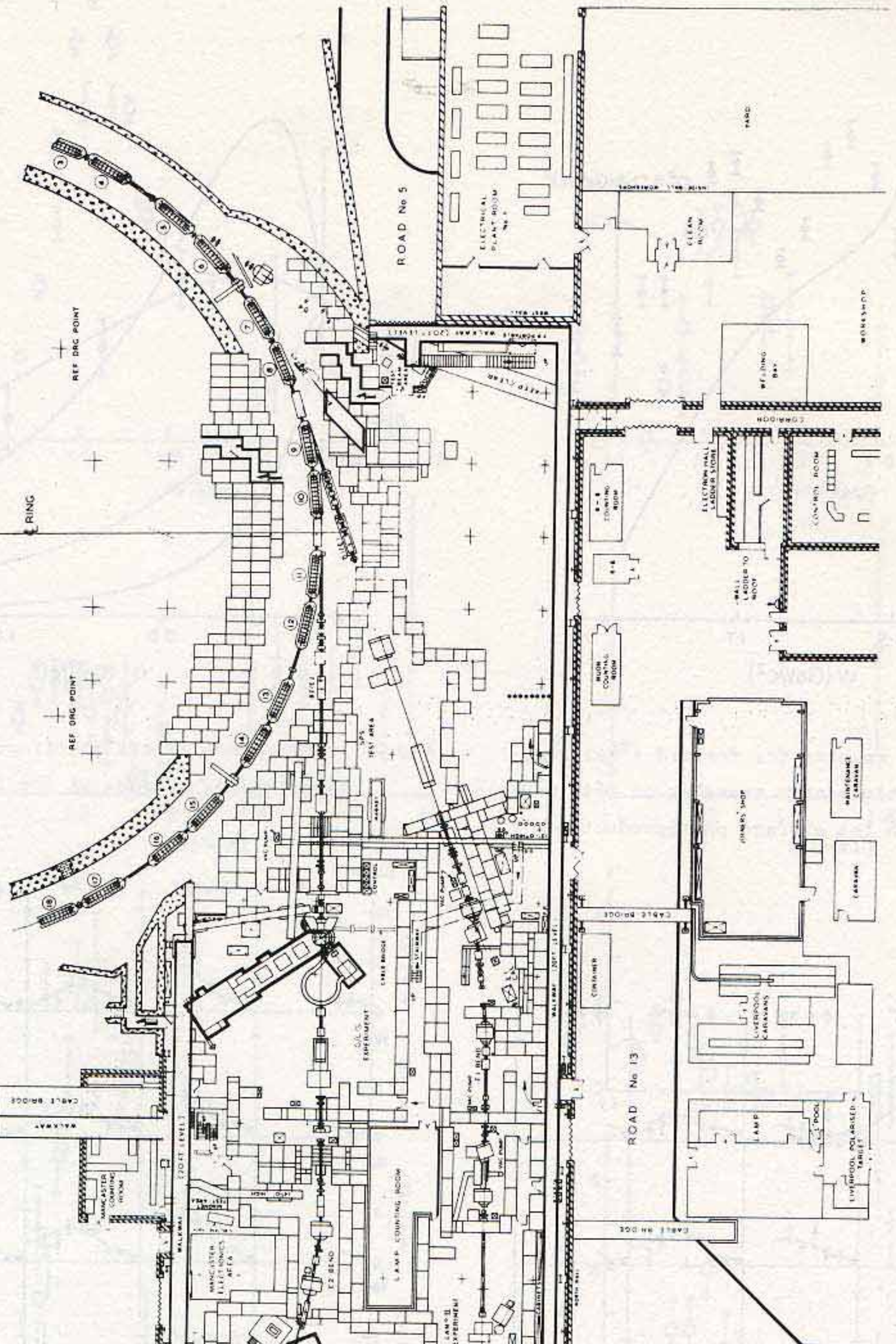


Fig.20 Layout of the Experimental Hall in 1976.

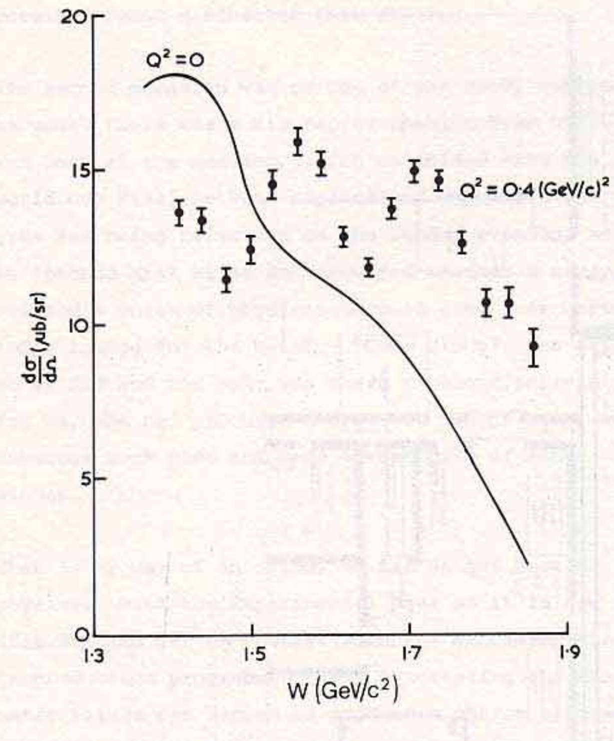


Fig.21 Experimental results for forward π^+ electroproduction at momentum transfer of 0.4 $(GeV/c)^2$ compared with the average photoproduction value.

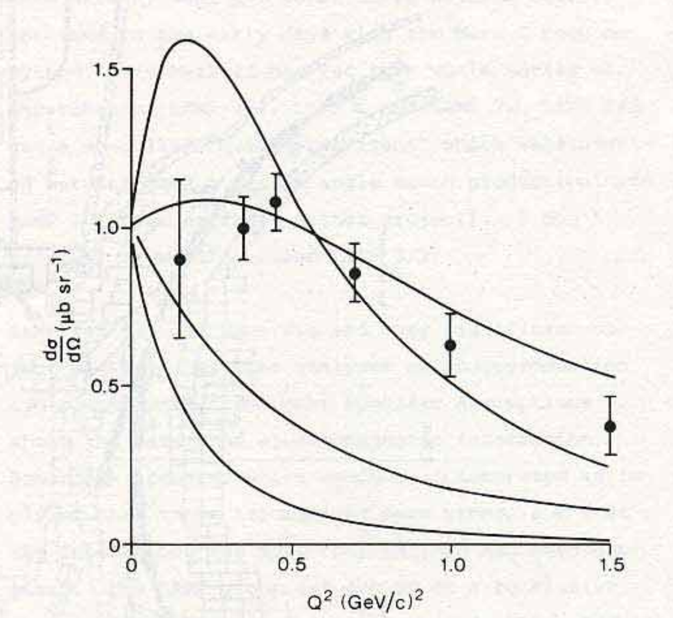


Fig.22 Measurement of eta electroproduction with theoretical guesses at the result.

