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NATIONAL INSTITUTE FOR RESEARCH IN NUCLEAR SCIENCE

AND

DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH

WORKING PARTY ON HIGH ENERGY PHYSICS

# THE UK PROGRAMME IN HIGH ENERGY PHYSICS 1965 - 1975

24th May, 1965

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## CONVENTIONS

(i) All estimates correspond to 1965 prices in M£.

(ii) The contributions to CERN are assumed to be 25% of its budget through 66/7, and thereafter 22½% in anticipation of the review to be made in 1966

(iii) The UK contribution to the costs of ISR and the 300 GeV machine is assumed to be 25%.

(iv) £1 = 12 Swiss Francs.

(v) No account has been taken of UGC contributions to the subject as we have found no sensible way of doing so.

(vi) The financial years in the UK and at CERN are displaced by 3 months from one another. To bring the expenditures into accord we assume that if the CERN expenditure in its year, is L, then for our year x/x + 1 the appropriate figure is $\frac{3}{4}$ Lx + $\frac{1}{4}$ Lx +1.

## ABBREVIATIONS

1 MeV = $10^6$ electron-volts
1 GeV = $10^9$ "
1 TeV = $10^{12}$ "

ISR: Intersecting Storage Rings; the principle may be applied to both electrons and protons.

DSIR: Department of Scientific and Industrial Research.

NIRNS: National Institute for Research in Nuclear Science.

NIMROD: The 8 GeV proton accelerator at the Rutherford Laboratory.

NINA: The 4 GeV electron accelerator at the Daresbury Laboratory.

CERN: The European Centre for Nuclear Research at Geneva.

CERN PS: The CERN proton synchrotron of 28 GeV at Geneva.

Brookhaven AGS: The "alternating gradient synchrotron" of 32 GeV at Brookhaven, essentially similar to CERN PS.

PLA: Proton linear accelerator; the Rutherford Laboratory installation gives protons of 50 MeV. Protons for the great accelerators are first accelerated in a PLA.

300 GeV accelerator: A proton synchrotron, identical in principle with the CERN PS and approximately ten times the size.

SLAC: The 20 GeV electron linear accelerator at Stanford, USA.

NSP: Nuclear Structure Projects - proposed new support for nuclear structure research in the UK.

## SOURCES AND REFERENCES

The principal sources giving forecasts of expenditure on various elements in the proposed high-energy programmes discussed in this report are as follows:

(1) For the CERN basic programme, ISR and the 300 GeV machine, see the paper of the CERN Scientific Policy Committee: CERN/SPC/196, 19th February, 1965.

(2) For the DSIR programme, see the paper of its working party on high energy nuclear physics, WP/HEP/6, 25th February, 1965.

(3) For the NIRNS forecasts - see the paper of its Executive Committee, NX/65/22, 24th February, 1965.

(4) For forecasts on nuclear structure research see the report of the Working Party on Nuclear Structure Research.

(5) For US forecasts and planning see: "Report of the Panel on High Energy Accelerator Physics of the General Advisory Committee to the US Commission" dated 10th May, 1963. This is referred to as the Ramsey Report, and

(6) "Policy for National Action in the Field of High Energy Physics" dated 24th January, 1965, which we shall refer to as the 1965 US White Paper.

(7) For details of the design features of the CERN, ISR and the 300 GeV machine, and a review of existing and projected facilities in high energy physics on a world scale, see Proc. Roy. Soc. A Vol. 278 No. 1374, pp. 288 - 464, 7th April, 1964, a Symposium entitled "A discussion on recent European contributions to the development of the physics of elementary particles".

## CONTENTS

## SUMMARY AND CONCLUSIONS

In this report we consider the UK's activities in high-energy physics over the next ten years.

The report is divided into three parts:

Part I is a general re-examination of the case for the promotion of high-energy physics and its continuation through a 300 GeV project. This question was discussed in detail by the DSIR/NIRNS Joint Consultative Panel for Nuclear Research in its report, "The Future High-Energy Physics Programme of Europe" dated 25th April, 1963 (The Flowers Report). The experience of the past two years has, in all essentials, confirmed and further emphasised the conclusions and predictions of the Flowers Report on the need for a continuing high-energy programme and a multi-100 GeV machine, and we include that report as an appendix to Part I of this present report.

From the considerations reviewed in Part I we conclude that, in the next ten years, the development of high energy physics in the UK ought to be continued on the ground of its contribution to the education and training of increasing numbers of scientific personnel called for by national plans for the expansion of the undergraduate and post-graduate student body; and because of the great scientific promise and high intellectual content of the subject.

For this purpose, access to machines giving particles of the highest energy, with the most advanced facilities, is essential, and it can also satisfy the need to accommodate more researchers. A crucial element in the expansion could be provided by a substantial share for the UK in a 300 GeV machine.

In Part II, we discuss the siting of a new high-energy accelerator and the suggestion that access to a US machine might provide a solution to our requirements. We conclude that only the European 300 GeV accelerator can satisfy our basic needs in the long term and we strongly support it. It should be proceeded with as a matter of urgency, and there would be great advantages if it could be sited in the UK. We are firmly of the opinion that a share in a similar machine located in the USA would not be acceptable on many grounds; in particular, it would not meet our basic needs and would be damaging to science in the UK and in Europe.

In Part III we consider in detail the complete UK high-energy programme. We conclude that these plans call for a rate of financial expansion of 19.3% per annum over the next five years. We consider that these plans are fully justified by the considerations brought under review in Part I.

At lower growth rates we should still recommend giving the 300 GeV machine highest priority except for those activities that must be a prior commitment, namely the CERN basic programme and direct support for work in universities. A growth rate of 15% would enable us to mount a significant high-energy programme, but would involve serious sacrifice in the domestic programme especially in the exploitation of the NIRNS Laboratories. Such a growth-rate would be acceptable if there were hope of a later recouping.

A growth rate of 10% would make impossible a balanced high-energy programme and the subject would suffer an inevitable decline in the UK. A growth rate of 5% would lead ultimately to the extinction of high-energy physics in the UK and we would recommend an early shutting down of the Rutherford Laboratory.

# PART I

## THE CONTINUING HIGH-ENERGY PROGRAMME AND THE 300 GeV PROJECT

### SUMMARY

1. High-energy physics is an exciting field of the highest scientific promise and intellectual importance in which the UK should continue to be actively involved if it is to make its proper contribution to world progress.

2. High-energy physics is a valuable training-ground for the modern technologies and the present proposals would make high-energy physics a considerable net producer of research scientists, after its own growing needs have been met.

3. Inadequate support for high-energy physics would be damaging to general undergraduate teaching in physics and for the balanced development of science in the UK.

4. The natural development of high-energy physics in the UK, together with the expansion of undergraduate numbers, will require extensive new facilities by the early 70s.

5. A crucial element in the new facilities is the 300 GeV project, and it should be proceeded with as a matter of urgency.

## I A INTRODUCTION

The aim of Part I of this report is to examine the case for the development of the UK high-energy physics programme in the next decade and the facilities necessary for its support. The case has already been considered in the Flowers Report which we append as an appendix to this part of our report. We have briefly re-examined the questions in the light of the experience of the past two years and endorse the previous conclusions. In I B the present promise and significance of the subject for the development of science are assessed. This is followed in I C by consideration of the proper energy for a new machine to support the subject in Europe in the 1970s and after; and the timing of its construction. In I D a review is made of the extended facilities necessary to satisfy the increasing demands for research places in high-energy physics arising from the planned growth in undergraduate student numbers, and to maintain a balance between high-energy physics and other sciences. The value of high-energy physics as a training for membership of the general scientific community is also considered and the contribution which it can make to the flow of trained scientists into industry and scientific occupations other than high-energy physics.

## I B THE SCIENTIFIC BACKGROUND

The academic case for promoting high-energy physics is even stronger now than it was in 1963 when the Flowers Committee reported. This is largely due to the recent great increase in our understanding provided by the unitary symmetry schemes. They promise a classification of strongly interacting particles, analogous to the Periodic Table, which may even include the weak and electromagnetic interactions in a grand synthesis.

There is, however, no likelihood of an early end to the search. An empirical classification such as the Periodic Table is only a beginning - we must then understand its deeper significance. With "elementary" particles we may now be approaching a situation like the pre-electronic era of chemistry. There is no understanding at present of why nature has chosen the invariances that we now seem to be uncovering. The history of science repeatedly teaches us that great discoveries beget greater; we can be confident that if we are now standing upon a threshold greater things lie beyond.

High-energy physics constitutes the present frontier of our investigation into the general laws which govern the transfer and interaction of energy in all its various forms - as matter, as motion and as radiation. We are following here the tradition established by Newton, Maxwell, Einstein, Bohr, Schrodinger, Heisenberg and Dirac. The physical principles discovered by these men, and the detailed and ingenious experiments which made their discoveries possible, are the roots of our scientific culture; their technical application is the basis of our industrial civilisation.

Already by the end of the 19th century it had been established that the motion of bulk matter can be explained entirely in terms of gravitational and electromagnetic forces, and matter had been analysed in terms of the chemical atoms or elements. During the first thirty years of this century an understanding was achieved of the structure of these atoms, of their interaction with radiation, and their chemical reactions with one another. This required the development of quantum mechanics, but the operating forces were again found to be electrical. The existence of the atomic nucleus was established by Rutherford in 1912, but the 1930s saw the discovery of the neutron and the essential properties of atomic nuclei. It was then realised that within the nucleus completely new and extremely powerful forces are at work. The foundations of nuclear technology, exploiting these forces, were laid during the war.

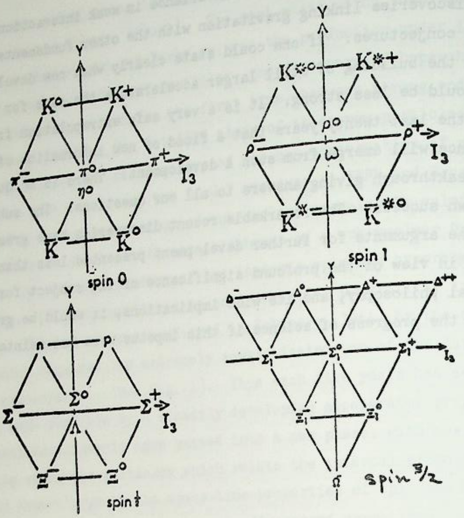
Activity since the war has been devoted to the latest phase of this fundamental investigation - to the study of the sub-nuclear particles. This has been the era of the big proton accelerators. They are the basic tools of the subject, providing the probes to search ever more deeply into the nature of matter, just as larger telescopes penetrate further into the structure of the universe. It has been an era of remarkable and very rapid development. The first generation of accelerators, built soon after the war, in the 400-650 MeV range, made possible a quantitative study of pion-nucleon interactions, confirming all the qualitative prediction of Yukawa's theory of 1935, and establishing the importance of isotopic spin. (For further details see the Flowers Report). The next phase using the 3 and 6 GeV machines at Brookhaven and Berkeley, was the production under laboratory conditions of the "strange" particles, the discovery of hypercharge, and the clear distinction, drawn for the first time, between strong and weak nuclear interactions (1952). The detailed study of these new particles led, through the $\tau$-$\theta$ puzzle, to the discovery of the non-conservation of parity in 1957. During the last five years the 30 GeV machines at CERN and Brookhaven have uncovered a whole spectroscopy of sub-nuclear mass-levels culminating during the last twelve months in the establishment of a sub-nuclear "periodic table", based on the unitary symmetry of the isotopic spin and hypercharge of the particles. (This is mentioned as an interesting possibility in the Flowers Report). This theory was confirmed in February, 1964, by the discovery of the $\Omega^-$, which required the extremely sophisticated use of the largest existing proton accelerator. (See Fig. 1). Thus each five years has seen a major breakthrough made possible by a steadily developing accelerator programme. Already the latest developments have passed into a new phase, with new patterns appearing among the particles which relate the internal properties of isotopic-spin and hypercharge to the space-time properties of spin and parity.

A curious feature of these newly-discovered symmetries is that they are approximate. At higher energies, when the mass differences between the particles are less significant, the symmetry and corresponding conservation laws should become more exact. This is an important specific prediction which should be tested. Other significant conjectures have been made about the behaviour of cross-sections at high energy, the shape of the diffraction scattering peak, and the shape of the charge and magnetic moment distributions of the particles. More important, the elaborate spectroscopy of sub-nuclear particles makes it clear that we have not yet discovered the basic building bricks of matter. There must be simpler entities which underlie this structure, just as the protons and electrons underlie the structure of the atoms. Unitary symmetry suggests a triplet of relatively massive particles, called quarks, with remarkable properties including fractional electric charge. In the relativistic theory of such particles the notion of helicity (right or left-handedness) plays a crucial role. This is also precisely the way in which parity violation shows itself in weak interactions. The electromagnetic current, not yet incorporated in the unitary scheme, nevertheless belongs naturally to it. There are thus clear hints that the weak interactions may only be weak at the energies available at present, and that a grand synthesis of the nuclear and electromagnetic interactions may take place as the energy is increased.

The significance of developments in this field, because of their extreme generality, are never long confined to the specific context of high-energy physics. Just as molecular biology is based on the atomic physics of the 1920s, so the theory of stellar evolution has developed out of the nuclear physics of the thirties. It is a reasonable speculation that the powerful energy sources - quasars - appearing in astronomy are related to quarks.

The recently discovered violation of CP invariance in weak interactions be a clue to discoveries linking gravitation with the other fundamental forces.

These are conjectures. If one could state clearly what new developments will come from the building of still larger accelerators the case for their construction would be less strong. It is a very safe extrapolation from the experience of the last twenty years that a flood of new information of fundamental importance will emerge from such a development. There is no question of a sudden breakthrough giving answers to all our questions. The subject feeds on its own success. The remarkable recent discoveries have greatly strengthened the arguments for further development presented less than two years ago, and in view of the profound significance of the subject for the whole of natural philosophy, and its wide implications, it would be greatly prejudicial to the progress of science if this impetus were not maintained.



The groups of well-established particles with the same spin and parity form regular patterns when their isotopic spin component, $I_3$, is plotted against hypercharge, Y. This is a prediction of unitary symmetry, SU(3), and the discovery of the $\Omega^-$ in February, 1964, to complete the triangle of the family with spin 3/2 and positive parity, finally confirmed the theory.

Fig. 1.

## I C: THE DESIRABLE ENERGY FOR A NEW ACCELERATOR AND THE TIMING OF ITS CONSTRUCTION

The Flowers Report reached the conclusion that there is an urgent need for a multi-100 GeV accelerator. The energy range 150-300 GeV was then under discussion and it was said that the particular choice would depend on many factors including US plans. The European Committee has now reported (The Amaldi Report, CERN/563), and its recommendation is for a machine of 300 GeV of which we give a brief description in II B (i).

The choice of 300 GeV for the energy of the CERN machine makes it fit in well with plans for development on a world scale. Accelerators of about 30 GeV are at present in operation at CERN (28 GeV), and at Brookhaven (32 GeV) in the US. The USSR plans within the next two years to commission its 70 GeV machine at Serpukhov. The recent 1965 US White Paper recommends an early start on a 200 GeV machine, construction of which is to begin in Fiscal Year 1968 with commissioning in 1973; and the later construction of an 800 GeV machine to be commissioned in 1980. Recent developments make it probable that the US will press ahead with the 200 GeV plans, endorsing the 800 GeV proposal for detailed consideration in due course.

The European 300 GeV proposal falls nicely into place in this pattern but only if we can proceed without delay. If a decision to construct could be taken early in 1967 the machine should operate in 1975. Any significant delay would bring us dangerously near the proposed date for the 800 GeV machine and would expose us to the risk of a wholesale loss of men to an already-operating 200 GeV machine.

In addition to the long time-scale of its construction, another factor which emphasizes the need for despatch in launching the 300 GeV project is the need to accommodate a balanced increase in the number of researchers in high-energy physics, a point considered in I D. If the 300 GeV accelerator is not brought into operation according to the planned time-scales, we shall tend to become relatively inadequately equipped by the middle 1970s, and such a situation is bad for efficiency and morale.

We endorse the choice by the Amaldi Committee of 300 GeV for the energy of a new accelerator and we regard such a new facility as vital for the progress of high-energy physics in Europe in the 1970s.

## I D: MANPOWER AND TRAINING

In considering the future needs in high-energy physics, and the significance of the 300 GeV machine in relation to them, it is important to be clear that such a machine could not come into operation until about 1975. We are now planning for our needs in ten years' time, and for the decade which follows, a period in which the resources of science as a whole will have increased fourfold if present trends are maintained.

In the light of later developments, we have re-examined the prognostications of the Flowers Report about manpower and the country's likely needs for high-energy physicists and have found no reason to change them: that by 1972/3 the balanced development of physics in this country, warranted by the growth of the undergraduate student body and by the scientific and technological promise of the subject, will provide at least 120 more post-PhD experimental high-energy physicists than can be supported efficiently by our UK facilities together with CERN Meyrin, whether or not storage rings (ISR) are built there.

We find that, allowing for the additional demands of graduate students, a European 300 GeV project at the proper level of utilization as now conceived, would just provide resources for the support of this increase in manpower; see II B (vi).

It must also be understood that, provided high-energy projects are properly integrated with universities, they are net producers of manpower for other scientific occupations, both at the graduate and doctorate levels. Most of the 120 post-PhD high-energy physicists in the UK in 1963 were engaged in university teaching. They were therefore helping to produce their share of graduates (about 600) and doctorates (about 40) each year. Such numbers of trained scientists, both graduate and PhD, are very much larger than those needed for the advancement of high-energy physics and most of them are available for other work.

The high-energy physics facilities available to British physicists were greatly increased with the starting of Nimrod in November 1963. It is already possible to make an assessment of Nimrod's effect on the manpower situation. The latest available figures show that 184 UK research physicists base their work on Nimrod, and 121 of them are post-doctorate. The number of physicists using Nimrod alone is therefore already equal to the total number in high-energy physics two years ago. In addition there are those in groups using the Liverpool cyclotron and the Birmingham and Glasgow synchrotrons, the bubble-chamber groups working on CERN film, those planning experiments on NINA, and the cosmic-ray groups. It seems quite clear that the estimate in the Flowers Report of 180 post-PhD physicists by 1967/8 will be significantly exceeded. Another important fact is that in a few years Nimrod will increase the total output of PhDs in high-energy physics by about 50%, i.e by about 20 new PhDs per year.

Nimrod is already approaching the number of groups it can reasonably accommodate at the present stage of development of its resources, but even if we assume that it could accommodate 180 post-PhD physicists by 1970, the present number being 121, this would mean that it was consuming only about half its own output.