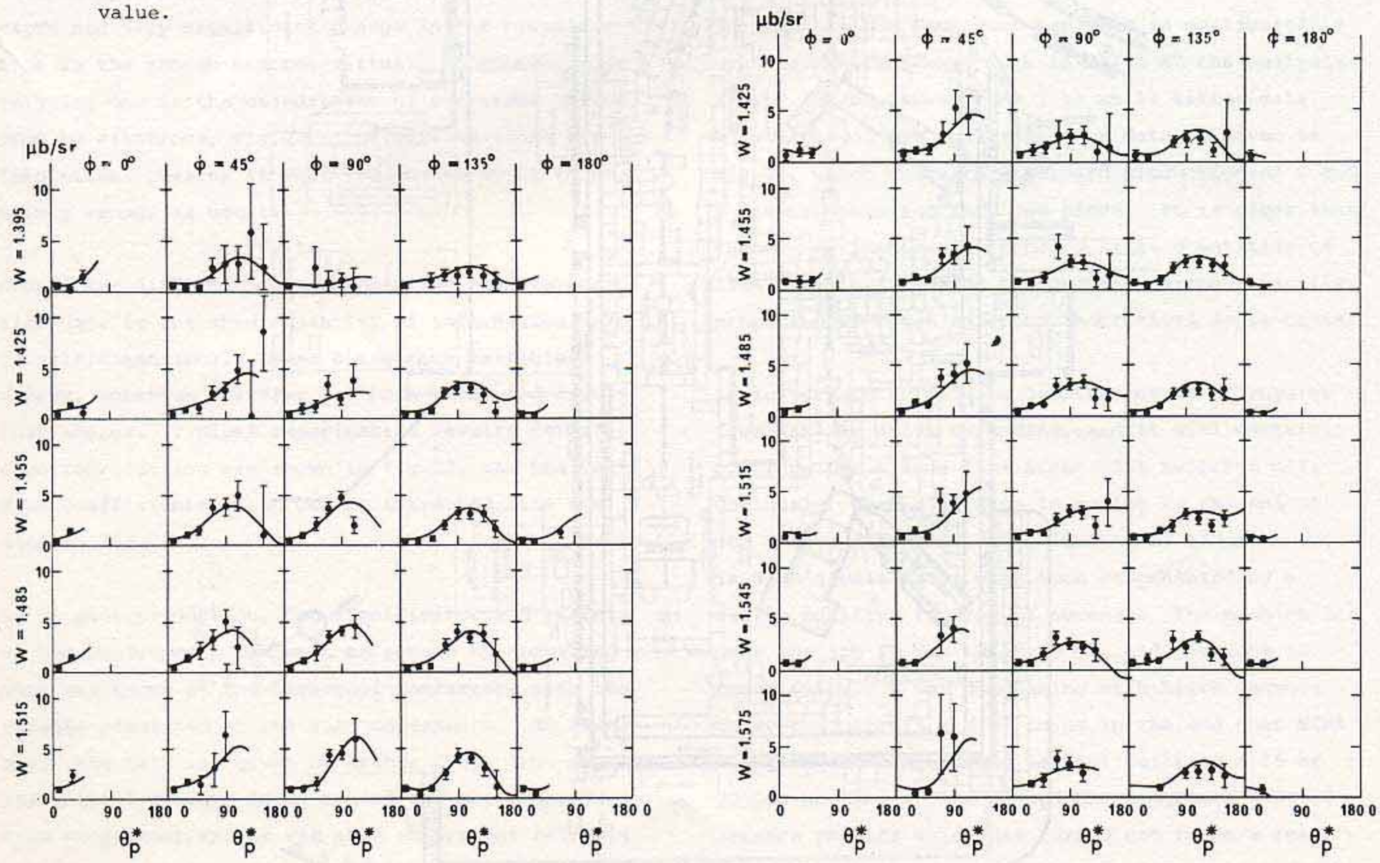


Fig.23 Typical results for π^0 electroproduction. The lines are the results of a nine-coefficient fit.

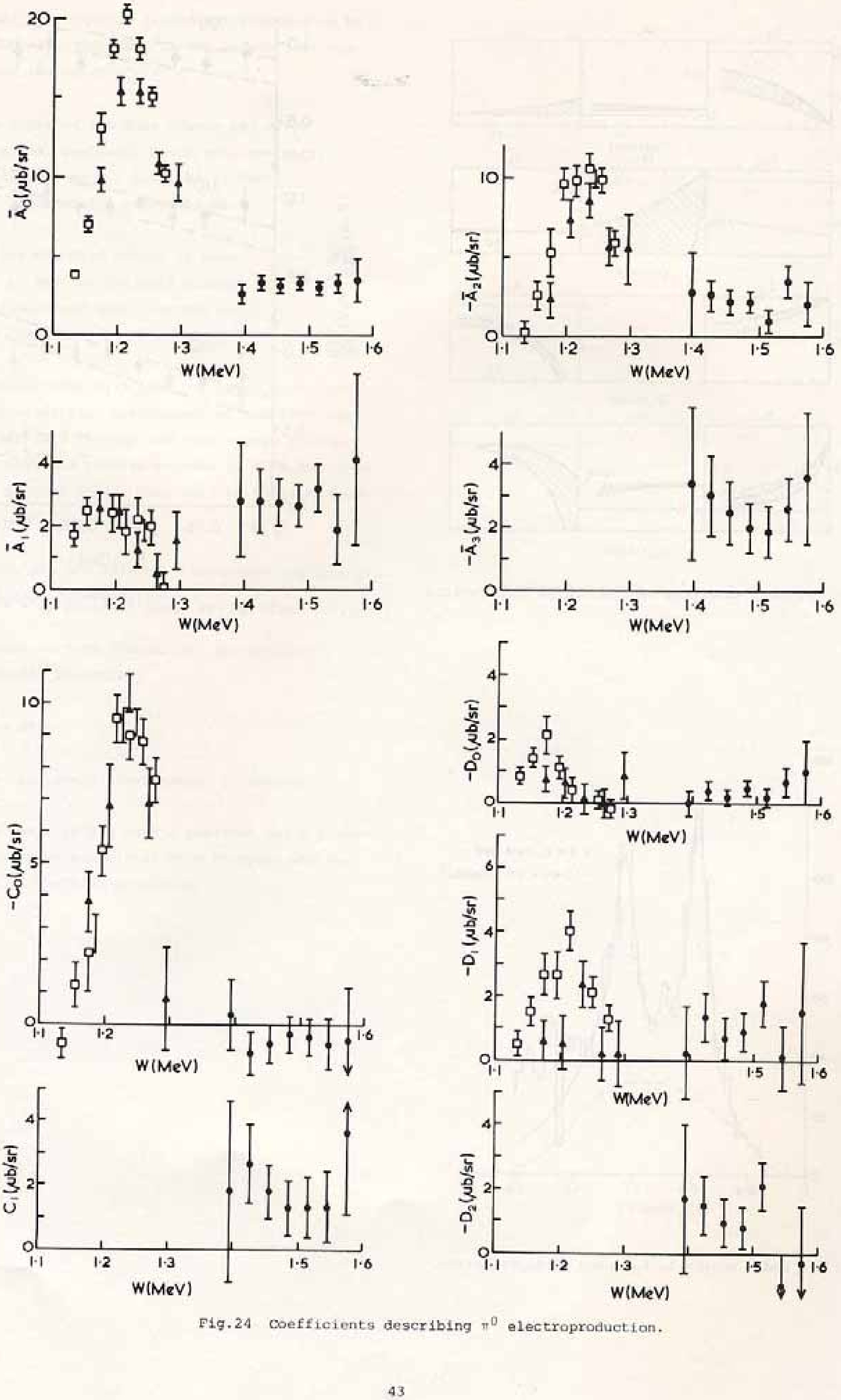


Fig.24 Coefficients describing π^0 electroproduction.

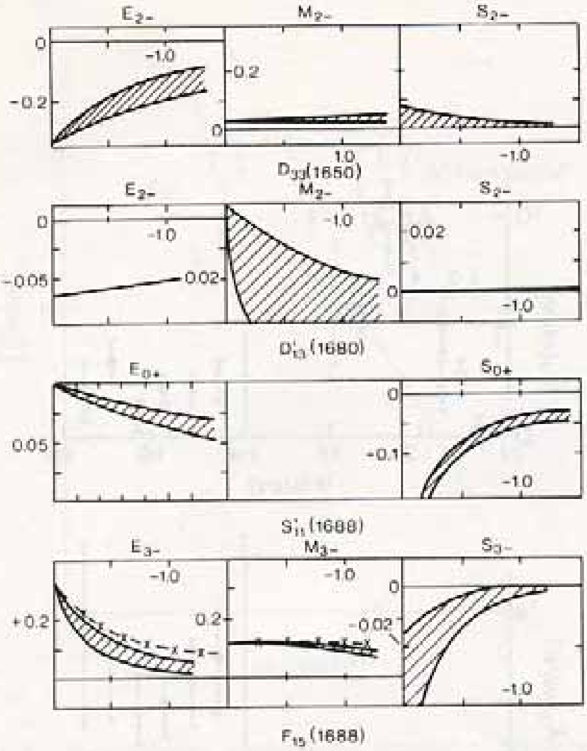


Fig.25 Present knowledge of transition form factors.

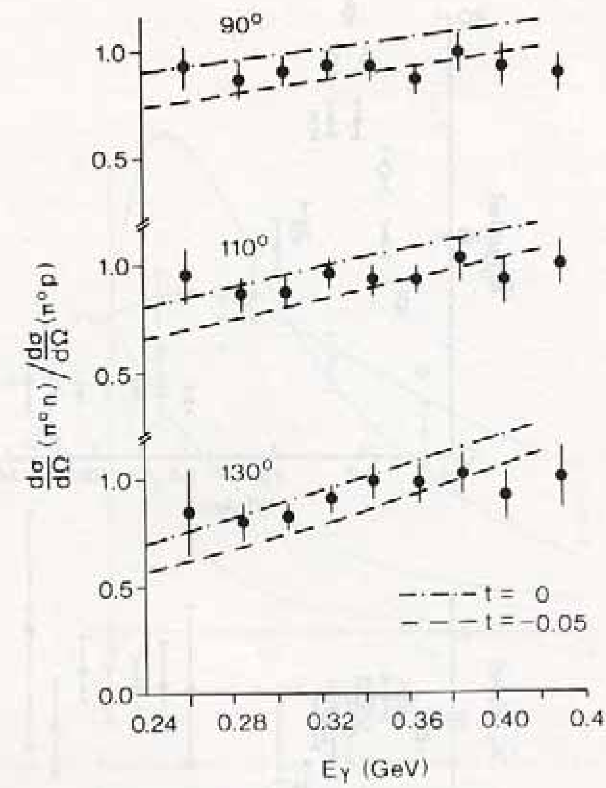


Fig.26 Ratio of the cross sections $\gamma n \to \pi^0 n / \gamma p \to \pi^0 p$.

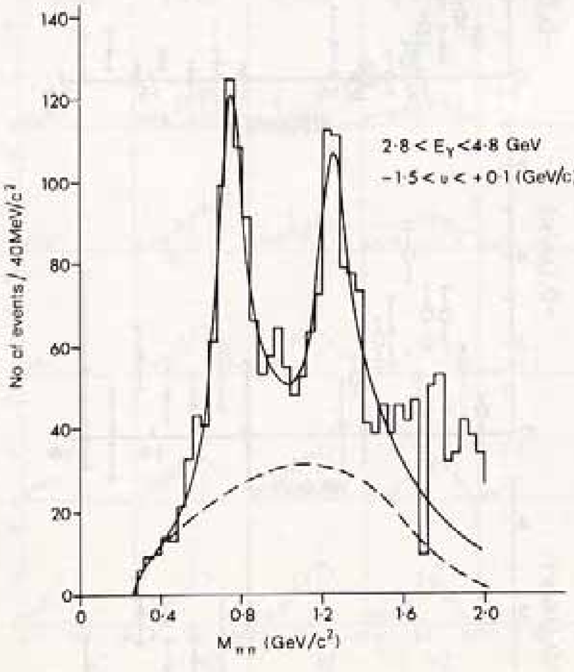


Fig.27 ρ and f mesons in backward photoproduction.