The effect of the 300 GeV project on this situation can now be put in perspective. Suppose that the annual output of high-energy physics PhDs only doubles in the ten years between now and 1975, as it did in the ten years between 1951 and 1961, and so becomes 80 per year in 1975. This is almost certainly an under-estimate since the investments in Nimrod, Nina and CERN will all come to fruition in this period. Then even if the 175 UK physicists using the new European machine (see II B (vi)) were all to be post-doctorate, which they will not be, this would represent an investment of only a little more than two years' annual output of high-energy physics PhDs.

In summary, we can say that in the past, high-energy physics in this country has played an important role as a net producer of graduate and post-graduate manpower. The investment we have made in CERN and the national UK facilities has ensured that not only can this production process continue; it will be easily possible to make the necessary manpower investment in a new European project. This project in its turn will then play its proper part as a source of trained physicists for industry and the rest of science.

We give a more detailed treatment of the manpower problem from the point of view of the planned growth of undergraduate numbers in Part III of our report, III C (iii) (b). As in the Flowers Report the basic assumptions have been that the growth of high-energy physics should be based on the growth of the student body and that there should be a roughly-constant proportion of graduate students to post PhD workers.

It may also be emphasized that experimental high-energy physics provides an extremely wide and deep grounding in a great range of modern technologies, electronic, electrical, vacuum, optical, computer, magnetic, cryogenic... and that a man trained as a high-energy physicist is well prepared for work in many industrial and technological developments. Further, he brings to that work the imagination, inventiveness and critical scientific outlook demanded by, and fostered by, research in high-energy physics. Industry in the UK must, without doubt, become more technology-based; this and the equally important consideration that technology must itself become more science-based leads to the conclusion that the training of graduate students through the technologies of high-energy physics can make a very significant contribution to the technological growth of this country. Plans for the development of high-energy physics to keep it in step with the rest of physics should always have these considerations in mind.

Finally the connection between research and undergraduate teaching may be stressed. A considerable proportion of general physics teaching in the universities of the UK is now done by high-energy physicists. If high-energy physics declines, not only will nuclear physics be inadequately taught, but the general teaching body in physics will be weakened. If research in high-energy physics does not expand with the undergraduate population, physics will become unbalanced in far greater measure; for existing high-energy physicists will be driven away and under-graduate physics teaching will show a general deterioration.

# THE FUTURE HIGH ENERGY PHYSICS PROGRAMME OF EUROPE

**Report of a Working Party appointed by the**

**DSIR/NIRNS JOINT CONSULTATIVE PANEL FOR NUCLEAR RESEARCH,**

**at its meeting of 6th March, 1963.**

## ABSTRACT

Recent developments in high energy physics are reviewed, and it is shown that an early decision is required on a programme of future European high energy accelerator construction, and of expanding support for existing facilities, if significant contributions to the subject are to come from Europe from 1970 onwards. Such a programme should specifically include the construction of a new proton accelerator whose energy should be as high as possible within the range 150-300 GeV, and the provision of a pair of storage rings in association with the existing C.E.R.N. proton synchrotron. Rough estimates are given of man power and cost in relation to the whole nuclear research programme. It is strongly urged that the United Kingdom should play a full part in this European programme and that, if possible, the new accelerator should be built upon a site in the United Kingdom.

## CONTENTS

## INTRODUCTION

The primary objective of physics is to provide an understanding of the fundamental laws of nature and of the ultimate structure of matter. In terms of distance the two present frontiers of physics lie at $10^{-14}$ cm on the one hand and at $10^{+27}$ cm on the other. At intermediate distances the laws of physics are known, they carry the power of prediction, they may be applied to situations of ever increasing complexity and practical significance.

At distances beyond $10^{27}$ cm, at which lie the furthest detected galaxies, we know nothing of the laws of physics or of the nature of the universe. Only at such great distances, it would seem, can we hope to learn in what manner and at what rate matter is created. For these studies we require large optical and radio telescopes. Our largest particle accelerators, on the other hand, enable us to investigate the interactions between the elementary forms of matter down to distances of $10^{-14}$ cm. Within such distances we again know nothing of the laws of physics or of the structure of matter.

High energy physics over the last 15 years has revealed the astonishing and unexpected richness of nature in the form of some 50 recognisably different states of elementary matter, most of them only semi-stable. To understand their properties - that is, to unravel the laws which govern the behaviour of matter at such exceedingly small distances - we have soon to go to still higher energies. Our present knowledge tells us that the increase in energy, if it is to be significant, must be substantial.

Progress in high energy physics has been such that we shall need the new facilities to be operative in the early 1970's. They will take at least 7 years to build and must therefore be begun within a year or two. Before describing the proposed facilities, however, we wish briefly to review the progress that has already been made in trying to understand the nature of matter and its fundamental interactions.

## A BRIEF REVIEW OF PROGRESS

We may distinguish four stages in the gradual elucidation of the nature of matter during the last 100 years or so. There was first the recognition of the atomic constitution of matter; second, the study of the electron shell surrounding the atomic nucleus; third, the problems raised by the structure of the nucleus considered as an assemblage of neutrons and protons; and fourth, the study of the structure of the elementary particles themselves, and of the forces between them, which constitutes high energy physics. The first two stages of this process are already part of our scientific and cultural heritage, fundamental to all our science. The third, thanks to the peculiar accident of the fission process, has already given rise to a nuclear power industry: it remains a field for pure research which continues to attract deep interest and in which many basic problems remain unsolved. But more important than that, from the present point of view, the study of nuclear structure has shown that the interactions which matter undergoes are far more diverse than we had thought in the days when the inverse square laws of electromagnetism and gravitation could account for most of what we knew. For the recognition in the early 1930's that the mere existence of nuclei demanded an entirely novel kind of force between nuclear particles, enormously strong but extending no further than about $10^{-13}$ cm; the prediction by Yukawa that such forces might be mediated by the rapid exchange between neutrons and protons of an entirely new kind of particle of mass intermediate between proton and electron, and the discovery of this particle (the pi-meson, or pion) in the cosmic radiation by Powell in 1947: these were the beginnings of elementary particle physics whose secrets can be uncovered only at increasingly higher energies and with the aid of increasingly complex and costly equipment.

A first glimpse of what was in store for us was already provided by Powell's original work which showed that there were in fact two kinds of particle of similar mass, the strongly interacting one required by Yukawa to account for the binding of atomic nuclei, and a weakly interacting one (the mu-meson, or muon) whose existence was quite unexpected and whose function we still do not understand. A second glimpse was provided at about the same time by Rochester and Butler's studies of the cosmic radiation whose extremely energetic particles were found to give rise, through interactions with ordinary matter, to still further particles, the so-called K-mesons and hyperons.

All these new forms of matter we have found to be ephemeral in the extreme, most of them having lifetimes of $10^{-10}$ sec. or less. However, on the natural nuclear time scale of $10^{-23}$ sec (a distance of $10^{-13}$ cm divided by the velocity of light) such lifetimes are very long indeed, so that the interactions responsible for them must be very weak. In fact, the decay processes now appear to be very similar to the well-known beta-decay process of radioactivity which is also, on the nuclear time scale, an exceedingly slow process. It seems likely that all these decay processes involve a universal weak interaction. Like the strong interaction which binds nuclei together and is responsible for the production of elementary particles in high energy collisions, the weak interaction is of very short range, not more than $10^{-13}$ cm. An important step forward was taken in 1957 when it was found, following a suggestion by Lee and Yang, that the weak interaction violates parity conservation, seemingly one of the most natural conservation laws of quantum physics.

Some of these decay processes involve the emission of the neutrino, the uncharged and mass-less particle proposed by Pauli in 1931 to account for the properties of beta-radioactivity. This particle eluded positive experimental detection until 1959: it plays a central role in our picture of the weak interactions and its detection therefore opens up in principle a fruitful new field of experimental investigation.

Thus, to the long-range electromagnetic and gravitational interactions of classical physics we have now to add the short-range strong and weak interactions. The relative strengths of these four fundamental interactions may be described by their "coupling constants" which are of the order of 1 for the strong interaction, 1/137 for the electromagnetic interaction, $10^{-12}$ for the weak interaction, and $10^{-38}$ for gravitation. It is often conjectured that there may be an underlying unity behind all four interaction types, for instance that each of them may be mediated by further particles whose properties are closely related.

The existing high energy accelerators have enabled us to produce the pions and K-mesons and the hyperons (particles heavier than the neutron and proton) in the laboratory, copiously enough to study their production mechanisms, their decay processes, and to elucidate some of their basic properties such as mass, electric charge, spin and parity. In particular, it has been found that the particles occur in groups, called charge multiplets, within which they differ essentially only in their electric charges. Thus there are three pions ($\pi^+, \pi^0, \pi^-$) of identical spin and almost identical mass, but of positive, zero, and negative charge (in units of the electronic charge), there are two nucleons ($p^+$, $n^0$), three $\Sigma$-hyperons ($\Sigma^+, \Sigma^0, \Sigma^-$), and so on. Still further groups of "particles" have shown themselves as resonances in high energy collisions produced with the aid of accelerated particles. Their lifetimes, deduced from the resonance widths, are still relatively long. The present picture of the hyperon states of matter is shown in figure 1 which may be thought of as displaying the excited states of the basic nucleon system. Except for the proton all are unstable. There is no reason to suppose that further

states will not be added to this spectrum as time goes on: most of them have been found in the last 5 years.

The existence of the charge multiplets - sets of almost degenerate states of matter - points to a new conservation law, that of the conservation of isotopic spin, which in turn tells us that the strong interactions, whatever their detailed description, must possess a certain symmetry property, that of invariance under rotation of the isotopic spin variables used to describe the degeneracy. The same symmetry property shows itself in the form of there being simple relationships between the cross-sections for production of different members of the same charge multiplets. Further analysis of the strong interactions has suggested that isotopic invariance is only a part of the full symmetry displayed by nature: there is evidence that the strong interactions may display the so-called unitary symmetry. On the basis of this more complete symmetry a number of new states of matter have been predicted by Salam and others, and some of them have already been discovered as resonances in high energy collisions. Still other conservation laws reveal themselves as selection rules governing the production and decay mechanisms. The new conservation laws represent our first attempts to describe the laws of nature which hold at very short distances.

One interesting way to describe the structure of matter is to work in terms of the electric charge and current distributions associated with each particle. The electromagnetic "form factors" of the proton and neutron may be measured using accelerators which give finely collimated beams of high energy electrons. Pioneer work of this kind has been done during the past 10 years, in particular by Hofstadter at Stanford and by Wilson at Cornell. The interpretation of the form factors is not a simple matter: for the effect of the strong interaction between nucleons and pions is to produce a pion cloud around the nucleon and the pion cloud itself contributes to the charge and current distribution. In this way we are able to use the relatively well-understood electromagnetic interaction of electrons as a powerful tool to learn more about strong interaction phenomena: electron accelerator and proton accelerator can be used to complement each other. This was the main reason for the NIRNS decision to build the 4 GeV electron accelerator NINA at Daresbury in addition to the 7 GeV proton accelerator NIMROD at Chilton.

But in addition to the electromagnetic form factors we may also discuss, and hope eventually to measure, the strong and weak form factors. If there indeed exists a unity among the fundamental interactions it may be expected to manifest itself in closely related form factors. But the weak form factor cannot be measured adequately with any existing machine: for this we shall need intense beams of neutrinos obtainable only from the radioactive decay in flight of pions produced by proton accelerators of much higher energy than is available today.

The present generation of accelerators has thus provided us with a tantalising picture of the world of elementary particles. It seems that we stand at a point in time similar to that of the 1920's when a whole range of quantum phenomena had been recognised and when many of them had yielded to the first crude attempts at classification and interpretation, but when the Schrödinger Equation had yet to be proposed. Over the next 8 to 10 years the present accelerators will certainly enable us to learn a great deal more of the strong interactions whereby most of the particles are produced, and of the weak interactions whereby most of them decay. Not only shall we possess a list of the elementary states of the spectrum of matter, but we shall have set them down firmly in a "periodic table" whose outlines in terms of the new conservation laws are already clear to us. There will remain, however, the problems connected with the existence of so many distinct states of matter, the quantitative nature of their interactions, and the relationship between the fundamental types of interaction. Whether or not the new Schrödinger Equation has been discovered by that time, we shall need to extend the energy range, intensity, and quality of the particle beams available to us 10 years hence if the present state of knowledge of the fundamental structure of matter is to be carried forward a further significant step.

## THE PROPOSED EUROPEAN PROGRAMME

We shall not attempt to give a detailed theoretical justification for any particular advance in energy. Each substantial advance in accelerator physics in the past has been amply justified in the event and has enabled us to learn much more than we had supposed would be possible beforehand. At the frontiers of physics the unexpected becomes commonplace. The physics that is being accomplished with the 25 GeV proton synchrotron at CERN has turned out to be far more significant than was predicted even in the most imaginative attempts to justify the construction of the machine. But even though that machine will continue to be an indispensable tool for European physicists for at least a decade to come, the list of profoundly interesting problems that can be elucidated only at much higher energies continues to grow. There is, for example, the suspicion that at sufficiently high energies the weak interaction itself becomes strong. By the early 1970's we must therefore expect a tremendous interest in higher energies.

The distance down to which we may explore the nature of matter varies less rapidly than inversely with the energy of the accelerated particles. Furthermore, there are certain important high energy phenomena whose properties appear to vary only logarithmically with energy. At sufficiently high energies, it would seem, the present complex situation may acquire a new simplicity, an example of which is the prediction that certain nuclear cross sections are expected to approach each other asymptotically. Thus, to break through the present frontier at $10^{-14}$ cm we shall require a very substantial increase in particle energies.

However, there are two rather distinct physical requirements. One is to provide secondary beams of pions, K-mesons, hyperons and neutrinos of much higher energies and intensities than are available at the present time. This requires high primary intensity as well as high energy. The other is to increase the energy of the primary accelerated particles to the highest value possible without for the time being necessarily requiring a very high intensity. Both requirements have been under close study by a panel of European physicists meeting at CERN, and we are in complete accord with their conclusions, a brief summary of which now follows.

The first requirement of intense energetic secondary beams can be met only by constructing a proton synchrotron in the energy range 150 to 300 GeV and with a circulating current of $10^{13}$ protons per second. This would go some way towards meeting the second requirement also since it would provide a primary proton energy some 6 to 10 times greater than the 25 GeV available at CERN or the 30 GeV available at Brookhaven. The new accelerator would incidentally require more than a mere scaling up of the existing CERN machine: it would be economically very favourable to inject into the machine at an energy of several GeV with the aid of a preliminary synchrotron. A 150 GeV machine would be about 1.2 km in diameter and would require a total site of about 10 square kilometres, while a 300 GeV machine would have twice the diameter but would require only twice the total site area. Apart from the obvious difficulties connected with the enormous size of such a machine, the project would be able to rely upon known technologies. A sketch of a possible 300 GeV machine is shown in figure 2 in which the present CERN machine is also drawn to scale.

In addition to the strongly interacting particle beams, a machine in this energy range would produce high energy neutrino beams for the study of the weak interactions. The neutrino beams would have an intensity 2 or 3 orders of magnitude greater than is available today, so that the radically new field of neutrino physics could at last be fully exploited.

We have to fix the actual energy by further considerations. In the first place the annual cost of any machine in this energy range will be independent of the energy for the first eight years of construction (see Appendix 2 and Figure 4), so that a lower energy machine would merely be finished sooner. However, the higher the energy the more useful and versatile the machine. To this extent the energy becomes a matter of how long we are prepared to wait before the machine becomes operational. On the other hand, we should take into account high energy facilities being planned elsewhere in order to avoid unnecessary duplication. A machine of 60 to 70 GeV is already under construction in the U.S.S.R., while one in the range 150 to 300 GeV is being actively considered in the U.S.A. It is possible that in the interests of rapid completion the U.S.A. may choose to build a 150 GeV machine to be finished in 1970, and that they may simultaneously make provision for a machine in the 600 to 1000 GeV range for completion in 1980. In that case the best European contribution might well be judged by a 300 GeV machine. Be that as it may, it is clear that the actual choice of energy, bearing in mind a proper phasing of accelerator construction over the world as a whole and the degree to which international co-operation in research can be made effective, is a matter that would have to be left to the European body as a whole.

There remains, however, the second physical requirement which is to increase the primary energy to the greatest possible extent. This requirement may be met by a device which would at the same time greatly improve the general facilities and flexibility of the existing CERN machine for operation at 25 GeV. For the proton beam of the 25 GeV synchrotron could be injected over many pulses into a pair of concentric, intersecting storage rings, and the two beams so formed could be made to collide with each other with an energy of relative motion of 50 GeV. Due to relativistic effects this energy is the same in the centre of mass system as that obtained by allowing 1400 GeV protons to strike a stationary target. The addition of storage rings adjacent to the CERN machine (figure 3) would not be equivalent to the building of a 1400 GeV accelerator because it would provide so few events of 50 GeV energy in the centre of mass: the range of experiments made possible by the clashing beam technique is very restricted due to the absence of secondary beams. Nevertheless, this device would provide a window into the very high energy region unattainable by any other means within the immediate future and at a very low relative cost. This proposal, too, is under active consideration by the panel of European physicists.

It must be emphasized that the two proposals - the building of a new accelerator and the provision of storage rings for the CERN machine - are not alternatives, but two complementary aspects of the same programme of high energy physics, each in itself desirable. It would make no sense to proceed with storage rings as a cheap alternative to building the new accelerator owing to the limited range of experiments which can be done with storage rings.

## FINANCE AND MANPOWER

We shall assume that the proposed facilities are to be provided within the CERN, or some similar, European framework in which the financial commitment of the UK will continue to amount to about one quarter. Expenditure on European projects is worthwhile, however, only if it forms part of a properly integrated national programme of high energy physics research. This is already apparent at CERN where those countries with adequate home-based high energy facilities are the ones which derive the greatest benefit. The big international facilities act not as a drain upon the home-based research groups but as a stimulus to them, the best use being made of both when regular exchanges of staff take place.

In trying to formulate an overall nuclear physics programme we have to bear in mind not only the new proposals of accelerator and storage rings, but also the support and normal development of existing facilities at CERN and NIRNS, the support for home-based high energy physics programmes financed through DSIR and NIRNS, and the support of a fairly massive programme of nuclear structure research. As the ACSP itself has emphasized, the UK will derive maximum benefit from international projects only if our contribution to these projects is considered as the apex of a large home-based programme of integrated high energy studies. It takes many years, in general, before a young physicist is fit to make use of very large and costly machines of the kind we are discussing. Merely in order that these facilities should be properly used we therefore have to insist that the first consideration must be the support of university departments and of the national facilities of NIRNS and D.S.I.R.

During most of the next decade at least, the existing generation of accelerators readily available to UK physicists (NIMROD, NINA and the CERN machine) will continue to provide vital and fundamental information provided that they are properly supported and developed. It would be very wasteful - and, in the case of CERN, damaging to European goodwill - if we were not to continue to gain maximum benefit from these machines.

The fact remains that if we are to envisage the continuation of a vigorous high energy programme beyond the early 1970's, work must begin within the next year or two upon the new European accelerator since it will take at least 7 years to build.

We have tried to estimate the likely numbers of UK research workers wishing to work in the high energy field by that time. Our detailed arguments are given in Appendix 1. We may summarize the situation here by saying that the facilities already provided or under construction (including those of CERN) should be sufficient to absorb all our high energy physicists by

1967. However if we examine the present size of the physics community and project into the future on the conservative assumption that the fraction of research workers wishing to work in high energy physics remains no more than it is now, we must conclude that by 1972 there is likely to be a substantial surplus of at least 100 high energy physicists who will have to be accommodated elsewhere. On present estimates this would be considerably more than the UK quarter-share of the proposed European programme. It therefore seems quite sure that there will be no lack of scientific manpower: on the contrary, there would even be a sizeable balance for a possible new national project from 1972 onwards. Nor is the provision of an adequate number of professional engineers and technicians considered to be a serious problem although we are concerned at the shortage of really outstanding engineers required for design and development work on accelerator projects of this kind.

It is hardly possible at the present time to give firm estimates of expenditure on the proposed high energy physics programme. For reasons we have already given, the long term scale of expenditure is a matter subject to considerable variation and dependent upon the results of diplomatic as well as scientific negotiation with other nations, not only those of Europe. Nevertheless in Appendix 2 we have tried to give as detailed a financial picture as we can of the maximum likely UK expenditure on high energy physics and on the rest of nuclear physics, and our estimates are based upon our experience of the total expenditure of CERN and other large research organisations.

The Appendix shows the relative annual amounts which we consider necessary (a) to provide the new accelerator, (b) to make full use of the existing CERN machine, including the provision of storage rings, (c) to support and develop home based high energy facilities financed through NIRNS and DSIR, and (d) to continue an active programme of nuclear structure physics. This last is included only because it is customary in the UK to consider high energy physics and nuclear structure physics together for financial purposes: we do not consider it our task, nor were we properly constituted, to attempt to evaluate the needs of the nuclear structure programme, and indeed this is being done independently by a specially appointed DSIR panel under the chairmanship of Dr. J. B. Adams. In any case nuclear structure research is likely to form a rather small fraction of the total nuclear physics budget by the end of the present decade.

The total presented in Appendix 2 amounts to a doubling in annual UK expenditure on nuclear physics over a period of about 5 years, reaching about £20 million per annum in 1968, and probably continuing at a somewhat lower rate of rise thereafter as far as this presently envisaged programme

is concerned. By far the largest item would, of course, be the new accelerator: it would require a total establishment of about 4,000 persons.

## SITING OF THE NEW ACCELERATOR

The proposed new accelerator is so large that the problem of finding a suitable site for it will have to be taken largely on geological grounds. The foundations must be stable over long periods of time to about a millimetre over the 1 to 2½ kilometre diameter of the machine. Experience at CERN has shown that we shall be able to make adequate use of a new large scale facility wherever it may be built (within reason) in Western Europe. However, it seems that there are two or three possible sites in the United Kingdom which would meet our stringent geological requirements (for example, in Lincolnshire) and the question naturally arises as to whether the accelerator could be built in this country.

Of the various international projects being undertaken in Europe at the present time only the Dragon reactor is on British soil. To site the new project in this country would be an immensely encouraging step forward from our point of view, and there can be no doubt that we should derive benefit in excess of our allotted share merely by its being so readily accessible to us and by having a large international scientific community in our midst. We consider that these advantages would be such that they would obviate the need for further national requirements in high energy physics for some considerable time. Quite apart from the purely scientific aspects, however, a project of this magnitude, calling for the most exacting standards of design and construction, would provide a considerable stimulus to British industry. As a matter of fact, the consumer spending of some 4,000 staff, many of them foreign, would alone amount to about a third of the total expenditure on the subject.

We therefore consider that if it is technically feasible, a site in the United Kingdom should be offered for European consideration at the earliest opportunity.

## SUMMARY AND CONCLUSIONS

Physics is today on the threshold of far-reacting developments which will come to fruition with a new theory of the nature of matter and of its fundamental interactions. Either at this stage or the one beyond, gravitation, matter creation, and the problems of cosmology are likely to be unified with high energy physics. Quite apart from the intellectual challenge of taking part in this great synthesis of physical thought, the new picture we shall have of the nature of matter is sure to produce resounding effects upon the whole of physical science. With complete justice we may point to the developments of the 1920's and early 1930's which saw the birth of the new quantum mechanics and the incorporation within its framework of the theory of relativity. The concepts which were then introduced seemed strange and esoteric and of no practical importance; they have now pervaded the whole of science. It may well be that the discoveries of high energy physics will never in themselves be of practical significance; but the grandchildren of these discoveries, if not the children, will one day form the new foundation of everything that we do.

It seems inconceivable that a country whose scientists from Newton onwards have been in the forefront of physical discovery should not continue to take part in this most exciting of intellectual activities. So far we have been well supported. At the present time we have, or shall shortly have, national facilities for high energy physics which are bettered only by those of the U.S.A., while our share in CERN gives us opportunities which are second to none. Already the effects of CERN and NIRNS are being felt: physicists are beginning to return from the U.S.A. to make use of what has been provided at home, many of them of the highest ability. But we shall be ready for the next big step in the early 1970's and the other European nations are determined to go forward. The international development of science would be struck a most serious blow if we were not to remain with them, and in this country we should undoubtedly face a new wave of emigration.

It is sometimes feared that high energy physics, developing at the present rate, might absorb too large a fraction of the country's scientific and technological skill. However we have shown that more than enough research physicists will wish to make use of the proposed facilities if the present ratio is merely maintained. Moreover, a large fraction of research workers in this subject leave it after a few years. They have received a training on the use of large scale equipment of the most exacting nature. It would be difficult to find a better training ground for people who wish to acquaint themselves with the most advanced techniques of vacuum engineering, high power electronics, and automatic data processing. It is simply not true that high energy physics makes no contribution towards the training of useful physicists and engineers.

It is also sometimes stated that high energy physics bleeds off too large a fraction of the highest quality research workers who would otherwise devote themselves to less expensive and more immediately rewarding research. It may indeed be true that the pace of high energy research is too great. Although the pace is mainly set by the intrinsic interest of the subject, it may well be possible to moderate it somewhat by increased international co-operation in research of which these proposals form an important part. But if adequate facilities are not provided at least on the European scale, the inevitable result will be that our high energy physicists will seek appointments in the U.S.A. where adequate facilities will certainly exist. We do not believe that a greater proportion of these physicists would be content to work in other fields: they would merely work in other countries. It should surely be the aim of United Kingdom Government policy to avoid this situation by playing a full part in future European collaboration in science.

We must also consider the educational effect of a decision not to participate further in this most fundamental and challenging field of research. It is one of the most important functions of an advanced research worker in any academic discipline to educate 50 to 100 undergraduates in following generations, only a small minority of whom, of course, will take up specialised research. The removal from the university scene of research workers who have dedicated themselves to high energy physics would thus be a most serious blow to the morale and intellectual spirit of the whole scientific community of the country.

We therefore recommend that the United Kingdom should continue to play a full part in European collaboration in high energy physics, and that it should be prepared to agree to a substantial enlargement of the existing programme in order to achieve significantly higher energies along the general lines discussed in this report. In particular we endorse the specific proposals of a panel of European physicists which call for the construction of a new proton accelerator whose energy is yet to be determined within the range 150 to 300 GeV, and the provision of storage rings for the existing CERN machine at Geneva. Finally we wish again to stress the advantages of placing the new accelerator on a site in the United Kingdom.

Members of the Joint Panel who attended
meetings of the Working Party were:

Professor B. H. Flowers, F.R.S. (Chairman)
Dr. J. B. Adams, F.R.S.
Professor C. C. Butler, F.R.S.
Professor J. M. Cassels, F.R.S.
Sir Harrie Massey, F.R.S.
Professor P. T. Matthews, F.R.S.
Professor A. W. Merrison
Dr. T. G. Pickavance
Professor C. F. Powell, F.R.S.
Mr. J. Hubbard (Joint Secretaries)
Dr. J. A. V. Willis

## MANPOWER FOR NUCLEAR AND HIGH ENERGY PHYSICS

It is necessary for clarity to distinguish between low energy and high energy nuclear physics, although the two fields are considered together for financial and planning purposes in this country. The proposed big expansion is in high energy physics. The dividing line, in terms of accelerator energy, is usually taken to be the threshold for production of pions - about 200 MeV. The whole CERN programme and the proposed new international programme are in the high energy field. N.I.R.N.S. are active in both fields (the smaller accelerator at the Rutherford Laboratory is a low energy machine), and D.S.I.R. finance work in both fields on recommendations by their Nuclear Physics Sub-Committee.

The number of post-Ph.D. experimental physicists in Britain, using high energy machines, is at present about 120. A number of others make little use of machines, but study cosmic radiation. The following 6 machines are involved:

| | |
|---|---|
| CERN | 25 GeV protons |
| CERN | 600 MeV protons |
| NIMROD | 7-8 GeV protons (experiments in active preparation) |
| Birmingham | 1 GeV protons |
| Liverpool | 400 MeV protons |
| Glasgow | 450 MeV electrons |

The number working in fundamental research with low energy machines is roughly 90; there are 5 university machines (two more under construction), one at N.I.R.N.S. and one at A.E.R.E.

A study of the output of Ph.D.'s in experimental physics over the last 5 or 6 years reveals the following:

1. A third of the theses have been written on high and low energy nuclear physics and cosmic rays.
2. About 30 per cent of the physicists have stayed in the subject of their theses in British universities or at N.I.R.N.S. or CERN.
3. The number of students accepted for postgraduate research has kept pace with the increase in undergraduate numbers and, judging by the classes of degrees obtained by applicants to D.S.I.R. for grants, the quality has been well maintained.

The future output of Ph.D.'s can, therefore, be fairly confidently predicted from the planned growth of student numbers. Allowing for wastage and for transfers (which are already taking place) from low energy and cosmic ray physics to high energy machines, there would be a total of 180 Ph.D. experimentalists in high energy physics by 1967/8, and about 300 by 1972/3.

This growth is calculated on the assumption that a third of physics research students will wish to work in nuclear research as in the past and that substantially over a half of them will take up other work after obtaining their Ph.D.'s

The existing programme, to which the 4 GeV N.I.R.N.S. machine Nina will be added, will be able to absorb most of the 180 by 1967/8 and, with proper exploitation of the machines, will be able to train the newcomers among them. But there would be a surplus of at least 120 by 1972/3 if no new facilities had been built by then. The 120 would, on present estimates, be considerably more than the U.K. share of the new European programme and would leave an adequate balance for the projects envisaged in the N.I.R.N.S. long range forecasts.

A similar survey of the low energy field shows that there would, on the same assumptions, be more than enough Ph.D. research workers to exploit the existing and planned machines and the major nuclear structure machine proposed in the N.I.R.N.S. forecasts. By 1972/3 a number of the older machines will have been scrapped; at least two of the high energy machines and three of the low energy machines.

It seems certain, then, that the proposed programme can be manned with experimental physicists without diverting students from other fields, and with more than half the output of Ph.D. physicists exported to other activities as at present. By comparison, it has been estimated in the U.S.A. that if the whole of the American programme now under discussion goes ahead 3,000 Ph.D. research workers will be involved. No difficulty is anticipated in training this number.

There remains the important question of supporting staff. High energy physics needs a higher proportion of support to research staff than any other field of fundamental study at the present time. The needs of low energy research in this connection are much less and can be ignored by comparison. The present high energy programme engages about as many honours graduate applied physicists and professional engineers as nuclear physicists, and considerably more technicians. Experience in this country and abroad has shown that there is little difficulty in recruiting the applied physicists, who work on accelerator design and development, the development of research apparatus, and on data reduction. They come from the same source as the nuclear physicists, but most of them are trained "on the job", having been recruited with bachelor's degrees. They are not, therefore, a heavy load on post-graduate University teaching and their numbers are not great in relation to the output of first graduates. The number of these physicists required from the U.K. by the proposed national and international programme would be not more than 150, in addition to those now engaged, by 1972.

Similarly, there would be no difficulty in obtaining the roughly equal numbers of professional engineers required. But here there is a difficulty of quality. Both in the U.S.A. and in this country it has proved to be very difficult to obtain enough really outstanding engineers for design and development work on accelerator projects. This is the most serious manpower problem, and appears to be connected with a deficiency in the basic training of engineers which is of broader national significance than the high energy research programme.

The technical staff are recruited at lower academic levels ranging from pass degrees down to G.C.E. at "A" level, H.N.C., or O.N.C. No difficulty has been experienced in obtaining suitable staff in the present U.K. programme.

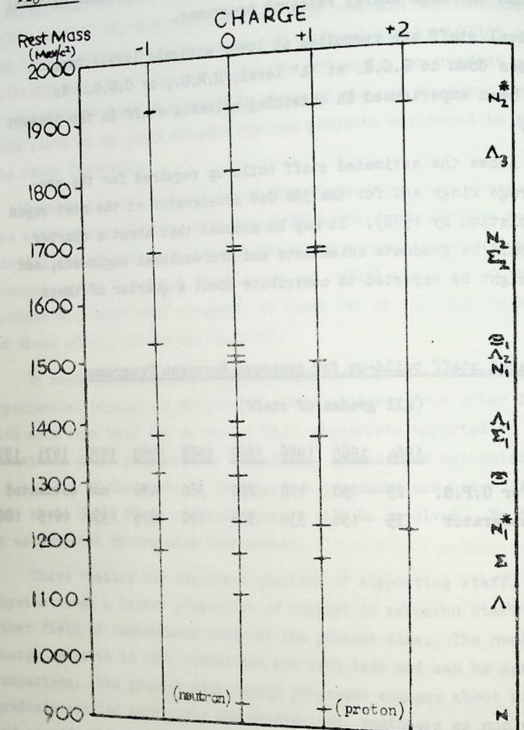
The table shows the estimated staff build-up required for the proposed CERN storage rings and for the 300 GeV accelerator on the most rapid programme (completion by 1972). It may be assumed that about a quarter of the staff would be graduate scientists and professional engineers, and that the U.K. might be expected to contribute about a quarter of these key people.

**Estimated staff build-up for proposed European Programme**

(all grades of staff)

| | 1964 | 1965 | 1966 | 1967 | 1968 | 1969 | 1970 | 1971 | 1972 |
|---|---|---|---|---|---|---|---|---|---|
| Storage rings for C.P.S. | 25 | 90 | 170 | 250 | 360 | 456 | not estimated | | |
| New 300 GeV accelerator | 35 | 150 | 330 | 505 | 790 | 1075 | 1390 | 1615 | 1860 |

Figure 1



The known hyperon states, indicating their mass and charge (November. 1962). It should be stressed that this diagram may rapidly become out of date.

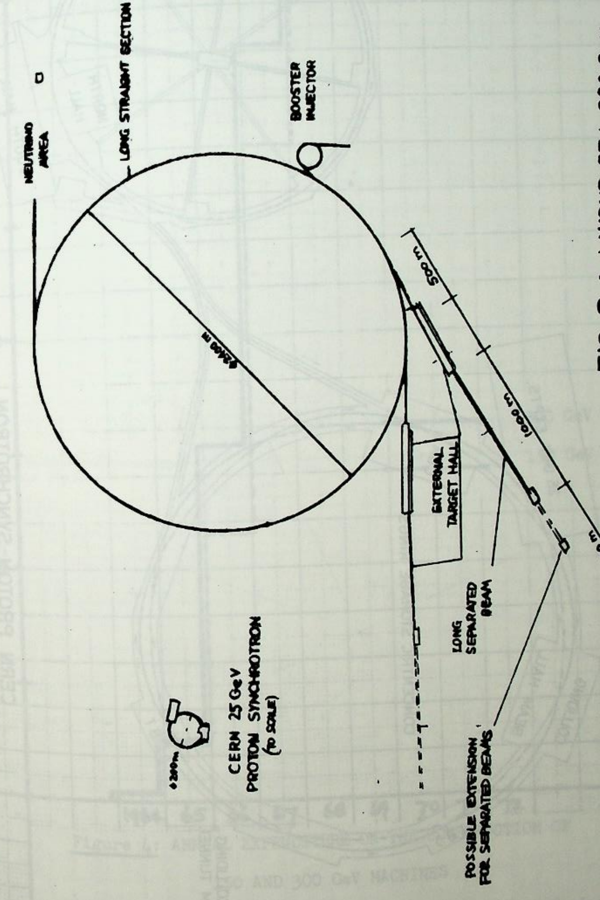
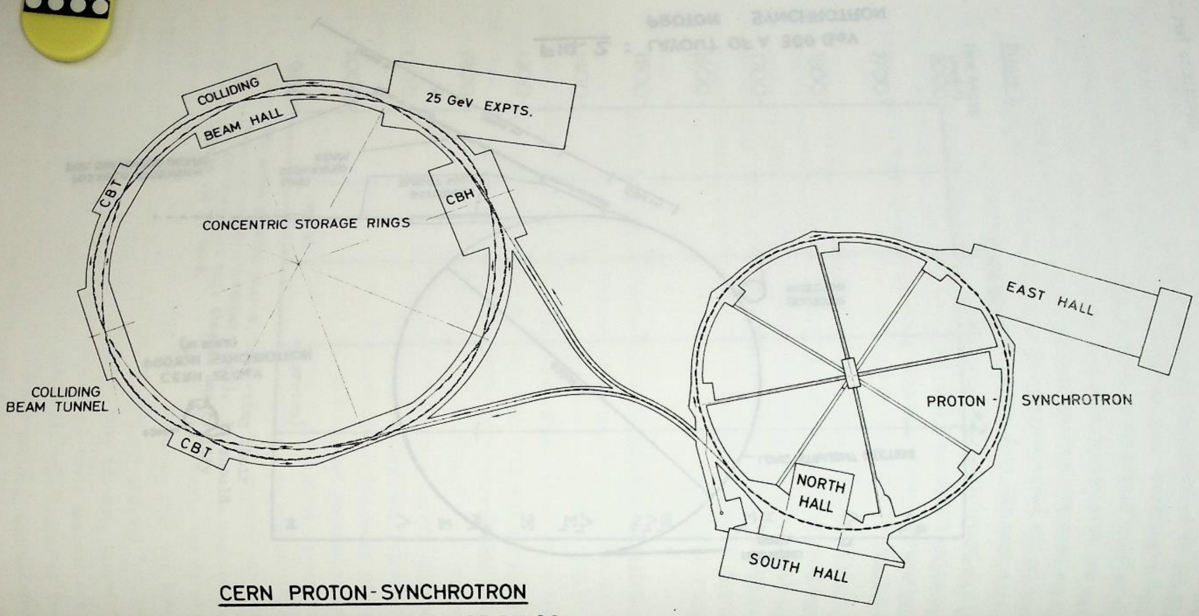