The situation of NINA is rather aptly summed up by William Dunbar (1460-1520) in two verses from his 'Lament for the Makaris':

"The state of man dois change and vary,
Now sound, now seik, now blyth, now sary,
Now dansand merry, now like to dee:
Timor mortis conturbat me

Sen for the deid remeid is none,
Best is that we for deid dispone,
Eftir our deid that live may we:
Timor mortis conturbat me."

In two senses, NINA will live on. Firstly in the very solid scientific achievement of the NINA programme. Secondly through the high energy physics community that was created because of NINA and which developed through NINA. They will be found at CERN on:

The e/γ project (Glasgow, Lancaster, Manchester, Sheffield, Daresbury).

The muon project (Lancaster, Liverpool, Sheffield, Daresbury)

and at DESY on

PETRA (Lancaster, Manchester, Daresbury).

For my own part, it has been a pleasure and a privilege to have been associated with everyone who has taken part in the NINA programme.

INTRODUCTION TO PROFESSOR ASHMORE

Professor J.R. Holt

The last speaker this afternoon is the third of our four speakers who at one time or another worked at Liverpool. Alick Ashmore carried out research with the small cyclotron immediately after the War at Liverpool and later on worked on the synchrocyclotron before moving to Queen Mary College and becoming Professor of Nuclear Physics. Whilst there he carried out work at Rutherford Laboratory and spent a sabbatical year at Brookhaven so he had plenty of experience on both small and large accelerators before he came here to take over the Directorship in 1970. Although he took over the Laboratory as a going concern he was soon faced with a variety of problems concerned, for example, with the siting of the new National Nuclear Structure Facility (NSF) and the Synchrotron Radiation Source (SRS). More recently of course, he's been much more involved with the negotiations concerned with the closure of NINA. All this has resulted in a good deal of re-organisation and a change in the research objectives of the Laboratory. But I think we can be assured that under his leadership the Laboratory has a very bright future, and he's now going to tell us something about that.

THE FUTURE

Professor A. Ashmore
Director, Daresbury Laboratory

Well, as is appropriate at a funeral, I'm going to talk about the heavenly future but I'm reminded first of all in hearing about the past again that Glasgow University was very much involved in the early days before NINA was approved and indeed there was some desire to site the new facility in Scotland. This reminds me that I was recently at a dinner at Imperial College sitting next to Abdus Salam who was asking me about how things were going at Daresbury, confessing that he'd never been there, and saying "Where is it?". So I said it was near Warrington - that didn't help! When I explained that it was between Liverpool and Manchester he said "But that's not in Scotland". Apparently he's only recently become aware of the fact that Daresbury is not in Scotland.

While Harold Rothwell was Secretary of Daresbury Laboratory he invented a new version of the game of snakes and ladders as applied to the Science Research Council. You can imagine the sort of thing, new chairman of SRC appointed go back to square one. I think I can make use of this happy notion in talking a little bit about the development of the new facilities which are planned to provide our heavenly future. In reviewing the 'Dairy of Events' we can think of them to some extent as snakes or ladders. I go right back in time to a weekend meeting at Cosoner's House in July 1969 when Denys Wilkinson, who was then Chairman of the Nuclear Physics Board, had arranged a contest between the proposers of the Nuclear Structure Facility and the High Field Bubble Chamber. The interesting thing is that I was the leader of the team on the High Field Bubble Chamber since I was then Chairman of the Film Analysis Grants Committee. That meeting was something of a rout for the NSF team but the consequence appears to have been that the HFBC wasn't built and the NSF was so you can dwell on the usefulness of such meetings. Whether it was a snake or a ladder as far as the NSF is concerned is open to doubt. In January 1970, by which time Alec Merrison had left Daresbury and Bob Voss was Acting Director, following a survey carried out by Bill Burcham, who is here today, and John Gunn, who was unfortunately unable to come, there was a recommendation from the Board to site the NSF at Daresbury. This came as a surprise to us - not to say a shock, and from that time onwards we had to take on board wrestling with electrostatic accelerators and the nuclear structure community. In May 1970 we had Nuclear Physics Board approval for the NSF Design Study, with Bob Voss as the project leader, and that was at a meeting at Sussex University which was Denys Wilkinson's farewell meeting as Chairman of the Board so I remember it quite well. It is interesting that it can be regarded as a ladder in the history of the NSF but then a typical snake occurred in June 1970 with the newly elected Conservative Government which decided on a review of all government capital expenditure and so there was a kind of moratorium in which everything was held up for some months and it was February 1971 before we got authorisation to proceed with the Design Study.

Although it is illustrated on the display outside, the story we have heard today has not said much about synchrotron radiation. In fact it has been an activity since fairly early in the history of NINA and there is a copy of a letter from Ian Munro to Alec Merrison written in January 1966, but wrongly dated in January 1965, which is the start of the synchrotron radiation activities at Daresbury. Preliminary measurements of the synchrotron radiation were already being made in 1967. So that has a long history, and the output of work with synchrotron radiation from NINA has been an important part of the success story. When the McWhirter Panel was set up in October 1972 the Science Board was well aware that the Council was likely to decide on the closure of NINA and therefore they had to consider what, if any, provision should be made for research with synchrotron radiation after NINA was closed. The meeting of Council in November 1972 was a traumatic one which I remember very well. I was invited to attend and it was at that meeting that the Council decided on the closure of NINA which is to take place next year. At the same time they approved the construction of the NSF so you could say that the snake was followed by a ladder but they injected another snake by saying they wished to defer the decision of the siting because it might in fact be better to site it elsewhere and close the Laboratory. So that looked like quite a venomous snake. However the following January they confirmed the siting - a ladder - but there was a sting in the tail in that it was subject to planning permission being obtained. Now that wasn't all that easy in the event because the application for planning permission had been put in the press during the autumn of 1972 and the local opposition was beginning to mount and I remember, as no doubt will Bob Voss and Bill Jones, going to meetings in Daresbury and Moore to discuss this with the parishioners. It was an interesting experience - a salutory one indeed. We were fortunate however in having the support of the Cheshire County Council with whom we have had over the years very friendly relationships, as other speakers have mentioned, particularly under the influence of Alderman Dewes who was then Chairman of the Council. The Planning Sub-committee of the County Council held a meeting at the Laboratory in February 1973 which resulted in them giving conditional planning permission. That is to say as far as they were concerned permission could be given but they recognised that because of the local objections this should be made subject to the concurrence of the Department of the Environment. The Department was under some pressure from the local MP Mark Carlisle who had visited the Laboratory and explained his position to us, to hold a public enquiry and this they decided to do. However, there is another event which was connected with the decision of the Council in November 1972. In March 1973 Brian Flowers, who was then Chairman of the SRC gave a talk in Newcastle about the problems of planning the expenditure on science in which he referred to the decision to close NINA as part of the price of providing new facilities, in particular the SPS at CERN. As might be expected the closing of the accelerator was confused with the closure of the Laboratory by the Press including the Times Science Correspondent, who even got an article on the subject put on the front page of the Times, which I was horrified to see on returning to London from a meeting in CERN. It was all a mistake as we know, and was subsequently denied in various places. The Laboratory indeed is still alive and kicking. The public enquiry was held in July 1973 and that was an interesting experience - I have never been in the dock before but I can't speak for Bob Voss and Mike Moore. It was certainly a slightly unpleasant experience being grilled by the opposition, but it may come in useful in the future I suppose. We were fortunate incidently to have George Bishop as the technical adviser to the Inspector, a suitably independent person who was nonetheless somewhat sympathetic to our problems and he played his part very well. Before planning permission was granted the McWhirter Panel reported recommending the construction of the SRS and so we have the various ladders and there is a snake in the middle of it namely the appointment of Sam Edwards as Chairman of the Science Research Council! I must say, however, that in fact the SRS project had had tremendous support from Sam and the state in which it is today owes a very great deal to him. Planning permission for the NSF came as a Christmas present from the Manchester Office of the Department of the Environment a day or two after we had heard from Jim Hosie that we would not get planning permission for several months. So that gave us particular pleasure. It is one of those things that go on between SRC laboratories and London Office which we all enjoy. Early in 1974 we had approval for the SRS Design Study with Jerry Thompson as project leader, and things moved quickly after that. There was an open discussion at Reading University in April 1974 to give an opportunity to the community to explain their views and we had an interesting exposition from the head of the Clarendon Laboratory (Professor Bleaney) as to why it should be sited at Oxford. However in the event the Council approved construction at Daresbury and we had authorisation on this occasion in a normal space of time. So both projects are now underway.

For the remainder of this talk I ought just to tell you a little bit about what is happening on the new facilities. Of course the fact that something is happening on the Nuclear Structure Facility will be apparent to you all in the form of the tower up towards the top end of the site (fig.1). But all of you may not know what is going into that tower, namely a 30 MV tandem Van de Graaff accelerator as shown in fig.2. The framework of the injector room on top of the tower is under construction at the present time. From this room the various ions will be injected into the tandem. The negative ions injected will be accelerated to the central terminal at high positive potential, stripped so that they acquire a large number of positive charges and then accelerated in the lower half. Since they are then multiply charged you can have ions coming out with energies of several hundred million electron volts. A wide variety of

Fig.1 General view of the NSF Tower and Buildings.