FIG. 2 : LAYOUT OF A 300 GeV PROTON SYNCHROTRON



CERN PROTON-SYNCHROTRON
WITH CONCENTRIC STORAGE RINGS

Fig. III

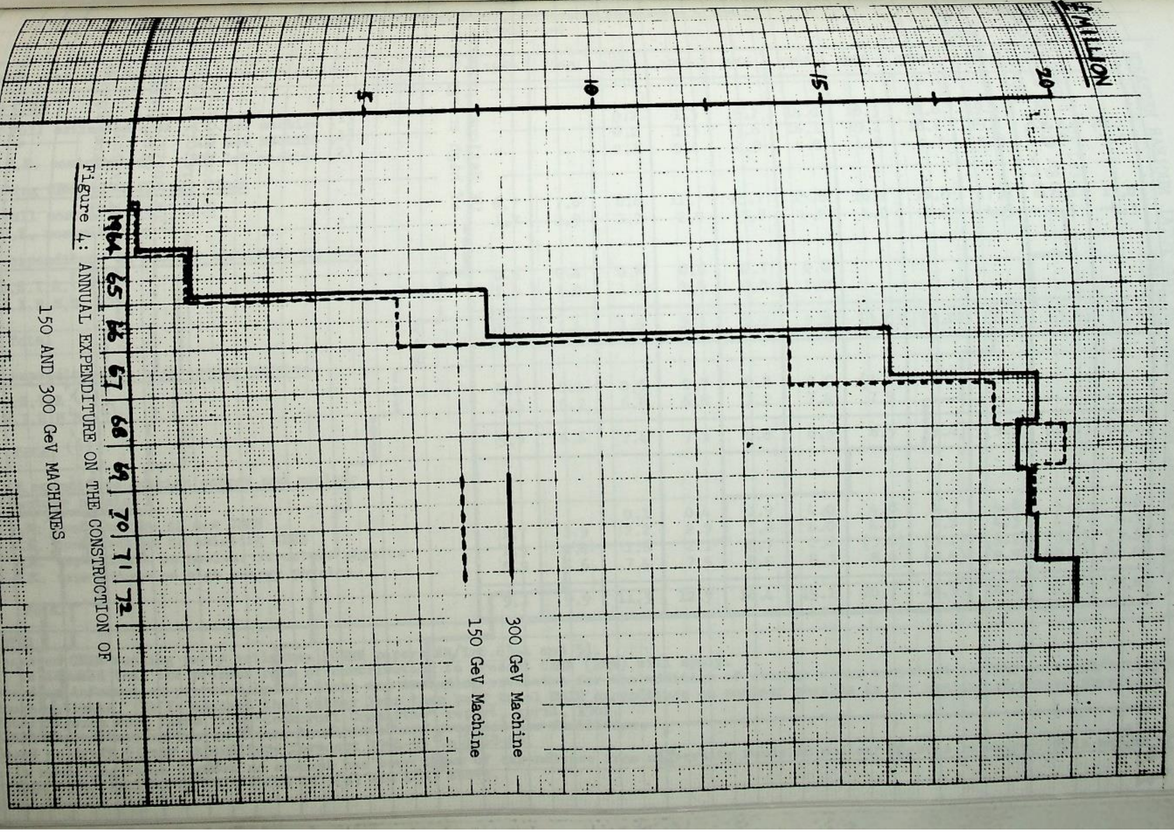


Figure 4: ANNUAL EXPENDITURE ON THE CONSTRUCTION OF
150 AND 300 GeV MACHINES

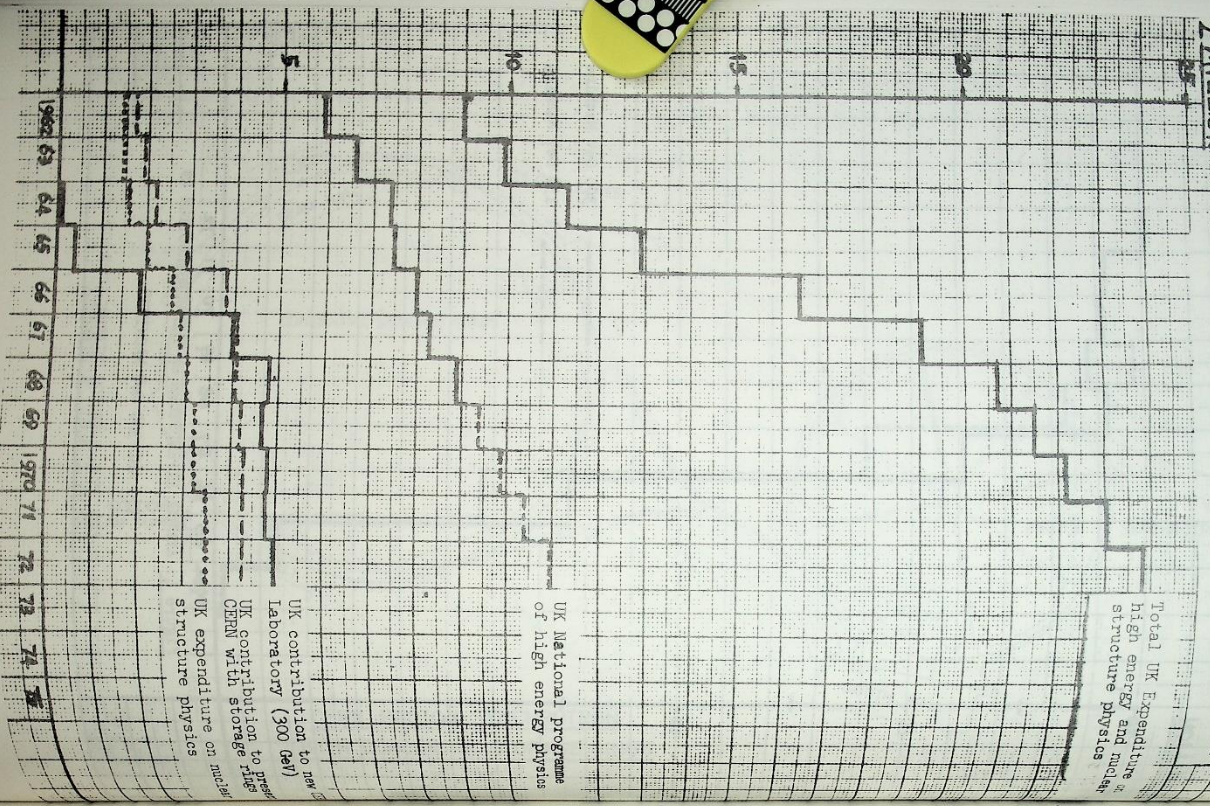


Fig 5: ANNUAL EXPENDITURE ON THE TOTAL NUCLEAR RESEARCH PROGRAMME

**Table of Estimated Costs represented in Figures 4 and 5**
(£million)

**Figure 6**

| *Calendar year or financial year beginning in* | 1962 | 1963 | 1964 | 1965 | 1966 | 1967 | 1968 | 1969 | 1970 | 1971 | 1972 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. **New CERN** | | | | | | | | | | | |
| (a) Full estimated cost 150 GeV machine (1) | | | 0.2 | 1.3 | 5.7 | 14.0 | 18.3 | 19.8 | 19.1 | | |
| (b) " " " 300 GeV machine (1) | | | 0.2 | 1.3 | 7.6 | 16.1 | 19.2 | 18.8 | 19.1 | 19.2 | 20.1 |
| (c) U.K. contribution 300 GeV machine (2) | | | 0.1 | 0.4 | 1.9 | 4.0 | 4.8 | 4.7 | 4.8 | 4.8 | 5.0 |
| 2. Existing CERN plus storage rings | | | | | | | | | | | |
| (a) Full cost (3) | 6.7 | 7.6 | 8.8 | 11.7 | 15.2 | 15.8 | 16.2 | 16.8 | 17.0 | 17.0 | 17.0 |
| (b) U.K. contribution(2) | 1.7 | 1.9 | 2.2 | 2.9 | 3.8 | 4.0 | 4.0 | 4.2 | 4.3 | 4.3 | 4.3 |
| 3. U.K. expenditure on nuclear structure physics | | | | | | | | | | | |
| (a) D.S.I.R.(4) | 0.5 | 0.4 | 0.6 | 0.6 | 0.7 | 0.8 | | | | | |
| (b) N.I.R.N.S. (5) | 1.0 | 1.0 | 1.0 | 1.5 | 2.0 | 2.0 | | | | | |
| Total | 1.5 | 1.4 | 1.6 | 2.1 | 2.7 | 2.8 | (3.0) | (3.0) | (3.5) | (3.5) | (3.5) |
| 4. U.K. expenditure on high energy physics | | | | | | | | | | | |
| (a) D.S.I.R.(4) | 0.6 | 0.4 | 0.6 | 0.7 | 0.8 | 0.9 | (1.0) | | | | |
| (b) N.I.R.N.S.(6) | 5.3 | 6.2 | 6.8 | 6.8 | 7.2 | 7.4 | 7.9 | | | | |
| Total (6) | 5.9 | 6.6 | 7.4 | 7.5 | 8.0 | 8.3 | 8.9 | (9.4) | (9.9) | (10.4) | (11.0) |
| 5. Total U.K. expenditure on high energy and nuclear structure physics | | | | | | | | | | | |
| (a) U.K. contribution to new CERN | | | 0.1 | 0.4 | 1.9 | 4.0 | 4.8 | 4.7 | 4.8 | 4.8 | 5.0 |
| (b) U.K. contribution to existing CERN | 1.7 | 1.9 | 2.2 | 2.9 | 3.8 | 4.0 | 4.0 | 4.2 | 4.3 | 4.3 | 4.3 |
| (c) U.K. expenditure on nuclear structure physics | 1.5 | 1.4 | 1.6 | 2.1 | 2.7 | 2.8 | (3.0) | (3.2) | (3.2) | (3.5) | (3.5) |
| (d) U.K. expenditure on high energy physics | 5.9 | 6.6 | 7.4 | 7.5 | 8.0 | 8.3 | 8.9 | (9.4) | (9.9) | (10.4) | (11.0) |
| Total | 9.1 | 9.9 | 11.3 | 12.9 | 16.4 | 19.1 | 20.7 | 21.5 | 22.2 | 23.0 | 23.8 |

**FOOTNOTES**

(1) Based on CERN working party estimates (CERN papers AR/Int.63-4 and 5).

(2) It is assumed that the present rate of contribution to present CERN (24%) will apply.

(3) Based on information provided by Dr. Adams and including estimates for the addition of storage rings contained in CERN paper AR/Int.SG/63-11

(4) Rough estimate. It is assumed that rather less than half total DSIR expenditure on nuclear physics is for nuclear structure physics. Total figures were taken from "Cockcroft Committee" report, April, 1962.

(5) NIRNS expenditure on Proton Linear Accelerator and proposed new facilities.

(6) Based on draft 5-year expenditure forecast 1964-65 to 1968-69.

**NOTE**: In compiling these figures no account has been taken of expenditure from university funds on nuclear physics research. This may be of the same order as that of DSIR shown in 3(a) and 4(a) above.

## FINANCE

A strong CERN working party set up in 1962 has provided estimates of the cost of both a 150 GeV machine and a 300 GeV machine. The former could be completed in 6 years; the latter would take about 2 years longer. The CERN working party's estimates have been used in this appendix.

Figure 4 shows the annual total estimated cost of both the 150 GeV machine and the 300 GeV machine. The U.K. contribution would, on the basis of the formula at present applied to CERN, amount to 24% of the total. It is estimated that in the first 8 years from the start of construction (1964) there would be no significant difference in cost between the two machines.

Figure 5 shows the estimated annual expenditure on the total nuclear research programme, if the proposal for the 300 GeV machine were adopted. The details shown are:

(a) U.K. contribution to the cost of the new European machine.

(b) U.K. contribution to the existing CERN laboratory on the assumption that storage rings will be added to the 25 GeV proton synchrotron.

(c) the cost of the national high energy physics programme financed directly from government funds (NIRNS and DSIR);

(d) the cost of the national programme for nuclear structure research (NIRNS and DSIR);

(e) the total cost of all these programmes.

Figure 6 shows in tabular form the cost estimates represented graphically in figures 4 and 5 and the sources of the estimates.

No clear prediction is available for a national high energy research programme going beyond 1967. It has been assumed for the purpose of this appendix that this expenditure will continue to increase at about the same rate for the period 1967-72 as in the prediction for the period immediately before 1967. But if the new European machine were not built, a more rapidly increasing national programme might be required in order to keep the subject in a healthy state.

The adoption of the proposal for the new European machine would involve a conscious decision that the U.K. expenditure on international programmes, which is at present about one third of the total U.K. expenditure on high energy physics, would rise to about half of the total in 5 years.

## THE SITING OF THE HIGH-ENERGY MACHINE; THE POSSIBILITY OF EUROPEAN-US COLLABORATION

### SUMMARY

1. The proper site for a new accelerator to serve European physics is in Europe and we strongly favour one in the UK.

2. A European-US machine would effect no significant financial economy to the UK for the same facilities available to the UK; we would recommend against any reduction in the available facilities.

3. A US site for a European-US machine cannot be opposed on major technical grounds.

4. We recommend against a European-US collaboration on a 300 GeV machine on a number of grounds and regard a US site to be particularly undesirable.

## II A: INTRODUCTION AND CONCLUSIONS

If the UK is to remain in high-energy physics, it is imperative that its physicists should have access to an accelerator of the highest energy. Mere access, however, is not enough; and many factors are relevant to acceptable conditions of access. After detailed consideration we conclude that any suitable European site for the high-energy machine would be acceptable but that a UK siting is most desirable. On the other hand, for a variety of reasons, access to a US high-energy machine would not be adequate for our needs and there are very serious objections to such a solution.

The arguments leading to these conclusions are here first briefly presented and followed in II B and II C by detailed considerations bearing on the utilization of accelerators in the multi-100 GeV class and on the questions of siting and costs.

Among other possibilities a tripartite European-US-USSR accelerator has been considered, although it appears not to be a sufficiently practical proposition at this time to warrant detailed study. There are very serious objections to such a proposal in relation to a machine at 300 GeV, from the point of view of physics in the UK.

It would be possible by the fullest exploitation of a 300 GeV European-US accelerator to support as many UK scientists as are now envisaged for a similar purely European machine. The overall financial economy to the UK resulting from such a co-operation of the US with Europe would, however, be small or even negative.

The only major saving to the UK would be on the capital of the accelerator itself and not significantly on the capital cost of buildings or ancillary equipment. The annual cost per scientist supported would increase for a site in the US. These other costs would rapidly exceed the capital cost of the accelerator and the financial saving would therefore become negligible or negative.

A genuine financial saving would only follow if the UK effort in high-energy physics were drastically reduced below the gross figures recommended in Part I so that the proportion of the operating time and facilities of the machine available to the UK were correspondingly less; this would upset the balance of nuclear as against non-nuclear physics in the UK and it would place Europe at a disadvantage vis-a-vis the US in the proportion of effort in high-energy physics undertaken with the most advanced facilities.

It would be possible for the UK to make some effective use of an accelerator in the US although our efficiency would be less than for a European site, and there would be additional difficulties associated with clock changes. It would be involve greater difficulties for a university man both to conduct a research programme and to participate in his university teaching. A US site cannot, however, be opposed on technical ground alone, assuming that all necessary travel facilities would be unquestioningly provided.

The fullest utilisation of a 300 GeV machine referred to above would probably result in an over-large laboratory with a corresponding reduction in accomplishment per researcher and so to a higher cost per unit of accomplishment than for a purely European machine. There would also be some danger of a serious loss of purpose and corporate spirit. We would recommend against the European-US 300 GeV concept on these grounds; and for the same reason, any fuller exploitation of a CERN 300 GeV machine than that now proposed. We recognize that as machines get bigger so must the laboratories that house them but we believe that at each stage of accelerator development the associated laboratory should be kept as small as is consistent with adequate utilization.

We have considered the benefits to the UK industrial and technological community from the various sitings. Such benefits would be negligible for a US site; for a site in continental Europe they would be significant but not great; for a UK siting they would be great. This is not a major argument for deciding between a US and a continental European site but it provides strong support for the European project with a UK site.

A significant material objection to a US site for a European-US accelerator is that it would be very difficult to retain for the UK the younger men in such a laboratory. The well-known attractions of the US would be immediately before them and in particular the very much more favourable conditions of work in US universities and industry as compared with those in the UK. This loss of men is serious not only in itself but also in depriving the parent UK universities of the intellectual and spiritual feedback without which there is little point in engaging, as universities, in work at large central laboratories. Such US counter attractions would be absent with a European site and a satisfactory feedback into the home bases would be assured, particularly for a machine in the UK. CERN was set up to bring back European nuclear physicists to Europe and keep them there; a US site for a European-US machine would promote their re-exportation and that of their successors. A US site for a European-US accelerator would actively subsidise and promote the brain-drain and on these grounds alone it should be regarded as a last expedient in the absence of any other means of gaining access for the UK to facilities of the highest energy.

There are also major arguments against a European-US accelerator with a US siting that are cultural and spiritual. Science benefits by being prosecuted in parallel at a number of closely connected but administratively independent centres. Differences between centres of outlook, ways of thought, and methods of work produce mutual stimulation; progress is then more rapid than if all the effort were concentrated in a single centre. In addition, different problems yield more readily to different modes of attack; the variety afforded by a number of centres produces a more rapid overall advance, some problems yielding more readily to the methods of one centre, others to the methods of another.

There is also, on the highest cultural plane, a European ethos that ought not to be submerged into that of the US if world culture is to develop in its fullest richness and that the preservation and development of that variety is a great human responsibility. There are great dangers or tendencies towards a mono-culture.

We believe that science is an integral part of the general culture of mankind and that high-energy physics is now playing a major role in science's contribution. A European-US accelerator sited in the US would effectively remove high-energy physics from the European scene, nullify its contribution to European culture and subtract from the European ethos what should be a great cultural force for its development. At an expense to ourselves that would probably be greater than that of doing the same job in Europe, we should be making a contribution to the ethos not of Europe but of the US. Our apparent contribution to world culture would be the same in either case but if it were made in the US it would have an unbalancing effect on the healthy tension between the European and US cultures and so in the long run the world would be poorer.

We believe such a view is not narrowly parochial. It was recognised at the inception of CERN that at a certain level of cost and sophistication scientific projects must become continental in scope. Similarly, above certain higher levels they must become intercontinental and ultimately world-wide. The arguments behind the establishment of CERN were sound. It is manifest that CERN has been a success and has made great contributions to European culture, to the European ethos and to the sense of European community. But the size of the existing CERN Meyrin is on the lowest level of what constitutes a valid objective for a European project. Europe herself can engage in very much larger undertakings without overstraining her resources of men, money or ideas, and the 300 GeV accelerator is well within her capabilities in all these respects; it would be damaging to Europe and to the world to insist prematurely on an intercontinental collaboration for its construction.

We recommend against the European-US 300 GeV concept on these grounds irrespective of the siting.

## II B: THE NUMBER OF RESEARCH PHYSICISTS THAT COULD WORK EFFECTIVELY WITH AN ACCELERATOR OF ENERGY SEVERAL HUNDRED GeV

We first make estimates on the basis of the proposed 300 GeV machine with parameters described in CERN Report AR/Int.SG/64-15. Since such estimates involve assumptions about the pattern of research so far in the future they are subject to considerable uncertainties.

### (i) Relevant machine parameters

Fig. 2 shows the schematic lay-out of the accelerator. Protons are injected into the main accelerator ring from a "booster" consisting of an 8 GeV proton synchrotron operating at a repetition frequency of 20 HZ. Injection takes place over a time of 0.6 secs. The radius of the main ring is 1.2 km and its structure embodies 12 field-free straight sections each about 30 m. long, inserted at regular intervals around the machine. Six of these straight sections are occupied by RF accelerating cavities. One is used for injection into the machine. The remaining five sections are potentially available for target or beam ejection regions. In order to reduce the problem of the build-up of induced radioactivity in the ring internal targets will commonly be avoided, the beam being extracted and led on to external targets placed in a number of different experimental areas, and to bring portions of the same pulse on to different external targets.

Three types of experimental area are envisaged. Fig. 3 shows a possible lay-out for experimental area A. This would permit the setting-up of separated beams of momenta around 200 GeV/c, 140 GeV/c, 70 GeV/c, 50 GeV/c, 35 GeV/c and 25 GeV/c from the same target. Since these beams are deflected out of the target by a common magnet it would not be possible to provide independent momentum variation for them. They are sufficiently separated spatially in the experimental hall to make it feasible to carry out several experiments simultaneously.

An alternative proposal (experimental area type B) involves refocussing the external proton beam several times in succession on thin external targets at separations of about 100 m. Each target would have its own magnet so that experiments set up around each target would be independent of those based on other targets, although of course the available extracted beam flux would be shared.

A third (type C) experimental area would be equipped with suitable muon shield to provide a facility for neutrino experiments. One can envisage several groups of experimenters using the neutrino beam in tandem as in present neutrino experiments.

### (ii) The possible number of simultaneous independent experiments

Simultaneous experiments could be envisaged in experimental areas of types A and B. Since neutrino experiments will probably need all the available machine flux it is unlikely that other experiments will be operative when an area of type C is being used.

One could envisage three main experiments utilising a large particle flux being carried out simultaneously in an experimental area of type A.

Many more simultaneous experiments, though employing a smaller particle flux, should be available in an area of type B, owing to the multiplicity of targets. Two experiments associated with each of three targets could be envisaged, making six in all.

Two experiments in tandem could be envisaged in experimental area C.

The proposed design of the 300 GeV accelerator envisaged the exploitation of only three experimental areas. Supposing one of each type were built, and the accelerator were operated for one quarter of the time for neutrino physics with the whole beam extracted into area C, and for the remainder of the time with the beam divided between A and B, the average number of simultaneous experiments that could be carried out would be seven.

If, however, all five possible straight sections were exploited, it might be reasonable to add two further experimental areas of type B, since this would enable the accommodation of the maximum number (sixteen) of simultaneous experiments.

### (iii) Trends in the future pattern of experimentation with high-energy machines

Although predictions of technical developments as far ahead as 1980 are likely to be widely off the mark, one can certainly assume a greatly increased rate of the accumulation of data. The development of filmless spark chambers that can be triggered many times during a single machine pulse, of vast arrays of solid state counters, of very large bubble chambers associated with large magnetic fields produced by cryogenic magnets will all assist in this direction. Above all, the increasing use of computers on-line with counter and spark chamber experiments and the possible storing of data in a vast central memory accessible to users in laboratories all over Europe by means of the telephone system should further decrease the machine time required per experiment.

### (iv) The number of research physicists employed using material from the 300 GeV accelerator

A relevant quantity is the number N of physicist-days of preparation and data assessment required per experiment per day of accelerator time. This quantity will vary from experiment to experiment using a given technique but the variation is likely to be greater in going from one technique to another. To obtain a rough estimate of the number of physicists likely to be involved in high-energy research programmes using the accelerator, we try to estimate rough values of N. for the main tachniques.

On the basis of experience with existing accelerators one can guess that with present-day techniques N = 100 for bubble chamber experiments, N = 20 for spark chamber experiments and N = 10 for counter experiments.

For the 300 GeV accelerator one might expect only a small change in N for bubble chamber experiments while there should be large changes of N for the other techniques with the increase of event rate per machine pulse and of on-line computer and store facilities. We make the crude assumption, therefore, that for the operation of the 300 GeV machine an average value of N = 100 will be achieved for all techniques.

This leads to an estimate of 700 for the number of experimental

physicists that would be provided for by the facilities made available if the proposed three experimental areas were constructed with the 300 GeV machine.

If, however, all five possible experimental areas were built and targets deployed in a way to provide maximum possible utilisation, it might be possible to provide material for a maximum of about 1600 experimental physicists.

It is of interest to compare these estimates with those made independently by the CERN Accelerator Study Group on the basis of the numbers of physicists using the facilities of present-day CERN. The Study Group concludes that the number of physicists who would be working on material from the 300 GeV accelerator on the basis of the construction of three experimental areas and five years after commissioning of the accelerator would be 1250, comprising 250 staff and fellows of the laboratory and 1000 other European physicists. Of the latter about 300 would be at the laboratory as visitors at any one time. These figures include also theoretical physicists but these have not been included in the numbers estimated above.

American estimates are, proportionately, considerably lower. Indeed, proportionately about half the number of physicists are using material from American high-energy accelerators compared with those using similar facilities in Europe.

### (v) The situation with an 800 GeV accelerator

Estimates of the physicist manpower likely to be involved with material from an 800 GeV accelerator are considerably less reliable even than those given above for a 300 GeV machine. From considerations mentioned above it appears likely that both the mean circulating current of protons and the number of straight sections (and thence the number of experimental areas) are likely to be proportional to the machine radius and thence to its energy. If one uses a crude scaling based on this principle one finds that for a degree of exploitation of experimental facilities comparable proportionately with that at present envisaged for the 300 GeV machine, experimental material to keep about 2400 physicists employed should be available, while for maximum exploitation the corresponding number would be about 4000.

### (vi) The number of UK high-energy physicists working with material from the accelerators

We make the assumption that in a European accelerator project the proportion of UK high-energy physicists using material would be ¼; for a European-US accelerator 1/8; while for a European-US-USSR accelerator 1/12.

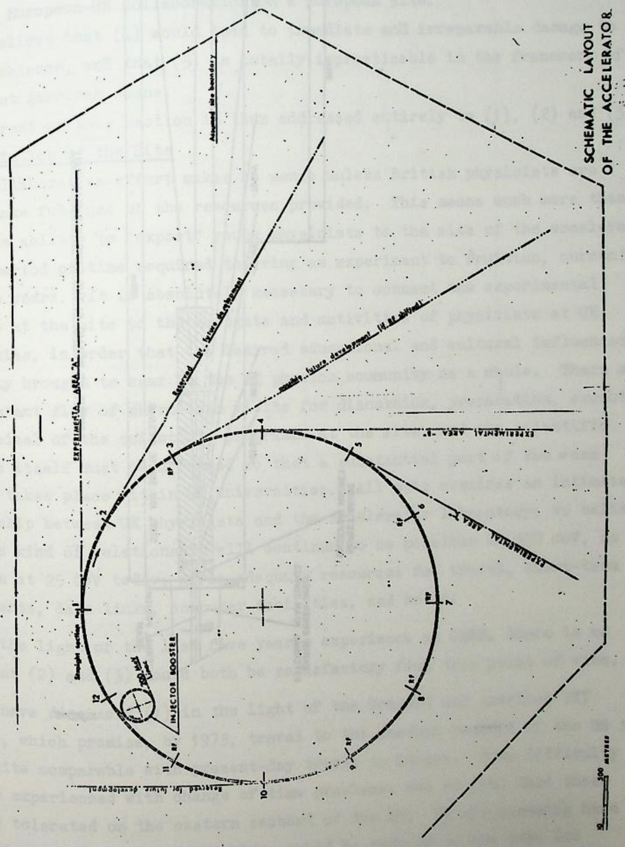
We then arrive at the following figures for the number of UK high-energy physicists that would be provided for under the different possible schemes:

| Machine Energy | 300 GeV | | 800 GeV | |
|---|---|---|---|---|
| | Proposed Utilisation | Maximum Utilisation | Half Utilisation | Maximum Utilisation |
| 1 Europe only | 175 | 400 | 500 | 1000 |
| 2 Europe-US | 90 | 200 | 250 | 500 |
| 3 Europe-US-USSR | 60 | 130 | 170 | 330 |

(vii) The optimum size of a high-energy laboratory

The present CERN laboratory employs a total staff of about 2500 persons and opinions are already being expressed that it is growing too large. The 300 GeV accelerator with the degree of utilisation proposed in the CERN Study Group Report envisages a total staff of 4660 five years after commissioning. To achieve maximum utilisation it appears likely that the total staff would rise to somewhere between 7000 and 8000 persons. Although a joint Europe-US accelerator with maximum utilisation would in principle provide experimental facilities for the same number of UK physicists as the European accelerator with the proposed degree of utilisation, it would do so at the price of a much larger and thence more unwieldy laboratory.

Similar remarks could be made with even greater cogency in relation to an 800 GeV accelerator.



SCHEMATIC LAYOUT OF THE ACCELERATOR.

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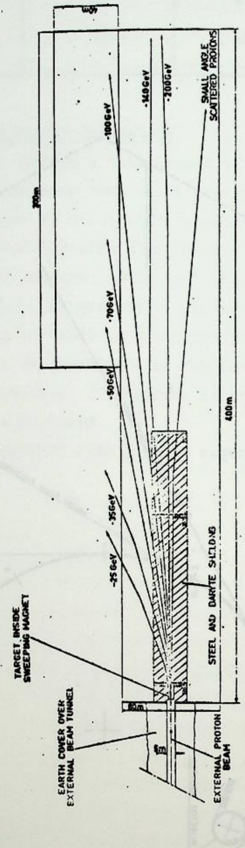
FIG 2



LAYOUT OF BEAMS IN THE EXPERIMENTAL HALL

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## II C: SITING AND RELATIVE COSTS OF THE 300 GeV MACHINE

### (i) Introduction

We now consider three specific possibilities for realising a 300 GeV machine with UK participation. They are:

(1) European-US collaboration on a US site.
(2) European collaboration on a Continental European site.
(3) European collaboration on a UK site.

The following ideas will not be discussed at length:-

(4) US-UK collaboration, independent of Continental Europe.
(5) European-US collaboration on a European site.

We believe that (4) would lead to immediate and irreparable damage to European science, and that (5) is totally impracticable in the framework of the present American scene.

The rest of this section is thus addressed entirely to (1), (2) and (3).

### (ii) Logistics of the Site

A collaborative effort makes no sense unless British physicists are able to make full use of the resources provided. This means much more than the simple ability to "export" young physicists to the site of the accelerator for the period of time required to bring an experiment to fruition, currently about two years. It is absolutely necessary to connect the experimental programme at the site to the thoughts and activities of physicists at UK universities, in order that the desired educational and cultural influences are really brought to bear on the UK physics community as a whole. There must be a constant flow of short-term visits for discussion, preparation, execution, and appraisal of the scientific programme at the site, and the scientific programme itself must be arranged so that a substantial part of the work actually takes place within UK universities. All this requires an intimate relationship between UK physicists and the accelerator laboratory; we believe that this kind of relationship will continue to be possible at 300 GeV, no less than at 25 GeV today, given adequate resources for travel, short-term appointments, data links, computer facilities, and so on.

In the light of the last five year's experience at CERN, there is no doubt that (2) and (3) could both be satisfactory from this point of view.

We have discussed (1) in the light of the Concord and American SST projects, which promise, by 1975, travel to the eastern seabord of the US in times quite comparable with present-day travel to Geneva. Some difficulty would be experienced with change of time problems, but we feel that these could be tolerated on the eastern seabord of the US. We are assuming here that immigration and customs problems could be reduced to the same low level that applies at present in Switzerland. This would certainly have to be the subject of specific negotiation with the US authorities.

The same absence of difficulty would not apply to the western seabord of the US. The range of foreseeable supersonic aircraft is too short, and the change of time difficulties would be prohibitive. We therefore feel (1) to be realistic from a strictly logistic point of view, provided that the eastern seabord of the US is chosen.

(iii) Relative Costs

As a preliminary to the discussion of relative costs, the gross national products (GNPs) of the states concerned should be considered. In 1962, according to the National Accounts Statistics available in the Central Statistical Office Library, these GNPs were:-

| States | Absolute GNP in 1962 (£ sterling at 12.2.65. rate of exchange) (x $10^{-9}$) | Relative GNP on US-CERN basis (%) | Relative GNP on CERN basis (%) |
|---|---|---|---|
| Austria | 2.6 | 0.8 | 1.9 |
| Belgium | 4.6 | 1.4 | 3.5 |
| Denmark | 2.6 | 0.8 | 1.9 |
| Fed. Rep. of Germany | 31.9 | 9.7 | 24.2 |
| France | 25.8 | 7.8 | 19.6 |
| Greece | 1.4 | 0.4 | 1.1 |
| Italy | 14.2 | 4.3 | 10.8 |
| Netherlands | 4.7 | 1.4 | 3.5 |
| Norway | 1.9 | 0.6 | 1.4 |
| Spain | 5.0 | 1.5 | 3.8 |
| Sweden | 5.2 | 1.6 | 3.9 |
| Switzerland | 3.8 | 1.1 | 2.9 |
| UK | 28.2 | 8.6 | 21.4 |
| US | 199.0 | 60.0 | - |
| TOTALS | 330.9 | 100.0 | 100.0 |

The relative figures noted above would seem to be the natural ones to assume for "proportions of scientific access to the project". As will be discussed, considerable economic and scientific benefits would accrue to the host country, and it would be only reasonable to assume that this country will have to make a special financial contribution. The above proportions, then, are not likely to represent the proportions of actual financial contributions.

We expect that UK access to the project would be reduced by a factor of 2 if (1) were adopted, as compared to the situation if either (2) or (3) were adopted. We feel that this underrates substantially the scale of the facilities which the UK needs, in view of the size of the physics community here.

So far as costs to the UK are concerned, (1) would certainly be absolutely cheaper than (2). It is, however, sensible also to consider the "cost per unit of access", and a reason has been given above for thinking that financial bargaining should reduce the "cost per unit of access" for both (1) and (2). In fact, however, we think that the following considerations more than counterbalance this, so far as (1) is concerned:-

(a) American salaries, not only for scientists but even more for supporting technical staff, are much higher than European. The need for higher salaries would undoubtedly feedback, in a much more direct way than at present, to UK-based physicists and technicians. Possibly this effect might be slightly offset by increased "productivity" on an American site.

(b) The overheads of the organisation would reflect the increased complexity of the political set-up, which in our view is too "heavy" for the task under discussion.

(c) Transport and data link costs would be increased by a factor of three to five.

(d) If there were only one 300 GeV machine, the pressure to develop it would be so strong that many facilities might become inefficient by overcrowding and over scheduling. The pressure on the present CERN organisation is a warning of this sort of difficulty.

To sum up, (1) is cheaper than (2), but it does not provide enough facilities for UK physicists. The "cost per unit of access" is likely to be higher unless a very hard bargain indeed is driven with the US on financial contributions, and we do not seem to be in a strong enough position to do this.

Some of these conclusions might be modified if the US were willing to accept much less than 60% access to the machine, but we see no reason why the US scientists should agree to this.

Possibility (3), on the other hand, offers the same notional access to the machine as (2). It therefore does provide facilities of the scale required by UK physicists. The "value per £ spent" would be much higher, if no bargaining about financial contributions were to take place. In fact this bargaining certainly will take place, and it will be a nice matter to balance the "value per £ spent" against the "cost per unit of access". It is clear that (3) is absolutely the most expensive of the three, but correct bargaining should aim to make the price worthwhile. If the price becomes too high during the bargaining process, then (2) will automatically become a good bargain with a low "cost per unit of access". The final choice between (2) and (3) depends on the result of negotiations whose quantitative outcome we cannot foresee.

(iv) Economics

The previous section has dealt with costs, but ignores a fundamental economic point. Much of the money fed into the project will make its way directly into the economy of the host nation. This is a very substanital "hidden" bonus which should be taken into account in any financial bargaining that takes place.

### (v) Effectiveness

It has been made clear that the project has important implications for the whole UK physics community. The money spent will be spent in vain if it does not lead to a continuing refreshment of the outlook of every physicist in the country. In this connection we must strongly contradict the notion that nuclear physics can be exported while other kinds of physics are retained; this kind of surgery is not possible without death to the patient. The aim of the project will not in our view be achieved by siting in the US. That country has universities and industrial organisations offering opportunities to young physicists that are clearly more attractive than those available here. In the late 1950s this led to an alarming run-down to the calibre of working physicists in the UK and threatened a serious decline in the standards of university physics teaching. This trend was only reversed when such facilities as the CERN PS and Nimrod became available in Europe. Because of the existence of these projects a substantial proportion of our young physicists proved to be willing to return to Europe, even when they were already well-placed in the US.

We view with alarm the prospect of actually assisting the westward flow of young physicists to the US by adopting (1), in a set-up in which the provision of similar counter-balancing facilities in Europe would have been explicitly renounced.

The close relationship of US and British general cultures might actually mean that the UK would suffer most severely of all the European countries.

We are therefore led to advise categorically against (1).

The same arguments, at much reduced intensity, come into the evaluation of (2) and (3). There is no doubt that the UK physics community would benefit very greatly from a UK siting, and it would be worth paying quite heavily to bring this about.

We also believe that technical benefits and developments will flow from the harnessing of local industrial resources to the needs of the project. Once again (1) would be useless and (3) better than (2).

### (vi) Politics

The arrangements connected with so large a project as the 300 GeV machine cannot fail to have their influence on the general political relationship of the states concerned, but this is outside our field of competence.

On a lower plane, however, the development of American, European, and British science is bound to be strongly affected by the decision now under discussion, and the decision itself will be strongly affected by the opinions of American, European, and British scientists. We wish to comment on the politics involved at this more restricted level.