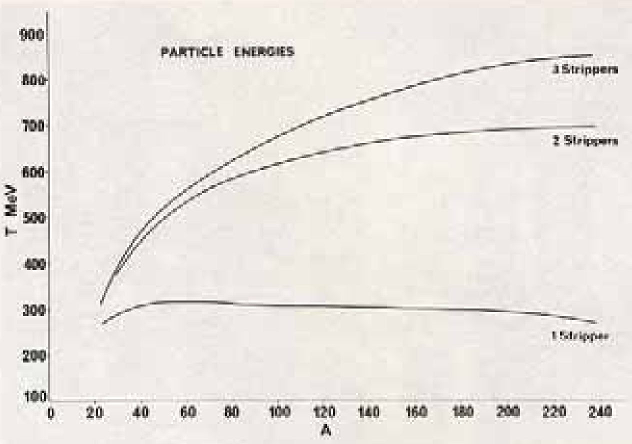


Fig.3 Energies of ions from NSF as functions of mass number.

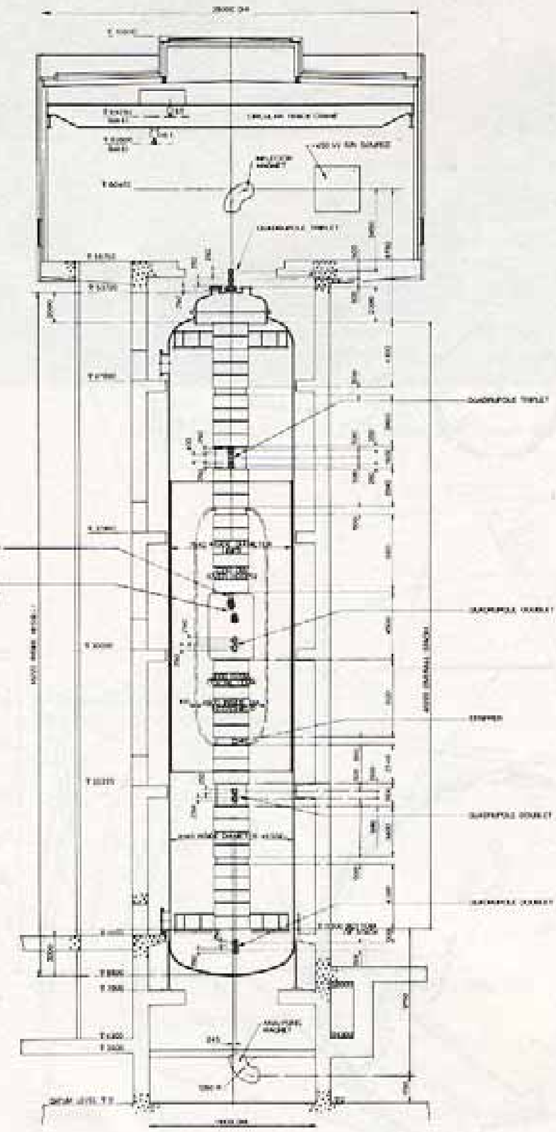


Fig.2 General arrangement of the 30 MV tandem Van de Graaff in the tower.

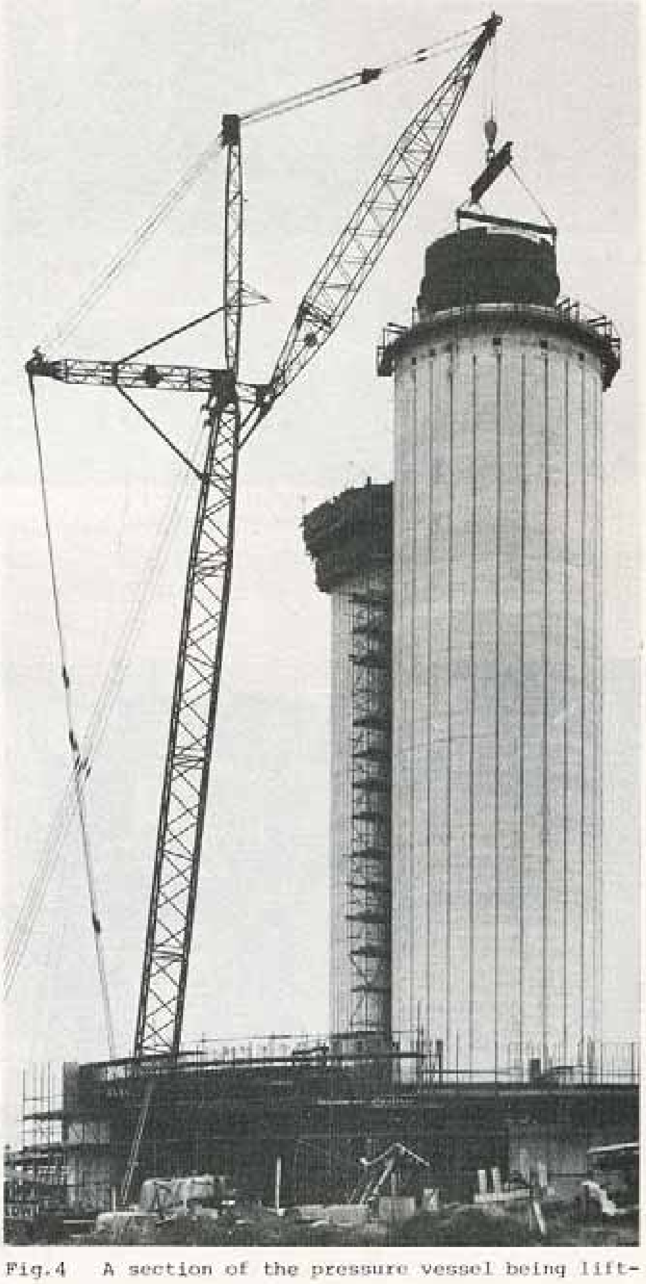


Fig.4 A section of the pressure vessel being lifted into the tower.

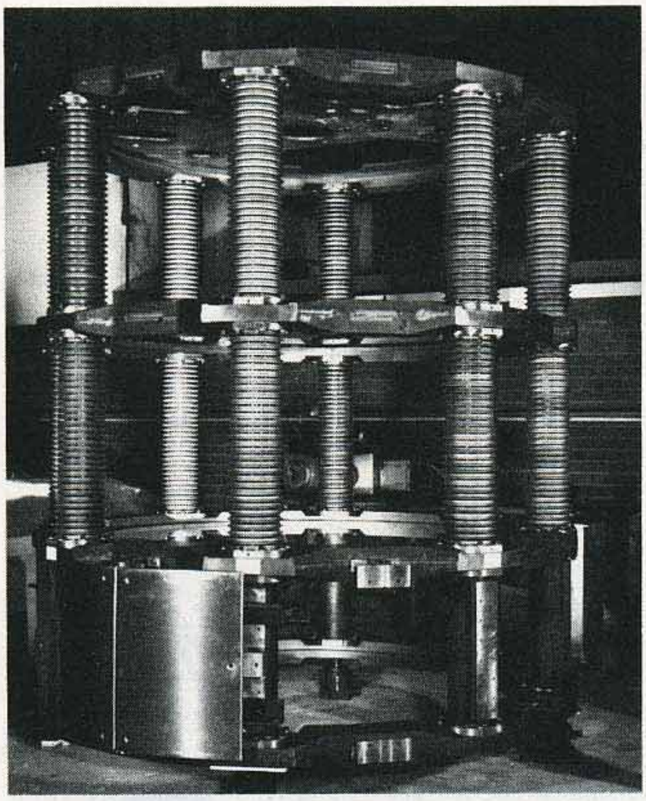


Fig.5 Unit of two live and one dead section of NSF.

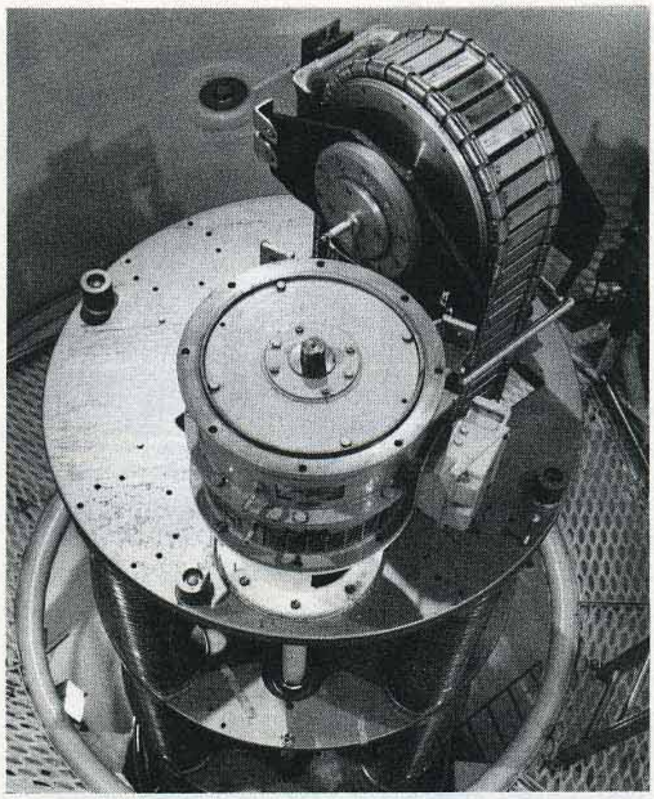


Fig.6 Laddertron charging system.