In our opinion, UK participation in CERN has been wholly beneficial;

European science, and UK science in particular, has made very impressive progress in the last ten years. The balanced tension brought about by the near comparability of US and European resources and projects has been of benefit to both continents. American scientists have perhaps pulled off, by a narrow margin, some of the more spectacular "firsts", but for real solid work there has been little to choose. A healthy consequence of this state of affairs is that physics teaching at European universities is still as good as it is anywhere.

We have explained that, in our opinion, this progress would be ruined by (1). European-US collaboration at 300 GeV is unnecessary, disadvantageous, and potentially actually harmful.

We know that similar views are held by the scientists of Continental Europe, and we judge that their Governments have no intention of ignoring their advice.

The prolonged pursuit of (1), or indeed the absence of any active pursuit of (2) and (3), is therefore likely to have serious political consequences at scientific level. Signs of this are already becoming clearly apparent, and decisions are urgently required.

We have referred to the need for close bargaining about the terms connected with (2) and (3). Our side will be seriously prejudiced in advance unless we make timely and progressive decisions.

There is another political matter to which attention may be directed:- For logistic reasons we have seen that a European-US collaboration should only be based on an eastern seabord site for the 300 GeV accelerator. The siting of the next machine in the US has been for some time the subject of active domestic argument in that country. We do not wish to be brought into that argument, in any way that could be resented by a substantial fraction of our American colleagues.

### (vii) Summary

European-US collaboration (1), European collaboration on a Continental European site (2), and European collaboration on a UK site (3) have been considered in detail.

Logistic considerations rule out only (1) for the partidular case in which the site is on the western seabord of the US.

On an absolute cost basis (1) would be cheapest, but it would not provide sufficient facilities for UK physicists, unless the laboratory were over developed.

On a "cost per unit of access" basis, (2) would be cheapest, and (3) probably the most expensive.

On a "value per £ spent" basis, (1) would have zero or negative rating. (2) and (3) must be considered in the light of the position reached after serious financial negotiations with other CERN states.

The value judgements depend on an appraisal of the relationship of the project to the UK physics community as a whole. An economic consideration is also important.

Decisions are urgent if the UK is to retain any worthy place in the European scientific community. A specific danger of undesirable involvement in US domestic politics is pointed out.

# PART III

## THE GROWTH RATE OF THE HIGH ENERGY PROGRAMME

## SUMMARY

1. We have the responsibility of formulating long-term plans for promoting an important branch of modern physics in the UK. In the event of financial restrictions, it is possible to make responsible decisions between competing proposals, and to ensure a continuing balanced programme, only on the basis of assured resources over a period of five - ten years.

2. A fully-balanced programme for the development of high-energy physics in the UK, including provision for intersecting storage rings at CERN and a 300 GeV machine, would require a growth-rate of 19% per annum over the next five years.

3. We consider such a programme to be fully justified on the grounds of the vitality and promise of the subject, the availability of scientific and technical personnel competent and enthusiastic to undertake the work, the essential contribution it would make towards educating an increasing number of scientists and technologists, and its influence in helping to create in the UK a more scientifically-based technology.

4. If resources were restricted to a growth-rate of 15% per annum, we should have to make painful, but not disastrous, sacrifices. We should recommend abandoning, or substantially delaying, the "additional programme" of NIRNS so that the annual growth-rate of its resources could be restricted to 9%. We should wish to contribute to the ISR at CERN, but to take all possible steps to ensure that that development did not prejudice progress on the 300 GeV machine which we regard as of cardinal importance for the position of high-energy physics in the UK and in Europe in the middle 1970s and later.

The abandonment of the proper development of the Rutherford and Daresbury laboratories, or its postponement, implicit in the sacrifice of the NIRNS "additional" programme, would result in a steady decline in their significance in comparison with similar laboratories abroad. There would be a corresponding reduction in the facilities available to UK physicists and for the training of well-educated graduates and high-level scientific personnel.

5. At a growth-rate of 10% we should recommend not taking part in ISR, delaying the 300 GeV machine, restricting the domestic high-energy programme, and attenuating or cancelling the UK nuclear structure programme. We have considered a number of possible alternative programmes, but they all involve very damaging sacrifices of our scientific, educational and technical aims.

The principal consequences of such a restriction would be a large increase in the damage which we distinguish at 15%, a serious blow both to European collaboration in the subject and to the standing of the UK at CERN;

and perhaps most significant a decline in morale among the UK nuclear physics community, with profound effects on both the numbers of its pupils and their quality.

6. At a growth rate of 5% we should recommend giving up the Rutherford Laboratory. We believe that such a growth-rate would be a disaster for high-energy physics in the UK with very grave consequences for the universities, for UK science and technology. No single activity could be adequately supported in the long-term and we believe that such a growth-rate cannot be responsibly contemplated.

## III A: THE TASK

We are asked to formulate long-term plans for research and graduate training in high-energy physics in the UK on the basis of maintained growth-rates of 15, 10 and 5% per annum, assuming the allocation for 1965/6 to be M£13.8.

We welcome the implication that it will be the Government's intention to declare a policy for the support of the subject in the long term. Only with a clear view of the resources which will be available over a considerable period is it possible to formulate a balanced and integrated programme and to recommend one new project rather than another. Such a perspective is essential if we are to make the most economical and effective use of the money in promoting the many inter-related responsibilities in which we are involved. The alternative is a piecemeal budgetary approach which will tend to foster a haphazard collection of opportunistic enterprises.

There are two other reasons why long-range planning is appropriate: (i) the inevitably long time-scale of modern nuclear physics projects; (ii) the international (CERN) character of much of our work.

(i) The most modest nuclear physics projects, such as small Van de Graaff generators, involve constructional time-scales, of two to three years, while the largest that we are actively contemplating, the CERN 300 GeV accelerator, will take seven - eight years to build. Such projects cannot be abruptly terminated and the scale of their utilization cannot be rapidly altered. Nuclear physics projects are few in number and each supports many workers; and because the time-scale of experiments on the machines is often as much as three years, a substantial fraction of each worker's entire effective research life may be associated with the machine to which he attaches himself. In these conditions sudden expansions and contractions are impossible; there is a natural inflexibility that comes from the protracted nature of our work, on the one hand, and our responsibility towards the capital investment on the other. We cannot suddenly hold back a programme for a year or two in bad times, and expand it again during the good without frustrating loss of efficiency and productivity of students. Nuclear physics should not be done in that way. It should be planned over periods of time commensurate with its natural time-scales, i.e. of at least five - ten years - and the time-scales are tending to lengthen. Thus, the "Ramsey Report", dated 10th May, 1963, contained detailed planning through the Fiscal Year, 1981. Similarly, the 1965 US White Paper presents a fifteen-year forward plan. We have ourselves repeatedly felt the need for such long-term policy and are glad to participate in making proposals that may help to frame it.

(ii) In CERN we have responsibilities towards our European colleagues and their research aspirations as well as towards ourselves and our own. We believe it impossible to play our proper role in CERN without a determined long-range plan in which our attitude towards all of CERN's proposals is clearly defined. In CERN, even more than domestically, we cannot afford to approach proposals for new projects piecemeal and without a long-term framework into which to fit them.

It is unfortunately true that in CERN the UK has not always led as it should: it has often had to be dragged. We regret to have to say that over and over again, the UK has given the impression of resistance, even of prevarication, and has finally appeared to be unwillingly dragged into one or another new development. This has, most regrettably, lost us the confidence of our CERN partners.

It is not for us, at this time, to analyse the history of our part in CERN and to trace the steps by which we have gained our present low regard, but we believe that unless the UK can, within the next few months, go forward to CERN with a clearly-defined policy towards the two current major issues, the Intersecting Storage Rings (ISR) and the 300 GeV machine, European collaboration at CERN will be endangered. We do not wish to use overcolourful language, but we are anxious that, in a vital branch of modern science, the UK should not come to be regarded as the "sick man of Europe".

From these considerations, while we warmly welcome the opportunity to contribute to formulating a long-term plan, we would urge that a decision on the long-term growth-rate should be reached at the earliest possible moment. It is, for example, very difficult for us responsibly to support ISR without knowing what total resources will be available for high-energy physics in the next five years. As may be seen from later points of the report, at a sustained rate of increase of 15% p. a. we would wish to support it: but at lower rates such a commitment could involve a certain imbalance in our total programme.

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## III B: HEADINGS OF THE DISCUSSION

We warmly welcome the bringing together of civil nuclear physics under a single authority. Over the past years, we have increasingly felt the artificiality of the arrangement by which some funding has come through DSIR and some through NIRNS. This has led to awkwardness and some little confusion. In the new circumstances, it might have appeared appropriate to abandon the old categories such as NIRNS, DSIR, etc., and to formulate our plans as a single SRC package. We retain them at this stage, however, because the only detailed forecasts available to us have been prepared by these agencies. Thus, DSIR on our forecast covers all those aspects of nuclear physics at present taken care of by DSIR; NIRNS covers the two national laboratories, Rutherford and Daresbury. The former includes the proton linear accelerator (PLA) and, for historical reasons, the funding of certain university reactor research which is not nuclear physics at all. We preserve CERN as a separate heading apart from DSIR, because the funding is determined by treaty; we are not, as the UK nuclear physics community, able to effect virement of our resources as between the CERN budget and our domestic programme.

For completeness we introduce another heading, Nuclear Structure Projects (NSP) defined below in III C (g) to make provision for major new enterprises in the nuclear structure field. We do this because such enterprises are not provided for in the 65/66 budget of NIRNS; and to separate them now will facilitate the separate consideration of provision for nuclear structure research and for high-energy physics.

## III C: THE FULL PROGRAMME AND ITS JUSTIFICATION

We begin by reviewing the role and relation of the different elements in our programme and the significance we attach to them. We then estimate the growth-rate necessary for their full support and justify it. Later we consider the cuts and postponements which would have to be made to bring the costs within growth rates of 15%, 10% and 5% per annum, and the consequences which such restrictions would be likely to entail.

### (i) The Scene

We have already, in Part I, estimated the numbers of persons who should be active in high-energy physics in the UK, from the point-of-view of the needs of science, education, and the production of trained technologists, and found that our estimates are concordant with the demands of new physics graduates wishing to go into high-energy physics. We shall here only reiterate that the different elements of our programme would in no way over-provide for our needs even if all could be pursued at the greatest rate permitted by technical considerations. These elements are:

(a) DSIR
(b) The CERN basic programme, including improvement in the PS
(c) The 300 GeV machine
(d) ISR
(e) The NIRNS basic programme
(f) The NIRNS additional programme
(g) Nuclear Structure

#### (a) DSIR

The DSIR element provides essential support for work in the subject in the universities and nourishes the tree at its roots. It is that part of our programme most closely and intimately linked with teaching which we regard as part of our prior commitment and we attempt in all circumstances to leave it intact.

#### (b) CERN Basic programme

CERN represents our present contact with the front of the field and even without the compulsion of the Council vote we should wish to support it strongly, perhaps at some cost to our domestic programme. This element is inescapable and, with the DSIR element, constitutes our prior commitment, which we seek to retain at all costs.

The CERN basic programme covers the naturally-developing plans for the continued exploitation of the proton synchrotron. It also provides for the necessary improvements in the accelerator; and in particular, for the replacement of the present injector by one of higher energy and current which should increase the intensity of the accelerated beam by an order of magnitude. Similar improvements for the Brookhaven AGS are foreseen in the report which the President has referred to a Congressional Committee for their consideration (see the 1965 US White Paper); the Brookhaven AGS is a closely comparable accelerator to the CERN PS.

It would be difficult to imagine CERN's remaining in comparable standing with Brookhaven without them.

#### (c) The 300 GeV Project

As emphasised in Part II, we regard the 300 GeV project as vital to the continued well-being of UK and European physics. It is difficult to imagine doing without this machine if, as seems almost certain, the US go ahead with their 200 GeV machine and look towards the starting of an 800 GeV accelerator in the early 70s (1965 US White Paper). If we were not to build the 300 GeV machine, but rather throw all our resources into CERN and NIRNS, we could have a few years of lavishly-supported research on our existing machines. But the day of reckoning would come with the operation of the US 200 GeV machine and its denial to us of access to the front of the field; or possibly of very limited access which could have fatal effects on promoting the loss to the UK and Europe of some of our most gifted young people. In our carefully considered opinion, European high-energy physics would then wither away. All work that could be done in Europe would be carried out with much more powerful resources in the US; and further, the US would have to itself the new field of multi-100 GeV physics. It is difficult to see any significant fraction of our better high-energy physicists remaining in Europe at that time. Our efforts in high-energy physics would become contemptible, if we were permanently cut off from the highest energies.

So long as we make a substantial contribution round a machine of the highest energy, we remain in the science of high-energy physics and so an international collaboration now, or at some future time, must be considered. For the reasons given in Part II, however, we very strongly favour the 300 GeV machine as a European rather than as an intercontinental machine, and the US does not seek or need our collaboration at this stage. If we fail to secure the 300 GeV machine in Europe, we should have to wait and hope to gain a place in an eventual intercontinental collaboration at 1 TeV or so. In our view, European high-energy physics would be so weakened by that time that we should be in no position to take part in a 1 TeV collaboration. We cannot emphasise too strongly our view that the proper development of high-energy physics in Europe can come about only through the CERN 300 GeV machine.

#### (d) ISR

The case for ISR is that if two protons each of 28 GeV, the maximum energy produced by the CERN PS, are made to collide head on, the useful reaction energy available is 2 x 28 = 56 GeV. This reaction energy is not similarly available by conventional techniques (stationary target particle) until the machine's energy is about 1700 GeV, for then most of a proton's energy is profitlessly used up in providing the mere movements of the colliding system as a whole. It may well be that critically-important new phenomena come into play in this effectively trans-TeV energy range. Such knowledge would be of great importance in itself.

But, in addition, it may be of great value in formulating plans for the very costly conventional trans-TeV machines that may ultimately be required since they alone can produce beams of secondary particles of high intensity and of a variety of types. ISR has been called a window into the future, a window through which the rough outline of trans-TeV physics may be glimpsed.

A further argument for ISR is that ultimately these techniques will provide our only access to the highest energies. A conventional accelerator of 1 TeV, such as is discussed for the near future and is already the object of a design study at Brookhaven National Laboratory, will have a diameter of about 6 km. Fitted with ISR, which appears to be technically feasible, such a machine would give a reaction energy of 2 TeV. The conventional machine to achieve this same reaction energy would have nearly the same diameter as the earth. (In conventional bombardments the reaction energy increased only proportionately to the square root of the energy of the accelerator). If, as always in the past, there continues to be scientific justification for collisions of higher and higher energy, ISR offers the only foreseeable way of achieving them. The experience offered by the CERN ISR may then be of vital importance. This argument is unaffected by the fact that cryogenic magnet techniques may eventually reduce the size of accelerators by a factor of ten.

These considerations are of great weight and lead us to give ISR our strong support. Whilst the possible programme of research with ISR seems, at the moment, to be limited, it may also be very profound; and it may well extend as new forms of experimentation are devised. It should also not be overlooked that ISR is technologically very challenging and exciting.

We regard ISR as an intrinsically-exciting and important forward-looking project, not vital to our continued well-being but an element that we should forego most reluctantly as being the only thing in our programme which provides world physics with a unique instrument.

ISR can in no sense be regarded as a cheap alternative to the 300 GeV machine. When the two oppositely-moving beams are made to intersect as in ISR, collisions between pairs of oppositely-directed protons do indeed occur with the advantages we have described above. But the cross-section which one proton presents to another is so small that, even when hundreds of pulses of particles have been injected from the PS into the storage rings, the number of beam-beam collisions occurring every second is very small compared with those which would occur if either of the beams were to impinge on a target of solid matter. It is for this reason that ISR does not produce the very important secondary beams of mesons and anti-particles which are essential for many aspects of modern particle physics and which are provided only by the conventional accelerators. In the production of secondary beams it is an advantage that the secondary particles share in the movement of the centre of mass of the collision, for they are thereby endowed with greater energy of motion.

An additional advantage of ISR is that, when some hundred of pulses have been injected into a ring so that very large steady currents of circulatory protons have been established, many of the experimental techniques become much more efficient than with the intermittent pulses of particles generated by the conventional PS accelerators.

### (e) NIRNS Basic programme

We consider that a healthy domestic high-energy programme is essential for the UK. We do not believe that it is possible for a country having a major share in CERN to exploit that share effectively without the backing of a domestic programme to provide both training and a programme of experiment complementary to that of CERN but not competing with it. Smaller shareholders in CERN attempt such second-hand working, but by necessity not by choice, and their national scientific interests suffer correspondingly. We also believe that a domestic programme must be as good in its class as any. It should, in that class, either make a significant contribution on the world scale, or not be pursued at all. There is no point in struggling with inferior facilities in an energy range in which other laboratories are working with more advanced equipment; it would be bad for morale, provide poor training, and lose men. With Nimrod and with Nina we must, therefore, at least keep abreast of the field.

We may emphasize that the NIRNS basic programme is a bare-bones programme. Within it we merely keep level with high-energy physics as practised <u>today</u>, providing only for the pressures that arise out of our current studies. The basic programme contains no hidden resources for unlisted developments and no new-style ventures of any kind. It is, in fact, a programme such as we should propse even if it had already been decided to close down Nimrod and Nina in a few years' time. All our new developments, and all our hopes for retaining or even reaching a proper place vis-a-vis our sister laboratories elsewhere, in view of their already-planned developments, are in the NIRNS additional programme.

The NIRNS basic programme is indeed essential to the survival of our two UK laboratories. We cannot maintain that the additional programme is vital in the same urgent sense, but it is most highly desirable and without it the two laboratories will decline in significance in the world scene.

### (f) NIRNS Additional Programme

Roughly speaking, the NIRNS basic programme would enable us to remain on a par with other comparable laboratories <u>as they are now</u>. The additional programme would enable us to keep up with planned developments abroad. For example, the additional programme of the Rutherford Laboratory includes a new, higher energy, injector for

Nimrod, to increase the intensity of the proton beam by an order of magnitude. Such a new injector for the Argonne ZGS with which Nimrod should be compared figures in plans which the President has referred to Congress. The Nimrod additional programme also contains provision for a new large hydrogen bubble-chamber which would permit considerable advances in bubble-chamber physics; a similar chamber is already approved for the ZGS.

The additional programme for Daresbury includes an ISR project. The physics of colliding electron beams may well be a fundamental field of great richness, particularly if departures from the predictions of quantum electro-dynamics in its present simple form are discovered; the great riddle of the muon may be solved this way; and we should gain information on the electro-magnetic structure of a wide range of strongly interacting particles and so achieve a critical test of the new schemes of particle-symmetries. The importance attached elsewhere to such electron projects is shown by the current construction in Italy of a 1.5 GeV ring, and by the inclusion in the plans referred by the President to Congress (the 1965 US White Paper) of a ring for SLAC at Stanford of about the same energy as that proposed for the Nina project. Here is a chance for us to remain with world physics and we attach considerable importance to it. The giving-up of the NIRNS additional programme would take a lot from the future and much zest out of our work and would be a very painful sacrifice.

We may remark at this point that electron ISR projects have a different significance from those for protons. A number of clear-cut fundamental questions can be investigated with the electron ISR and extensive experimental programmes requiring several ISR installations exist. This is not so with the proton ISR which is more a tool for exploration, and where it is agreed that one in the world at 30 GeV is all that needs to be built in the first instance.

(g) Nuclear Structure

As a High Energy Physics Working Party we are not concerned with details of nuclear structure research. We must, however, consider overall growth rates and this involves assumptions about the allocation which may be made to nuclear structure research from the overall resources of the Nuclear Physics Board. It will be convenient at this point to establish the funds needed for its support.

We assume, with the Nuclear Structure Working Party that, if funds permit, the resources available for nuclear structure work should grow at 15% p. a. from a base of M£1.5 in 65/6, which we understand is adequate for a vigorous programme. We account for these sums as follows:

(i) For the PLA at the Rutherford Laboratory, M£0.5 p. a. throughout the period in question.

(ii) From DSIR, M£1.0 in 65/6, M£0.7 in 66/7 and the same from then on;

(iii) The remainder under the new heading Nuclear Structure Projects (NSP) which will bear all or most of the cost of any new nuclear structure enterprises.

This gives:

Table 1

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| PLA | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 | 0.5 |
| DSIR | 1.0 | 0.7 | 0.7 | 0.7 | 0.7 | 0.7 | 0.7 | 0.7 | 0.7 | 0.7 |
| NSP | 0 | 0.5 | 0.8 | 1.1 | 1.4 | 1.8 | 2.3 | 2.8 | 3.4 | 4.1 |
| TOTAL | 1.5 | 1.7 | 2.0 | 2.3 | 2.6 | 3.0 | 3.5 | 4.0 | 4.6 | 5.3 |

(We work on a ten-year period for reasons given below - III E (ii)).

In what follows the resources for the PLA remain included under the NIRNS heading, and the DSIR contribution to nuclear structure research remains under the DSIR heading unless otherwise explicitly stated, NSP being thus left as a separate item. The PLA may be closed down before the end of the period under review, but our assumption is that its resources, estimated at £M0.5 p. a., would then be made available for the nuclear structure programme unless the Nuclear Physics Board should appropriate them to the high-energy programme.

(ii) The Cost

We have carefully considered these elements and believe them all to be necessary if the UK is, by herself and through her partnership in CERN, to continue to make a major contribution to high-energy physics. To give up different elements of this programme would be damaging in different ways. We are satisfied, after detailed investigation, that the whole of this programme is fully justified. In it there is no overlapping between parts, no covert insurance policy to meet the possible rejection of one or another element. This is a desirable and realistic programme and we are obliged to urge that it should be fully supported, to the time-scales and in the amounts that the various agencies involved have proposed. These time-scales and amounts, and the NIRNS, CERN and DSIR authority for them, are specified in detail below, (III F). We here collect the data. For completeness, we have included the full DSIR figures together with the nuclear structure estimates formulated in the manner explained above. The result is the full SRC nuclear physics programme for the next five years, over which detailed estimates are available under all the headings.

Table 2

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|---|
| CERN basic | 2.7 | 3.1 | 3.2 | 3.6 | 4.0 | 4.4 |
| ISR | 0.1 | 1.2 | 1.6 | 1.4 | 1.4 | 1.3 |
| 300 GeV | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 |
| NIRNS basic | 9.5 | 10.7 | 11.5 | 12.5 | 12.9 | 14.1 |
| NIRNS addit. | 0 | 0.4 | 0.8 | 1.7 | 3.9 | 5.4 |
| NSP | 0 | 0.5 | 0.8 | 1.1 | 1.4 | 1.8 |
| DSIR | 1.4 | 1.2 | 1.2 | 1.4 | 1.6 | 1.8 |
| Total | 13.8 | 17.3 | 20.1 | 24.9 | 29.8 | 33.7 |

(M£0.2 has been displaced from 65/6 to 6/7 (M£0.1) and to 7/8 (M£0.1) for ISR in order to leave our 65/6 total at its conventional value of M£13.8). If we now define the effective growth-rate as that steady rate, starting from the stated value for the first year, which brings in the same total sum over the period in question, these figures correspond to an effective growth-rate of 20.7% per annum.

As we remark below, III F (i), we do not consider it possible, for reasons external to this country, to start the 300 GeV machine to the time-scale implicit in Table 2. A more realistic approach would seem to be to delay the 300 GeV machine by one year; this gives our "realistic" programme. The result is then:-

Table 3

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|---|
| CERN basic | 2.7 | 3.1 | 3.2 | 3.6 | 4.0 | 4.4 |
| ISR | 0.1 | 1.2 | 1.6 | 1.4 | 1.4 | 1.3 |
| 300 GeV | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 |
| NIRNS basic | 9.5 | 10.7 | 11.5 | 12.5 | 12.9 | 14.1 |
| NIRNS addit. | 0 | 0.4 | 0.8 | 1.7 | 3.9 | 5.4 |
| NSP | 0 | 0.5 | 0.8 | 1.1 | 1.4 | 1.8 |
| DSIR | 1.4 | 1.2 | 1.2 | 1.4 | 1.6 | 1.8 |
| Total | 13.8 | 17.2 | 19.3 | 22.7 | 28.4 | 33.4 |

This corresponds to an effective growth-rate of 19.3%. It may be noted here that, to some degree, the CERN basic programme in the form that we discuss it and have costed it in Table 3 is inter-connected with ISR. In effect, ISR allows certain savings on the basic programme for its budget contains about (total cost) M£5 worth of facilities (experimental hall, beam equipment, etc.), that would have to be added to the basic programme if ISR were not built. This must be borne in mind in our later discussions although we have not there attempted to correct for it. If ISR were not built, the CERN basic programme as costed here would not be enough to keep the CERN PS abreast of the Brookhaven AGS and a further M£0.6 - M£0.8 per year (UK contribution) would then be called for by CERN.

We consider the CERN basic programme as inescapable both scientifically and because the UK can be voted into it; we take it as an invariable element of our subsequent discussion, part of our prior commitment.

### (iii) The Justification

In justifying a growth-rate of 19% we make some observations of a general nature and others relating particularly to the present position in high-energy physics in the UK and Europe.

#### (a) General considerations

It is sometimes objected that a sustained annual growth rate of 15 or 20% involves such a rapid increase in expenditure on science that in the course of fifty years, starting from present rates, it would take up a large fraction of GNP. It is also true that if an infant continued the progressive four-fold increase in weight which it commonly makes in its first year, it would weigh as much as the whole earth by the age of fifty. We know of no principles which would suggest that the present expenditure on fundamental science, running at about three parts in 1000 of the GNP, should be regarded as an upper limit, nor that the headlong advance of science and technology, in a peaceful world, is not likely to continue. We see no reason in principle, with which to oppose the suggestion made by some chemists, that in fifty or a hundred years, science may absorb a very large part, perhaps a half, of the total creative resources of mankind. Whilst it could not be dogmatically asserted, it appears to us not an unreasonable speculation, and useful in tending to prevent us taking too narrow a view to which we are often prone. And what we know is that in the past fifty years, scientific resources have been increasing at about the rates we are contemplating and they have continually transformed our technological resources and the whole social fabric. We believe they will continue to do so.

It is also sometimes said that whilst fundamental physics has in the past made decisive contributions to other sciences and technology, its present studies in high-energy physics are so remote from matter in the forms wich which we deal in terrestrial conditions, that it is unlikely to have an important bearing on other sciences and technology in the future. It is again not reasonable dogmatically to assert the contrary, but exactly similar remarks could have been made about decisive discoveries in physics of the past. Who, for example, could have asserted at the time of early experiments and speculations about electricity, when almost the only phenomena in which it was manifest was the lightning flash or the galvanic twitching of a frog's leg, that it would play the overwhelmingly important role in every aspect of our industrial and scientific practice that it does today. Or who in 1905 would have ventured to assert the practical role of nuclear physics, or its essential contribution to our understanding of the energy generation in stars, or its great significance for many other sciences including chemistry and geology. Many other examples not less impressive might be quoted.

In spite of the division of natural philosophy into a number of distinct disciplines, it is in fact a whole in which there is a most complex mutual interaction between the parts. We know of no simple principles which would allow us to compute the proper allocation of limited resources among different disciplines. We would not advocate, for example, that some sciences which make little contribution to the education of scientists at the undergraduate level should therefore be abandoned or exceptionally restricted although, as we have pointed out in Section I D, high energy-physics makes an important contribution to undergraduate training. It is not always easy to assess the relative significance of important discoveries in quite different disciplines. The cost of educating a PhD student is more expensive in high-energy physics than in some disciplines, less than in others, but fundamental physics has always been remarkable for the wide-ranging consequences for other sciences of its changing basic conceptions of the nature and constitution of matter and the very powerful technical resources to which its discoveries have led. The fact that we cannot at the moment see how the present discoveries and innovations of high-energy physics will be applied parallels similar situations in the past and merely reflects the fact that practice cannot immediately follow the progress of discovery.

(b) Particular considerations

As a result of the hard work and resources put into high-energy physics in the UK and in Europe in the past ten years, we have built up a very creditable, if not pre-eminent position on a world scale, and a substantial impetus in the subject. The success of the great accelerators at Nimrod and CERN, and the needs of the subject, make it most desirable that they should be further developed and fully exploited. We therefore warmly support those provisions in Table 3 which would allow such developments. They are very important elements towards ensuring that the position which has been built up will be maintained and improved. Nimrod and the CERN PS must not fall behind the Argonne ZGS and the Brookhaven AGS with which it is natural to compare them. Nina must similarly be enabled to take her proper place alongside the large electron synchrotrons in Hamburg (DESY) and Cambridge (CEA). The CERN and NIRNS basic programmes and the NIRNS additional programme make the necessary provision for such progress. (Note that the CERN basic programme includes improvements of a type that, within our domestic convention, have been put into the NIRNS additional programme).

Beyond these existing machines we must contribute on a world scale in new developments; we need above all the proposed CERN 300 GeV machine and we should pioneer the important though limited insight into the future that is offered by ISR. Considerations of science and manpower, technical or scientific, show that these developments could be made contemporaneously. We know what to do and why, and we are ready to do it. The denial of any element of this programme will disturb the proper and anticipated development of UK and European high-energy physics. The rejection of ISR would, indeed, disturb the development of high-energy physics on a world scale. The US is looking to CERN to provide, through ISR, the unique glimpse of the trans-TeV range that may be of extreme importance in coming decades (see the 1965 US White Paper).

The growth-rate of 19% is needed and justified on the practical grounds of the state and momentum of the science. It is also justified on the quite different grounds of the expansion of our general UK educational facilities and the sophistication of the discipline. University places are expanding at an overall rate of

6% per annum or more. This makes it essential, if we are to continue to play our balanced part in the science of the country, to increase our graduate student intake into high-energy physics at least in proportion. The expansion of scientific research can then remain in step with the expansion of the undergraduate scientific body, and high-energy physics with the rest of science; principles that we adopted as fundamental in Part I of this Report.

Three factors suggest that the rate should, in fact, be higher than 6%; the first two factors are common to all sciences, the third peculiar to high-energy physics:-

(i) With the increasing emphasis on science and technology in the UK, a swing towards science in schools, inhibited in some small measure at the moment by a shortage of good science teachers, is inevitable. As industry learns to use scientists properly for for R and D work and, increasingly, in management, and as Government service in all its branches, administrative as well as nominally scientific, increases its call for scientists, science's share of the total number of students must increase.

(ii) As the country moves towards a technology-based economy, it must also move towards a science-based technology. A technology that merely exploits the results of scientific research at second hand is almost static. A dynamic technology must contain within itself the mainspring of its own further development; it cannot afford to fall further and further behind the advancing front of science. It follows that an increasing proportion of science graduates should go into research to maintain contact with that advancing front, and not directly into industry at the level of training represented by the first degree. It also follows that there should be some infiltration of the ranks of research scientists by those whose initial training has been more technological - a healthy relationship between science and technology cannot be a one-way traffic. For support of this view, and for a commentary on the UK's regrettable backwardness in recognising a decisively-important tendancy, one need only consider the principles and practice of the Massachusetts Institute of Technology, the California Institute of Technology and the Eidgenössische Technische Hochschule, Zurich.

(iii) The UK has made a very considerable investment in high-energy physics facilities particularly at the Rutherford and Daresbury Laboratories. Nimrod at the Rutherford Laboratory is only just coming into full operation and Nina at Daresbury is not yet even completed. The availability of these two powerful machines will inevitably attract nuclear physics students Although, as a matter of policy, we should not wish to see a significant increase in the proportion of all physics students going into nuclear physics research, there will be some pressure for this. Further, we should consider some shift towards high-energy physics from the nuclear structure side, within nuclear physics as a whole, as reasonable. A new equilibrium will be established, but this will take some time. Until then, the student growth rate in high-energy physics must be expected to be abnormally high. Such an effect, which should be carefully controlled, is already clearly seen in those university departments where both high-energy and nuclear physics research are pursued.

We believe that each of these effects, during the five - ten years, may well contribute about 1%. A more realistic figure for the rate of increase of high-energy physics graduate students may therefore be significantly greater than 6%; probably towards 10%.

The rate of increase of the graduate student body does not, however, itself determine the rate of increase in the total number of researches in the subject. But we believe we may reasonably expect a total growth-rate in the number of researchers to match the number of graduate students in the subject. In particular, we believe that it is reasonable to expect a total growth-rate of about 10% p. a. in the number of researchers, and the maintenance of approximately the present ratio between graduates and post-PhDs.

In addition to an increase in the expected number of researchers, the sophistication factor has to be taken into account; i.e. the escalation in the cost per researcher that attends the continual introduction of new techniques and methods of experimentation. This factor is a minimum of 5% for all sciences and it is significantly higher, perhaps 10%, in the growing points of science, of which high-energy physics is one.

We see that these two factors, the 10% rate of rise in the strength of the research body and the 10% sophistication factor lead to an expected financial growth-rate of around 20%. We regard this as a confirmation, from another point of view, of the need for a growth-rate of 19% to satisfy our programme. We repeat our view that the 19% growth-rate is demanded by the circumstances and that a lower rate would represent a damaging interference with the development of the subject and our responsibilities in relation to national policy on science, education and technology.

(iv) Summary

In summary, we believe that our programme for nuclear physics in the UK, which calls for a growth-rate of 19% over the next five years, is entirely reasonable in view of the demands which the discipline imposes at this stage of its development and is justified by the combination of: (i) the growth-rate to be expected in the research body, which is a consequence of the Government's policies of expansion in the universities and of the increasing role of science; and (ii) the high sophistication factor in nuclear physics.

## III D: 15%, 10%, 5%: OUR RELUCTANCE

We approach that part of our task which involves the analysis of growth rates of 15%, 10% and 5% with some anxiety. We repeat that we regard our recommendation of a 19% growth-rate over the next five years as fully justified and in discussing lower growth-rates we would not wish to retreat from that position. We may reiterate that we believe the full programme to contain no indefensible elements or any lavishness in the forecast provisions, no redundancies, overlapping, or hidden reserves. Any substantial cut-back from the programme of Table 3 will have wide and damaging consequences and we believe they ought to be resisted.

In approaching the task we are asked to perform we are therefore not adopting the standpoint that a man who agrees to participate in the construction of his own gallows forfeits his right to protest at being hanged upon them.

## III E: PRINCIPLES OF DISCUSSION FOR GROWTH RATES OF 15%, 10%, 5%

### (i) Literalness

We are asked to consider the consequences of growth-rates of 15%, 10%, 5%, and we shall attempt to propose programmes which match the assumed resources, within the estimates and forecasts now available to within a few hundred thousand pounds. We shall not, for example, in considering 10%, argue for a rate of 11%.

### (ii) The Time Scale

So far we have presented proposals covering the next five years, but in considering growth-rates below what we believe to be necessary, we must both cut and postpone. We then find it necessary to consider a longer term. We should, in any case, prefer to present plans that cover longer periods, more commensurate with the natural time-scales of the larger elements of our programme such as the 300 GeV machine and ISR: ten years rather than five.

There is a second reason for adopting a ten-year period for a forward look. We feel that any present long-range forecasts should show a decrease of the growth rate if they are to be acceptable. Such a tendency cannot, however, be expected in a time less than the natural time scale of the discipline. It is the characteristic feature of a living and growing science that it produces fundamentally new and unexpected discoveries which, by their very nature, cannot be foreseen; they often extend greatly the field of investigation and increase the expense of essential experimental resources. But at least we must assure ourselves that, over a time of the order of the natural time-scale of the discipline, the rate of rise of our best forecasts tails off. In that case some real reduction in growth-rate may be achieved; and we are in a better position in the event of the imposition of a diminishing growth-rate or a decline in the significance of the subject, to run down our programme in a rational way.

Some of us believe that high-energy physics, pursued within the limitations imposed by the rate of increase in the University population, will in fact show a levelling off in expenditure at a certain stage. But when it occurs will depend on the development of the subject and, in view of its immense promise and vitality, it seems to us most unlikely in the next decade. But there are many historical precedents for disciplines which were overtaken by others which grew to present a superior intellectual challenge.

We have therefore taken a ten-year period as the basis of our exercise on the 15%, 10%, 5% problem to see under what circumstances a diminution of the growth-rate appears to occur. We wish to repeat the above warning, however, that such a diminution may turn out to be illusory for a variety of reasons, some of which we shall touch on as we go along.

(iii) Contingencies

In matching our forecasts as accurately as possible to the growth-rates we shall make no explicit allowance for contingencies above that already implicit in the individual elements of the forecasts. It is the experience with major enterprises of the type with which we are chiefly concerned here that escalation (beyond the normal effects of costs and wages) is usually associated with difficulties, and so with an increased time-scale. The escalation then tends to appear in the total spend rather than in the rate of spend. It would therefore not affect rate-of-spend forecasts such as these unless the construction project ends within the period in question. The only major terminating project of this kind in our programme is ISR, and although there are novel elements in it, it is for the most part a very similar enterprise to the CERN PS. It will be on the same site so that all the PS experience has been available in costing which should therefore be exceptionally reliable. Costs of exploitation are notoriously difficult to estimate. We have allowed the ISR experimental costs to increase at 15%. The 300 GeV machine does not become operational during the period under review. For these reasons we believe that our procedure of not showing a separate contingency on the programme as a whole is correct. The absence of an overall contingency means little opportunity for a change of mind.

(iv) Shadow Cuts and Delays

We have made no allowance for shadow cuts or delays except in our NIRNS programme. It is not possible to make sufficiently accurate estimates of such items in other people's projects to permit us to use them reliably for the improvement of our own programme. We prefer simply to bear them in mind as something effectively on the credit side in our plans, and as offsetting the lack of contingency against escalation of rate-of-spend.