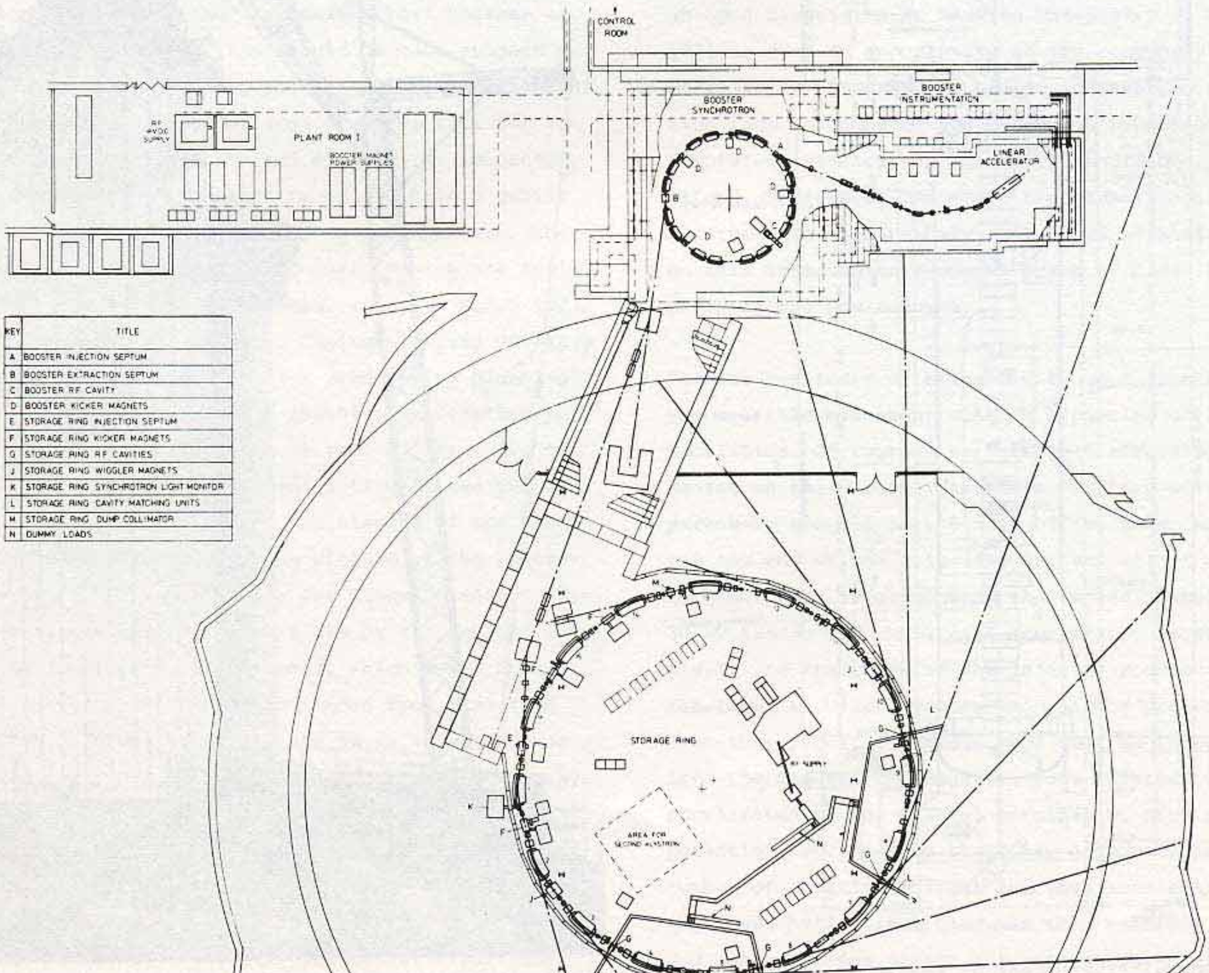


Fig.7 Layout of the Synchrotron Radiation Source (SRS).

Fig.8 Foundations for the SRS booster magnets.

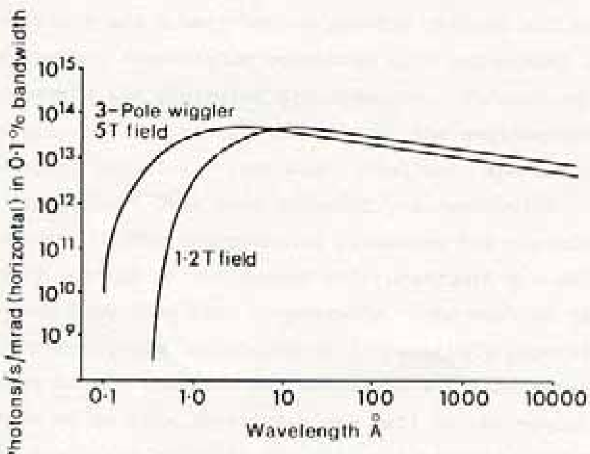


Fig.9 Spectral curves from 5 T wiggler and normal bending magnet for 2 GeV at 1 A beam.

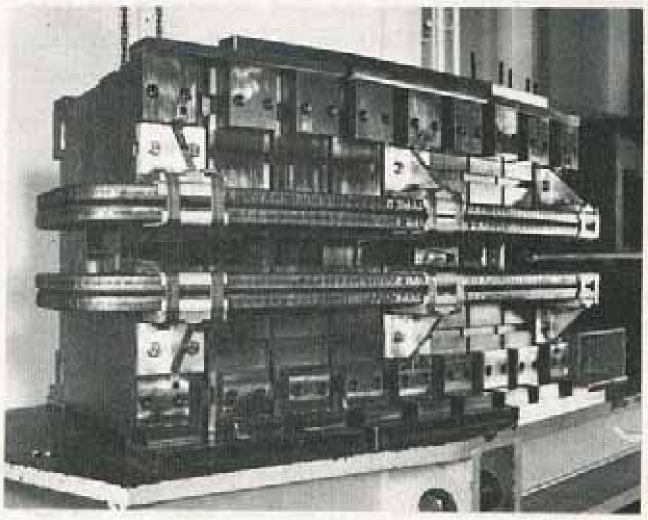


Fig.10. Prototype booster magnet assembly.

ions covering the whole range of elements can be accelerated for research in nuclear physics and to some extent other areas of science, for example atomic physics and radiochemistry. The kind of energies obtainable are shown in fig.3 as a function of the mass number of the ion which is being accelerated. What is shown in fig.2 is the guts of what will be inside the tower. What is inside at the moment is the pressure vessel. Sulphur hexafluoride gas at high pressure will be used to insulate the accelerator column, which is at high potential, from its surroundings. Figure 4 shows a section of the pressure vessel being lifted into the tower at the beginning of this year (1976). The vessel was actually lifted in five sections using a mobile crane nearly 300 ft. high and the sections were welded together *in situ*. A unit of two live sections and one dead section of the stack is shown in fig.5. There will be 36 live sections altogether in the final machine. The insulating legs, eight per section, consisting of glass insulators and stainless steel electrodes are being manufactured at Daresbury and the stack will be assembled here and installed in sections in the pressure vessel. Figure 6 shows the new charging system which has been developed here known as the Laddertron with metal rungs connected at each end by nylon insulators, this is installed on the pilot machine which has been built here for testing components. A laddertron over 20 m long is needed for the charging system in the tandem but the system has worked very well in the pilot machine and we are in a position to proceed with the manufacture of the laddertron for the main machine. Well that gives you a glimpse of the kind of work in the Laboratory on the construction of the Nuclear Structure Facility by a team made up mainly from people who were already in the Laboratory but working on projects connected with high energy physics, thus giving ample proof of the fact that scientists and engineers can be effectively redeployed.

Going on then to the SRS, the new storage ring to be used as a dedicated source of synchrotron radiation, it is not obvious where it is and it is at a much earlier stage of construction when very few of the components are in the Laboratory. The proposed layout is shown in fig.7 with the 2 GeV electron storage ring in the Inner Hall of NINA where at the moment there are power supplies and other equipment for NINA. It is designed for a circulating current up to 1 A. For this quite a high injection energy is needed. 600 MeV has been chosen and a synchrotron of that energy has been designed for the purpose. So we are building two machines not one and the booster synchrotron will be in the Experimental Hall along with the linear accelerator which feeds it. None of this exists at the moment apart from the room for the linear accelerator, which is being arranged along with the room for associated instrumentation, and the mountings for the booster magnets which are being laid as shown in fig.8. As can be seen from fig.7 a dozen beam ports can be accommodated around the storage ring. Figure 9 shows what spews out of the beam ports, namely radiation in the vacuum ultra-violet and x-ray regions of the spectrum. There is interest from the users for going to shorter wavelengths and for this purpose the design allows for special sections of superconducting magnets with a higher field than the normal bending magnets which will enable us to get to wavelengths about four times lower. A prototype bending magnet for the booster synchrotron is shown in fig.10. This magnet is under test here at the present time.

We are pressing on as fast as we can with these new facilities, in spite of the difficulties, particularly limitations of money and manpower that face us in the years immediately ahead. Our aim is to complete them within about three years and I hope that their exploitation will prove as successful as that of NINA. I have a feeling that it will be not for me in the main to direct that exploitation.

CONCLUSION

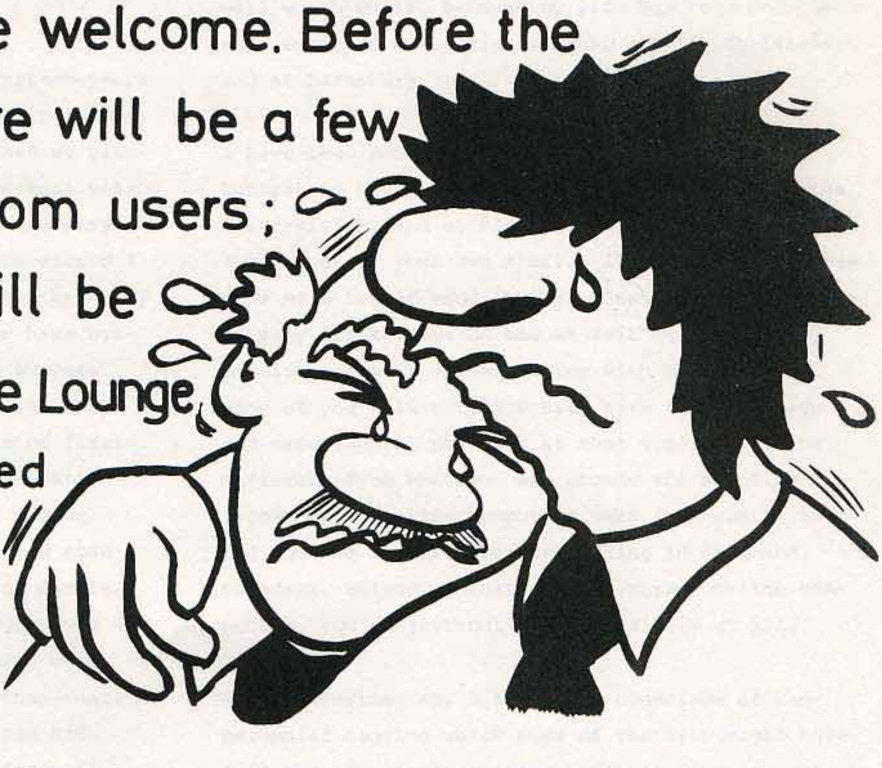
Professor J.R. Holt

On your behalf I should like to thank the four speakers this afternoon not only for their most interesting talks, but also for the contributions each of them has made in various ways to the success of the laboratory. In fact, this is a good opportunity for me to express thanks on behalf of all the high energy physicists who have been priviledged to work here during the past 10 years. Some of them are present today, but others have already moved away and are actively engaged in planning experiments for other accelerators, particularly for the new Super Proton Synchrotron at CERN. Erwin Gabathuler is now in residence in Geneva as leader of a European collaboration to study muon physics, and has sent the following telegram: 'Please convey to all those who contributed my sincere thanks for their excellent support, including all the machine and experimental hall support staff who kept NINA going at all times of the day and night. The past ten years have produced excellent physics which we shall endeavour to match for the next ten years at CERN through the photon and muon programmes at the SPS!'

For my part, and speaking for the university users of the laboratory, I should like to say how much we appreciate the efforts that have been made on our behalf. The administration under Harold Rothwell was always most helpful and unobtrusive and we invariably had a very smooth passage through all the necessary formalities concerned with university agreements and financial arrangements. Turning to other sections of the laboratory, the engineering support facilities have been excellent, and all our demands have been most cheerfully accommodated. The success of the experimental programme has depended very heavily on the design and construction work for which they have been responsible. The work of the computer group has played an increasingly important part in the experimental programme with the provision of on-line facilities, as well as the operation of the large computer for analysis of data, and we have had excellent support and co-operation in that field also. The machine operations group has been responsible for the beams which have formed the basis of all our work, and the people in that section have worked tirelessly at all hours over the past ten years, not only to keep the machine running, but also to steadily improve it so that at the present time NINA is working at the peak of its efficiency. We are heavily indebted to them. Finally, let us express our sincere thanks to the Science Research Council for making it all financially possible. May the Laboratory, and those associated with the new phase of its development, have a bright and prosperous future.

GOODBYE NINA

NINA will be closing down on the morning of 1st. April 1977 and a 'wake' will be held in the Main Workshops/Experimental Hall at 11.15 a.m. All those who wish to pay their respects to NINA on her passing are welcome. Before the coupe de grace, there will be a few words of thanks from users; these speeches will be relayed to the Coffee Lounge.



A drink will be provided in the workshop........ to make the end more sweet.

NINA CLOSEDOWN CEREMONY

On the 1st April 1977 NINA was finally closed down. Crew, Staff and Users gathered in the workshops for an informal ceremony to mark this very special occasion. Dr. R.G.P. Voss introduced and co-ordinated the proceedings. Tributes were paid by representatives of both university and resident users as well as Daresbury Staff.

Dr. R.G.P. VOSS (Daresbury Laboratory)

One has so little experience in shutting down accelerators that I find myself in difficulty in knowing what to say on such an occasion. However, feeling that I had to say something, I thought about it very hard and then was saved the problem because some telegrams arrived which I think I should read to you. One is to Godfrey Saxon from Peter Norton at the Rutherford Laboratory saying "At the closure of NINA please pass on the sincere thanks of the PEP Group to all those people associated with NINA who contributed so much to the success of our experimental programme". Then there is another one, which is a little suspicious considering today's date! This is from SRC London Office to Professor Ashmore and it reads: "Please state the consequences to your projects if NINA (a) continues to run until 1978, (b) indefinitely or (c) is transferred lock, stock and barrel to Rutherford or CERN. A reply is necessary before 12 noon today, Friday, 1st April, or it may become a little difficult to arrange".

It's quite extraordinary to look back fourteen years and to think that this site was a field of potatoes. Of course, as soon as the farmers knew that we planned to build something here it became the most valuable field of potatoes in the country - and very nearly scuppered the project. When I look around I see so many faces who were here right from the early days but all of you in one way or another have contributed to what really has been quite a success story. In fourteen brief years we have seen the construction of the machine and ten years of first rate operation. Some of you will have been at another function this morning when we were saying "Goodbye" to one of the engineers who played such a role in the experimental programme, George Corrie. I had a nightmare that I would solemnly give you a farewell card signed by a lot of people and give George NINA but so far it's gone better than that. We have a galaxy of speakers to address you and afterwards a glass of beer to wish NINA farewell, and so it's with great pleasure that I call on the first of our speakers, Bill Galbraith.

Professor W. GALBRAITH (University of Sheffield)

NINA was never famous for its public address system and I hope everyone can hear what I'm saying. An occasion like this, as my wife said, is one of sadness tinged with pleasure. The sadness, of course, is saying "Goodbye" to an old friend of the last ten years, and the pleasure is that I don't have to drive across the Pennines in the middle of the night any more when it's snowing! I've worked out that in the last ten years I have spent something like two months of my life just in going to and fro, and covered sixty thousand miles in that time. So it has not been simply a question of slipping out of your room to do your research. However, I have felt that the whole purpose of NINA, which brought me initially to Sheffield, so far as I am concerned, has been well worth while, because my life has been considerably enriched by my experiences, both in Sheffield, and at Daresbury.

I have been particularly pleased at the way this Laboratory has evolved in its co-operation with the universities, and at times I have almost acted as if I were one of your own staff. I'm not sure this has done me a lot of good, but at least we have exchanged very frank views on how we felt the Laboratory should be run in collaboration with universities. Many of you will not have been here ten years ago and experimental physics, at that time, was quite different from what you see around the counting rooms now. On-line computers were just coming in and you had to take down everything in longhand. Nowadays, unless you have two or three on-line computers, you're just not in the business at all.

As an experimenter, I have been conscious of the perennial nagging which some of the crew might have felt the experimenters have been engaging in, but let me assure them that this nagging was absolutely essential in order to take good data. So if you, the crew, have felt from time to time like murdering the experimenters, you can rest assured that the feeling was mutual. When NINA was groggy, one had to rely very much on the so-called expert, that mysterious 'Dr. Who?', who would be coming along to twiddle NINA's prolific knobs and get her on her feet again. The dedication of people getting out of baths in the middle of the night and coming in to set the machine going again is something which isn't usually acknowledged, other than "we are grateful to the NINA staff for the work which they have done to provide a beam". However, I do feel that the Computer Group has missed a God-sent opportunity, because the BBC show you how to run a machine like this now, with their 'Cybermen' or 'Bionic Women', to come and slap NINA into sensibility again.

I recall with pleasure a very special person around here, and this really stems back to Gerry Rainbird and the Hydrogen Target Group. But the special person I want to mention particularly is the Duty Tech., that Jack-of-all-trades, who rouses himself in the middle of the night from perhaps a few dozey minutes, to come and deal with some emergency on a hydrogen target. I don't know if you realise how dangerous it is to play with liquid hydrogen, but I think this Laboratory has had a safety record in this respect which is without doubt a very good one, and we have to be very grateful to Gerry and the hydrogen technicians, who ran the early hydrogen targets. Thank goodness, nowadays, we don't use these targets quite so much, and I believe you are being 'let off the hook' for the future here.

Then, there was the question, whenever we were setting up, 'where has the beam gone to?' Here we have to acknowledge the sterling work - in meaning they were actually paid to do this sort of thing - of Harry Stott and Derek Gregson. Somehow or other, NINA seemed to move around mysteriously when we were not on the machine, and it was always quite a struggle for those people to find out where the photon beam had gone; we are very grateful for all their work on survey.

However, you may well wonder what does it all add up to? I was taken aback about a week ago last Sunday when I was talking to one of the experts over lunch. There were just two of us in the restaurant at the time, and he said "Has the work that's come out of NINA been original, or are you just repeating someone else's work?" When I had recovered from the initial shock of that question, two things struck me, and I pass one on for the future here. Alick, I hope that you will ask all visiting university teams to report informally in short talks, perhaps every six months or so, on the state of work in hand, and I hope that such a question will not need to be asked again. But if the expert is in the audience, (I won't mention him by name) let me reassure him, and everyone else, that high energy physicists are not people of that breed, and we don't like doing work that other people have done. Originality has been the keynote of the research work around NINA, whether it has been in connection with photon physics or electron physics, and everyone who has been associated with the on-going work of this Laboratory can be satisfied that the contribution NINA has made to such physics has been a considerable one over its lifetime.

The overt value of the work is not realised unless you publish papers. Now some of you have seen young students about who are working for Ph.D. Theses, and one of the difficulties left for us 'oldies' to cope with, occurs when they go off to new pastures, leaving analysis incomplete. However, when we finally manage to publish the work, it appears in a reputable journal and you are not aware, when you see journals on the shelves, getting fingered and perhaps dusty as well in years to come, what an awful lot of work has gone into those papers. To demonstrate to you what I mean about this, and here I didn't want to embarrass any other groups, but I can assure you that they can do this type of exercise for themselves and be equally surprised how much paperwork is involved in just announcing to the world what physics has been done here. May I ask for two young ladies to come forward? It is the way I would like to say "Thank you" on behalf of Glasgow-Liverpool-Sheffield, to everyone around NINA, particularly the NINA crew for providing us with the wherewithal to do the physics. So that you know what these strung out bits of paper are, I didn't bring the publications themselves, but over the ten years, I have picked out those which happened to have my name on them, so I can honestly say that I have read every single word, probably twenty times over in these papers. These are the front cover pages only and go to show you what we do, in fact, achieve a long string of publications from working around NINA.

Finally, I had hoped that this little farewell would have taken place in the control room, so that we could have festooned this string of flags around the ship's deck. I always felt that working at that control desk was rather like being in the wheel house, and if you happen to be working there in the middle of the night, you can glance forward to see what distant lands come into view through the forward hatch. It recalled to me that well known song, containing about 365 verses but, as there are ladies present, I propose only to mention the first one:-

"'Twas on the good ship NINA,
You really should have seen her,
The captain's crew
A motley few,
For pornography keener ....'"

Dr. R.G.P. VOSS
Thank you very much, Bill. Congratulations on the conjuring trick. It was first rate. And now I call on Mike Ibbotson.

Dr. M. IBBOTSON (University of Manchester)
Well, I'm very pleased to be able to use this period I've been allotted to thank the many people who have helped Mancaster produce an awful lot of good data. The Mancaster Group has been running at NINA for eight years now and we have produced something equivalent to the number of papers that Bill Galbraith showed. I think we are now going to reach our twentieth major publication on the strength of the analysis of our new data. This work would not have been at all possible without the continued support of the Director, or the management services and the general welcome the Laboratory gave to university visitors.

This volume of work could not have been produced without the excellent facilities that NINA has given us. In our case we use the small size of the electron to probe the complicated structure of the proton, and we need very intense beams and very long duty cycles. In both those counts NINA has come up with facilities second to none in the world so we're very grateful for that.

Now many people have helped us in the past eight years and I've only time to mention a few. We've always been extremely impressed here with the mechanical and electrical services. Again, I think the services provided by NINA are without parallel in the rest of the high energy physics community. Our thanks from the Mancaster group go particularly to our supporting engineers. To Don Taylor in the early days, Hamish McFarlane, Jim O'Gara, and all these names, I'm sure there are many others. They gave tremendous support to the group's work in difficult times. We've also been most grateful to the dedication of the machine physicists. These are a band of men - we see different ones, as they change every couple of years - but the one common feature they have is that they are all prepared to turn out in the middle of the night when there is a problem. Here I mention people like Godfrey Saxon, Tom Aitken, Vic Suller, Mike Poole, Alan Jackson, Steve Armitage - a long succession of machine physicists who have given us very good service. We, as Bill Galbraith has said, are particularly grateful to the work put in by the hydrogen target people and it's more than competence in the case of Gerry Rainbird. He's really had the interests of our group at heart and again he's been prepared to turn out every other night at times when the targets in the early days were giving trouble. In more recent years the targets have been so reliable that there has been no case for him to come out; we have been very impressed with Gerry Rainbird's competence in producing a safe reliable target. We are also very impressed with the loan pool services and in particular I'd like to mention the work of Ken Wray and Charlie Darlington who got us out of a difficult period with our large multi-wire proportional chambers. Their work was superb and made possible a series of experiments that looked at one stage to be floundering. Of course, no respectable high energy physics experiment these days can survive without a big back-up computing service and NINA had a very good one there too. Our thanks go to Brian Davies and his team for the excellent off-line and on-line facilities provided in the Laboratory. Finally, of course, the people who really get the rough end of our tongues at times are the crews and we hope we tried to be reasonable in difficult times. Our thanks go to them for maintaining patience when we kept bellowing for more beam or longer duty cycle every five minutes. It is essential I assure you and our data is all the better for that effort. We thank you for all you have done to keep NINA running smoothly. All the various back-up people, the duty techs., the technician services, the mechanical workshop, all these people have contributed tremendously. One person I often felt sorry for was Brian Taylor who often had to report at Scheduling Committee meetings and define how much good beam time the group had had. Well it's very difficult for someone to report how much good beam time you've had when the group has the decision on whether the beam is good or not and there was always a very big argument about this. So I'd like to finish today, not to be outdone by Bill Galbraith and his two young ladies, by presenting Brian Taylor with a log of Manchester's last run, showing the amount of beam we've had over the last month.

Dr. R.G.P. VOSS

Thank you Mike, thank you Bill, for your tributes from the university users. I think all of us have found the relationship we have established very satisfying and I trust that we will be equally successful with our new users who will be moving in before too long for the two new facilities. Now, to speak on behalf of the resident users, here is John Thompson.

Dr. J.C. THOMPSON (Daresbury Laboratory)

I'm rather overcome by the size of the audience and there is very little I can add to follow the excellent speeches of the last two people. The first resident users were the WAPP group, of which I was not a member, so I do feel more qualified to speak on behalf of LAMP - actually LAMP 1/2, LAMP 1 and LAMP 2. Some of the people you probably only faintly remember but they were not always renowned for their patience with the MCR. The crew had to learn to satisfy the demands of one of our American members who wore a white stetson hat, as well as our original group leader shouting down the 'stentofon' in staccato voice for a better beam.

So now is my opportunity to express our appreciation to three groups of people, to all of the crews, to the accelerator physicists who worked so hard to produce the beam quality we desired and to the mechanical services who often worked with dedication and speed to repair our faults. It is surprising when you look back over the years how the bad times are forgotten, but there is one incident I can remember during the start-up of LAMP 1 in September 1973. In those days there were more users and machine time was very precious indeed. Just as we were about to take data, a serious intermittent fault occurred in the magnet a.c. power supply network which nobody knew how to solve. The grid-controlled mercury arc pulse valve by spurious triggering was depositing $\sim$ 12 MJ into a 1 Ω resistor. Fortunately, one of our 'favourite' crew leaders, Brian Maynard, who happened to be in the vicinity, suffered no personal injury from the flying shattered remnants of this resistor. It was visualised that NINA would be off for months since the only remaining spare valve did not solve the fault either. This occasion typified the concern and dedication of the people who worked on NINA. On a Saturday morning during this period, I can remember discussion going on between the Scheduling Committee Chairman, Alan Kemp, Jim Prior, whom I'm delighted to see here today, Niel Marks, Godfrey Saxon, the crew and others. Of course, NINA did not yield her darkest secrets easily and I think its true to say that we do not know what went wrong to this day.

That occasion exemplified the atmosphere we've all enjoyed even when there were problems. In more recent times, it was expected that this dedicated purposeful atmosphere would suffer as the number of users declined and the final shutdown approached. Nevertheless, it is fair to say that the morale has been maintained by our support staff as a whole right to the very end. Indeed, NINA's operating performance and the speed with which faults have been rectified has been exceptional. I can point to an occasion during our final run when a long H film vacuum window imploded in our photon tagging system. Having survived for seven years, it decided to let us down two weeks before NINA closed! The repair involved the disconnection of over 500 cables and uncoupling of a hodoscope of 200 scintillation counters from a magnet - a system which had taken several months to construct. In fact, it was back together again in 12 hours thanks to the support of our mechanical services technicians.

It has been a superb seven years and we hope, although we cannot show you results from LAMP 2 now, that some exciting results will emerge from our analysis making it all worth while.

It is a sad although inevitable occasion - sad because NINA herself is still young, performing well and beautiful to look at. Thank you very much.

Dr. R.G.P. VOSS

Some years ago we were approached by some people, in the first instance from Manchester University, who wanted to use the synchrotron radiation from NINA. They assured us we would never notice their existence, they'd be no trouble at all, they would be parasites in the best possible sense of the word, and now look what's happened. I ask Ian Munro to speak.

Dr. I.H. MUNRO (Daresbury Laboratory)

Well, last but not least, comes the SRF. Today is a sad day for me, both personally and in terms of the closing of the SRF - but let me begin by reminding you that the SRF (which many of you may have never even seen) is that rather isolated garage-like building squeezed between what was to be the shielding around the NINA booster ring and the present NINA injector. The SRF has been something in the nature of a cuckoo's egg planted in the High Energy Physics nest at Daresbury some ten years ago. Now look what's happened! The SRF Laboratory is crammed with apparatus, high energy physics is being displaced and in a few years time synchrotron radiation research will become a major activity of the Laboratory! Of course, the Synchrotron Radiation Facility is not a single kind of experiment or research group, it's a community of users, who work together although with a varied range of research interests, using NINA and the facilities at Daresbury. A few years ago we were struggling for parasite running time on NINA and it is amusing and ironic that yesterday, on the last day of NINA operation, the SRF cuckoo flexed a muscle when we actually had one of the high energy physics users pleading for parasite time from the SRF. Best of all, we decided to have the machine running in the optimum mode for synchrotron radiation. There is nothing like preparing for the future!

Speaking on behalf of all the users I have to say that it has been great fun working here, because the tremendous variety of experiments and science that has been carried out has drawn together a wonderful mixture of physics and personalities. It is satisfying for me to recall that very early on we realised that synchrotron radiation research was going to become expensive, if not very expensive. To generate the necessary cash we had to produce a few preliminary results and write an impressive scientific proposal. Unfortunately, the first experiments inside the NINA ring (hidden behind a massive castle of lead blocks) gave absolutely nothing because the intensity of high energy radiation simply destroyed the light pipe - not a solitary photon trickled down into the detector. Eventually two, or was it four, points were established on a graph and with the wonderful confidence of physicists, we extrapolated a straight line to ten thousand times the measured intensities in the visible with the result that the SRC (wisely!) handed out a third of a million pounds! That may have been our greatest achievement! Well, since then, we have experienced (or suffered) exploding gas bottles, hydrofluoric acid down in the canal, lost researchers and staff in the radiation areas, burned sodium and complained bitterly about NINA being suddenly turned off only to discover that we were responsible! Nevertheless, the growth of the SRF in the last two or three years has been impressive. There have been something like thirty groups and perhaps 100 different people working at Daresbury depending not only on the machine and the high energy physics programme but, of course, also on the Laboratory for computing support, engineering support, admin., the library, etc. In fact it is cooperation with the entire Laboratory that has really made the SRF the success it is. Our most striking contribution (noted not least by the library staff) has been that the number of publications rose from one in 1972 to some 66 publications in 1976 with probably the same number to come during 1977. So in these few years the SRF has been running very effectively as a national facility, even though it was not a purpose-built source and has remained to the end dependent on the activities of the high energy physics.

It would be unreasonable for me to mention individuals, either from among the users or the Laboratory staff except to say that we have been supported from the beginning by Laboratory Management and particularly by Alick Ashmore, who has always had our interests at heart and has helped to steer our present successful course.

My closing words and parting gift to NINA is not only a list of publications, it's much thanks to the Laboratory, particularly to you Alick - and most important it is the foundations of the new, even better synchrotron radiation source arising beside NINA herself. We, with all future SRF users, look forward to a new period of support and collaboration at Daresbury in 1979. Thank you NINA.

Dr. R.G.P. VOSS

You notice that smart move, getting in now for the support that he wants later on. All this business about building NINA reminds me of a story which was told to me at the Weizmann Institute in Israel recently. A visiting North American got on a bus and sat next to a pretty Israeli girl. He wanted to impress her and he said "Have you heard of the Golden Gate Bridge?" and she said she had, he said "My father built it". A little later, as this didn't have any effect, he said "Have you heard of the Empire State Building?" she said "Yes", he said "My father built it". This went on for some time and she was getting rather exasperated so finally she turned to him and said "Have your heard of the Dead Sea?" and he said "Yes" she said "My father killed it". I now call on Godfrey to kill the proceedings.

Mr. G. SAXON (Daresbury Laboratory)

Well this really is the last one so all those thirsty people at the back have not much longer to wait. No matter how much we try to make light of this occasion it really is a momentous one when a major facility which has been serving the community for so long is finally closed. It's been a major part of the working life of many here, for upwards of 14 years for some of them. I'd like to thank the previous speakers on behalf of all Daresbury people for their appreciative remarks about the support that's been offered. Of course, we have to remember that there would have been no NINA without their thirst for scientific knowledge. None of us would have been here and this wonderful toy that the accelerator physicists have had to play with over the years would not have been built. I would join in the plea of Bill Galbraith that somebody from amongst the users would give an explanation of what the achievements have been over the years in terms that the majority of us here could understand. I think an interpreter is needed for many of us and I'm sure somebody should do that. By and large we've had, from the crew point of view, very friendly relationships with the experimenters. Its varied a little bit from the, perhaps, slightly hectoring attitude of Erwin Gabathuler to the friendly charms of a lady known as "Diamond Lil".

Speaking for myself, I've been very proud of the crew, and very proud to have led them, and when I talk about crew, of course I include the services technicians who are such an integral part of the team. I'm proud because they responded to the challenge of declining crew numbers and declining resources and determined with me to make the machine go out, if it had to go out, with flying colours. Also I would particularly like to thank Brian Taylor and Malcolm Richards for the personal support which they have given to me, which has made my own task fairly negligible really. They have done all the work that had to be done from the operations side. We should remember, too, that this has not ben achieved without the backing of a tremendous number of people here, for instance, all the services people, (vacuum, electrical, mechanical) who have had to dive in and do things on the machine either during shutdowns or during breakdown periods. The industrial staff here have been involved in modifications and maintenance, and I would also like to mention people who don't often get a mention - those who have kept that machine clean and looking good. It's always been a matter of pride to me to take somebody in and show them that machine. It has always looked good and still does.

It's been somewhat astonishing to most of us involved in the experiments, or in running the machine, how well the machine has behaved particularly in the last two cycles and indeed before that. We just switched on in January and left it to run without very much maintenance or support. I think the machine's been a little bit like Macbeth, "Nothing in her life became her like the leaving of it". The efficiency this year, that is the time during which a good beam has been available to experimenters, has been 92% and that is a record for the whole of the period of NINA's life, and I'm still astonished. One would come in in the morning and find there'd

Fig.1 Neil Kelly, crew leader, switching off NINA for the last time.

Fig.2 Bob Voss acting as M.C. for the Closing Ceremony.

Fig.3 Bill Galbraith speaking on behalf of the NINA users at the Closing Ceremony.

been no interruptions over a weekend, or perhaps just ten minutes off. The machine just got the bit between its teeth and ran and ran. In fact the only major interruption during this cycle has been that we've had such intense beams that the pole face windings on one of the magnets just curled up their toes with radiation damage and had to be replaced.

During these long nights and days when the machine had been running well and you might think the crew had nothing to do, you may wonder what they do in the long night watches. There's a photograph in the control room which shows three of them stretched out on the control desk in sleeping bags. I think that was really a put-up photograph for the occasion, but when the machine is running smoothly and experimenters are happy or else asleep, then out comes the tea and one or two books are produced. Quite learned books some of them, books about synchrotrons, for instance "Lord of the Rings". Also some of the magazines appear which, to my casual glance, remind them in the long night watches of certain salient, or should I say prominent, features of the female anatomy. We've also had a crew leader who dreamt up some quite poetic and lyrical prose, which was published in the Bulletin not long ago, in praise of NINA's undulating curves. I think there's not one of them, in the era when computer games were all the rage, that didn't know how to land a lunar module in real time given a limited amount of propellent. I'd like to echo the thanks to the accelerator physicists, particularly, in recent years, Vic Suller, Alan Jackson, Steve Armitage and John Poole, but others too, whose names I'm not mentioning, who over the years have tried to understand the ways of NINA. It took quite a long time before we really understood this machine and out of that knowledge some of the improvements came about, particularly in the setting up of beams as well as the understanding of NINA. She still carries away with her some mysteries which I think will never be solved.

Just a few statistics now. One way of looking at what we've done in ten years is that we've produced 1/30th of a microgram of electrons and accelerated them to a relativistic mass of 1/3rd of a milligram. Now it doesn't sound so good that way and if you really want the big figures, we've accelerated something like 400 million-million-million electrons and provided a good beam over some 40,000 hours.

I don't know whether you've studied the notice convening this meeting and whether those of you who are taking French lessons have noticed the deliberate mistake. It talked about NINA being put out of her suffering with a *coup de grace*, actually the 'coup' was spelled 'coupe' and the coupe de grace is I think what we're going to drink afterwards, in a very few minutes now. At the end of the notice it talked about having a little drink to make the end more sweet. What we are going to make the end more sweet with - is bitter! So we come to the bitter-sweet ending. We had a little debate about who would actually perform the switch-off. We decided it would be one of the crew and debated whether this should be decided by pulling a short straw or by using some collective method of switching off. In the end I decided that the simplest way was to choose the crew that had been appointed for today. It was only right that they should do it and they are Neil Kelly, Jeff Meehan, who struggled for a long time to achieve the highest beam and claims to have achieved 40 mA circulating in this very cycle, and the services tech., Jim Finlay. So I will ask them now to close down the machine. Its "Goodbye" to NINA, but long life to the SRS and the NSF. Will the crew please now close down the machine.

LAST ANNOUNCEMENT

N. KELLY

Attention, attention. This is a final announcement from the main control room to all at Daresbury Laboratory. NINA is about to be switched off for ever. NINA is closing down.

IMPORTANT

This is an informal document and the views expressed are those of the individual contributors.

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