## III F: COSTS OF THE TEN YEAR PROGRAMMES

(i) CERN

The source of the CERN figures is CERN/SPC/196 dated 19th February, 1965.

In the case of the basic programme we have used the Director General's figures through 1969 and have allowed a 10% growth-rate beyond that (the DG's growth-rate, 69/71, is 5%). The basic programme is then:

Table 4

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Basic: | 2.7 | 3.1 | 3.2 | 3.6 | 4.0 | 4.4 | 4.8 | 5.3 | 5.8 | 6.4 |

In the case of ISR we have, since it is a novel project, allowed a growth rate of 15% beyond 1972, the last year of the DG's figures. This gives:

Table 5

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| ISR: | 0.1 | 1.2 | 1.6 | 1.4 | 1.4 | 1.3 | 1.5 | 1.7 | 2.0 | 2.3 |

We shall also have to consider the effect of displacing ISR by one year:

Table 6

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| ISR - 1: | 0.1 | 0.3 | 1.1 | 1.5 | 1.4 | 1.4 | 1.3 | 1.5 | 1.7 | 2.0 |

(The M£0.2 displaced from 65/6 in Tables 2 and 3, III C (ii) has now been re-distributed. We introduce the notation ISR - x to mean a time schedule slipped back x years relative to the schedule of SPC/196.

In the case of the 300 GeV machine we depart from CERN's schedule as mentioned in III C (ii), by putting it one year later. We do this for two reasons. First, we understand that it is most unlikely that our CERN partners will be able to meet the schedule of SPC/196; secondly, to adopt the time-schedule of SPC/196 for the 300 GeV machine would bear so heavily on the total budget for a growth-rate of 15% that we could not, within the spirit of III E (i), agree ISR and safeguard our basic domestic programme; the time-scale of SPC/196 would then kill ISR. With this one year displacement from SPC/196 we have:

Table 7

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| 300 GeV | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 | 5.4 | 5.4 |

(Subsequently the notation 300 - x will mean a 300 GeV schedule slipped back by x + 1 years from SPC/196).

We now have the full CERN figures:

Table 8

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Basic | 2.7 | 3.1 | 3.2 | 3.6 | 4.0 | 4.4 | 4.8 | 5.3 | 5.8 | 6.4 |
| ISR | 0.1 | 1.2 | 1.6 | 1.4 | 1.4 | 1.3 | 1.5 | 1.7 | 2.0 | 2.3 |
| 300 GeV | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 | 5.4 | 5.4 |
| Total | 2.9 | 4.4 | 5.1 | 6.0 | 8.6 | 10.3 | 11.2 | 12.0 | 13.2 | 14.1 |

or, with ISR delayed by a year:

Table 9

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Basic | 2.7 | 3.1 | 3.2 | 3.6 | 4.0 | 4.4 | 4.8 | 5.3 | 5.8 | 6.4 |
| ISR - 1 | 0.1 | 0.3 | 1.1 | 1.5 | 1.4 | 1.4 | 1.3 | 1.5 | 1.7 | 2.0 |
| 300 GeV | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 | 5.4 | 5.4 |
| Total | 2.9 | 3.5 | 4.5 | 6.1 | 8.6 | 10.4 | 11.0 | 11.8 | 12.9 | 13.8 |

### (ii) DSIR

DSIR figures for 65/6 through 70/71 come from WP/HEP/6 dated 25th February, 1965; for subsequent years we have maintained a growth rate of 15% with a notional reduction of M£0.4 below the figure appropriate to that rate in 72/73 to represent take-overs at the beginning of the 72-77 UGC quinquennium.

This gives:

Table 10

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| DSIR | 1.4 | 1.2 | 1.2 | 1.4 | 1.6 | 1.8 | 2.1 | 2.0 | 2.3 | 2.6 |

### (iii) NIRNS

We take the NIRNS basic and additional programmes from the five-year forecast in NX/65/22 dated 24th February, 1965. We extrapolate the basic programme beyond 70/71 by continuing the growth rate of 8.8%:

Table 11

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Basic | 9.5 | 10.7 | 11.5 | 12.5 | 14.1 | 15.3 | 16.7 | 18.1 | 19.7 | 19.7 |

The NIRNS additional programme is available only as a five-year forecast and we have no definite figures for it in the latter half of our ten-year period. As a guide to the sort of sums that could be involved if full development were possible, however, we have extrapolated the programme beyond 1970/71 at 15%. We do not use this total again.

Table 12

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Addit. | 0 | 0.4 | 0.8 | 1.7 | 3.9 | 5.4 | 6.2 | 7.1 | 8.2 | 9.4 |

We should remark that the NIRNS figures for the first five-year period covered by NX/65/22 are already heavily shadow-cut.

### (iv) Nuclear Structure

The provision here, under the heading NSP, has already been explained in III C (i) (g).

Table 13

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| NSP | 0 | 0.5 | 0.8 | 1.1 | 1.4 | 1.8 | 2.3 | 2.8 | 3.4 | 4.1 |

## III G 15%

(i) Principle

We may remark that we cannot get the different elements of our programme in a rigid order of priority which, in the event of financial restrictions, could be nibbled away from the bottom. Nevertheless the elements of the programme can be partitioned into three principle groups:-

| | | |
|---|---|---|
| I (Prior) | | DSIR, CERN Basic; |
| II | : | 300 GeV, NIRNS Basic; |
| III | : | ISR, NIRNS Additional. |

Group I is indispensable, and Group II has clear priority for us over Group III. In particular, we should not like ISR to go ahead until we feel that NIRNS Basic programme is secure, and that acceptance of ISR will not jeopardise the 300 GeV project. It is clear that any conflict within Group II would be extremely painful. Within Group III we feel that the novelty and excitement of ISR warrant our giving it priority over NIRNS Additional; but if funds would not run to ISR we should then like to go ahead with some fraction of NIRNS Additional.

Our first line of retreat, if we could not secure the full 19% support for our work, would be to give up the NIRNS additional programme, keeping CERN Basic, ISR, 300 GeV, NIRNS Basic and Nuclear Structure.

(ii) Incidence

Such a programme can be supported at a 15% growth rate.

Table 14

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| CERN | 2.9 | 4.4 | 5.0 | 6.0 | 8.6 | 10.3 | 11.2 | 12.0 | 13.2 | 14.1 |
| DSIR | 1.4 | 1.2 | 1.2 | 1.4 | 1.6 | 1.8 | 2.1 | 2.0 | 2.3 | 2.6 |
| NSP | 0 | 0.5 | 0.8 | 1.1 | 1.4 | 1.8 | 2.3 | 2.8 | 3.4 | 4.1 |
| NIRNS | 9.5 | 10.7 | 11.5 | 12.5 | 12.9 | 14.1 | 15.3 | 16.7 | 18.1 | 19.7 |
| Total | 13.8 | 16.8 | 18.5 | 21.0 | 24.5 | 28.0 | 30.0 | 33.5 | 37.0 | 40.5 |
| 15% | 13.8 | 15.9 | 18.3 | 21.0 | 24.2 | 27.8 | 31.9 | 36.7 | 42.2 | 48.5 |
| Shortfall | 0 | 0.9 | 0.2 | 0 | 0.1 | -0.2 | -1.4 | -3.7 | -5.7 | -8.5 |

The line "15%" in Table 14 shows the corresponding growth of resources, while the line "Shortfall" shows the excess of demand for funds over their "15%" supply. We see that there is a quite precise match between the programme detailed in this way and the 15% formula. The total shortfall over the first 5-year period is only M£1.4 against total resources of M£107.2 - a deficit of little more than 1%. We regard this as a sufficiently good fit within the spirit of III E (i). Putting it another way, the effective growth rate demanded over the first 5-year period is 15.4% against the 15% of the exercise.

If it were necessary to show a forecast complying rigorously with the 15% formula we should do it by delaying ISR by one year to ISR-1 finding:

Table 15

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| CERN | 2.9 | 3.5 | 4.5 | 6.1 | 8.6 | 10.4 | 11.0 | 11.8 | 12.9 | 13.8 |
| DSIR | 1.4 | 1.2 | 1.2 | 1.4 | 1.6 | 1.8 | 2.1 | 2.0 | 2.3 | 2.6 |
| NSP | 0 | 0.5 | 0.8 | 1.1 | 1.4 | 1.8 | 2.3 | 2.8 | 3.4 | 4.1 |
| NIRNS | 9.5 | 10.7 | 11.5 | 12.5 | 12.9 | 14.1 | 15.3 | 16.7 | 18.1 | 19.7 |
| Total | 13.8 | 15.9 | 18.0 | 21.1 | 24.5 | 28.1 | 30.7 | 33.3 | 36.7 | 40.2 |
| 15% | 13.8 | 15.9 | 18.3 | 21.0 | 24.2 | 27.8 | 31.9 | 36.7 | 42.2 | 48.5 |
| Shortfall | 0 | 0 | -0.3 | 0.1 | 0.3 | 0.3 | -1.2 | -3.4 | -5.5 | -8.3 |

Since we believe it to be politically most desirable for the UK to take a positive attitude at CERN over ISR, we would press strongly for the time scale of ISR rather than ISR-1 in the event of the imposition of a 15% formula, the very minor shortfalls entailed being made up from outside the formula.

(iii) Surpluses

We see that, on a growth rate of 15% continued into the second 5-year period, the programme shows progressively-larger surpluses. The amounts are roughly equal to the demands of the NIRNS additional programme as set out in Table 12, and we feel that they should be used to recoup the sacrifices made by the NIRNS laboratories in the first 5-year period. Alternatively, the NIRNS programme might be kept at its growth rate of 8.8%, as in Table 14, taking the view that the 300 GeV machine, with its much more powerful facilities should be coming into sight during the second 5-year period and that it would therefore be time to consider some levelling off of the domestic high energy programme. In this case, by holding NIRNS to an 8.8% growth rate, the overall programme offers some reduction in overall growth rate after 70/71: the totals of Table 14 show a growth rate of about 11% starting from 70/71.

The problem of running down the NIRNS laboratories would however be greatly influenced by the siting of the 300 GeV machine especially if it were built in the UK. In some circumstances it might be desirable not to run them down until they had provided a significant overlap of facilities with a fully working 300 GeV laboratory.

(iv) The Falling Growth Rate

We wish to make several comments on this situation:

(i) We believe that to hold the NIRNS programme to a growth rate of 8.8% after 70/71 is too restrictive and that a proper return on our investment in the domestic programme will not be achieved unless NIRNS is allowed the improvements and the more advanced experimental facilities represented, at this time, by her additional programme. The right time to launch the additional programme is now, but if that is denied us we should ask for it to be restored at the earliest opportunity; after 70/71 on the present forecast. In other words, the removal of the NIRNS additional programme now and the failure to restore it at the earliest opportunity, in whatever form might then be appropriate, would represent a too-rapid transfer of the centre-of-gravity of our UK overall high energy programme out of the UK and into CERN. For this reason we feel that we cannot positively envisage a decrease in growth rate after the first 5-year period.

(ii) As we have remarked in III E (ii), long-dated forward estimates tend to be too low. The surpluses evident in Table 14 may be consumed by important developments as the early 70s come nearer; but such needs are not susceptible of quantitative analysis.

(iii) CERN. The CERN estimates for ISR and the 300 GeV project have been made with care and we believe them to be realistic. Both ventures, however, are novel and contain difficulties of a kind so far not encountered: In ISR there are novel techniques such as the ultra-high vacuum requirements, although the total vacuum costs are less than 10% of the whole: and in the 300 GeV project large changes of scale. We cannot be certain there will be no escalation, but as remarked in III E (iii) it is commonly in total spend and not in rate of spend. It is therefore not so important from our present considerations. Rate of spend, however, can also escalate.

This raises an important question of principle. We are doing our best to produce a well-balanced programme over the next 10 years. There are two major elements in this balance, setting aside DSIR and Nuclear structure, namely CERN and our domestic programme. Our domestic programme is under our own control but that at CERN is not. The UK can be voted into the CERN programme through treaty provisions even if it does not agree with it. We have seen that the balance of the programmes is very tight within a 15% growth rate and that the domestic programme has been sacrificed to the CERN plans. It would be disastrous for the domestic programme if any CERN escalation in rate of spend were to be paid for out of our provisions for the domestic programme. In this situation we would urge that the UK negotiate an appropriate change in the voting rights at CERN, so that we may be protected against such an escalation of the CERN programme that our planned domestic developments in high-energy nuclear physics are thrown out of balance with out international commitments.

The above considerations show that by the damaging expedient of eliminating, at least for a time, the growth in NIRNS resources envisaged in the 'additional programme', we can devise a significant programme at a growth rate of 15%. These cuts in the programme are painful and will carry serious consequences, and we would again urge that they ought not to be made.

(v) 300 GeV Time Scale

As explained in III F (i), we have adopted for our plan a time scale for the 300 GeV project 1 year behind that of SPC/196, because we believe that our CERN partners will not be ready to meet the earlier time scale. It may be argued that our standing in CERN would be stronger were we able to go forward and press for the time scale of SPC/196. We may therefore examine the consequences of such a proposal. Moving the 300 GeV project forward to the SPC/196 time scale and leaving the rest of the programme as in Table 14 leads to shortfalls of:

Table 16

| | 66/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|
| Shortfall | 1.0 | 1.0 | 2.2 | 1.7 | 0.5 |

If this were done to our ISR-1 programme of Table 15 we should find:

Table 17

| | 66/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|
| Shortfall | 0.1 | 0.5 | 2.3 | 1.7 | 0.6 |

These shortfalls could not be represented as an allowable perturbation of the 15% formula in the spirit of III E (i) so we could not responsibly propose this restoration of the SPC/196 300 GeV schedule. Our only way to move the 300 GeV machine forward would be to remove ISR from our programme. Even then we should have a shortfall of M£0.8 in 68/9, although it would be more than balanced by surpluses in other years.

We conclude that a refusal to participate in ISR is much too high a price to pay for the doubtful psychological advantage of moving the 300 GeV project back onto the schedule of SPC/196. Although we believe that, in practice, the start of the 300 GeV project would even then be delayed and that this, with shadow cutting, would provide sufficient funds for ISR, this would be too late. We should have had to abandon ISR in order to bring forward the 300 GeV project to the SPC/196 schedule.

We also believe that if we have to opt out of ISR this may react so unfavourably in CERN that the spirit of unity and endeavour vital to CERN's success may be irremediably damaged. The other member states may then lose heart and the 300 GeV project, to which we attach the highest importance, may be lost.

## (vi) Balance of the Programme

It is important to examine the proposed distribution of resources among the various elements of the residual programme which the imposition of a 15% growth rate over a 10-year period would entail.

We have adopted the programme of Table 14 taking as our resources the 15% growth line supplemented by the shortfalls where they are positive. We have allocated negative shortfalls to NIRNS recognizing that it was the sacrifice of its additional programme which enabled the 15% growth rate to be achieved.

Since we now wish to look at the division of our resources among activities rather than among the old administrative groupings we subtract from NIRNS and DSIR those elements which, as explained in III C (i) (g), it is reasonable to attribute to nuclear structure research and add them to NSP to give the heading Nuclear Structure (Nuc. St.) as listed in Table 1. The residual NIRNS and DSIR headings, NIRNS* and DSIR* then contain only funds for high energy work.

The sums then become:

Table 18

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| CERN | 2.9 | 4.4 | 5.0 | 6.0 | 8.6 | 10.3 | 11.2 | 12.0 | 13.2 | 14.1 |
| DSIR* | 0.4 | 0.5 | 0.5 | 0.7 | 0.9 | 1.1 | 1.4 | 1.3 | 1.6 | 1.9 |
| Nuc.St. | 1.5 | 1.7 | 2.0 | 2.3 | 2.6 | 3.0 | 3.5 | 4.0 | 4.6 | 5.3 |
| NIRNS* | 9.0 | 10.2 | 11.0 | 12.0 | 12.4 | 13.8 | 16.2 | 19.2 | 23.3 | 27.7 |
| Total | 13.8 | 16.8 | 18.5 | 21.0 | 24.5 | 28.2 | 32.3 | 37.2 | 42.7 | 49.2 |

and the appropriate proportions:

Table 19

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| CERN% | 21 | 26 | 27 | 29 | 35 | 36 | 34 | 32 | 31 | 29 |
| DSIR*% | 3 | 3 | 3 | 3 | 4 | 4 | 4 | 3 | 4 | 4 |
| Nuc.St.% | 11 | 10 | 11 | 11 | 11 | 11 | 11 | 11 | 11 | 11 |
| NIRNS*% | 65 | 61 | 59 | 57 | 50 | 49 | 50 | 54 | 55 | 57 |

The constancy of the proportions going into DSIR* and nuclear structure is a continuation of a long established tendency. The only significant change is the swing of our high energy activity from the domestic programme towards CERN. The partial swing back in the later 5-year period is very questionable for the reasons given in III G (iv). Briefly, we may conclude that over the ten-year period our imposed 15% programme would call for a reduction in the proportion of our total resources going into the domestic high-energy programme from about two thirds to about a half, with a corresponding rise in our CERN investment from about a fifth to a third.

In nuclear structure work we continue to be self-sufficient and so it is appropriate that a roughly-constant proportion of our resources should go into that field, assuming that it retains its interest. In high-energy physics, however, we must look progressively more towards collaborative efforts: facilities grow in cost more rapidly than any possible acceptable growth rate. It is, therefore, appropriate that, over a long enough period, an increasing proportion of our resources should go into those collaborations.

In considering Table 19 we must remember that the proportion of our resources associated with CERN considerably exceeds those simply shown under the CERN heading above; these latter merely cover our actual payments to the CERN organisation. In addition to this the bulk of DSIR* supports work in CERN as also do significant fractions of NIRNS*: for example, all expenditure on second-generation film analysis units which will be quite largely used for CERN work is budgetted for under NIRNS*, and similarly for our largest-scale computing facilities. In cutting down on NIRNS we therefore also reduce our exploitation of CERN. The sacrifice of the NIRNS additional programme is particularly important in this respect since it contains a large proportion of the provision for capital facilities in universities.

We also see that, despite CERN's very large double construction programme, the overall expenditure called for varies rather smoothly. This is, to some degree, the result of the onset of major expenditure on the 300 GeV project in 69/70 coinciding with a momentary pause in the NIRNS basic budget. This is purely coincidental.

It therefore seems that a reduction of the growth-rate to 15% would still leave us with a reasonably-balanced overall programme. This is not to say that we believe 15% to be acceptable except, marginally, as a temporary measure. But we believe it is very unwise to run down NIRNS in relation to CERN as rapidly over the first five-year period as the 15% growth rate imposes. Any rapid movement of resources should be made only after the 300 GeV machine has come into operation. We would, therefore, urge that a growth-rate of 15%, if it is imposed at all, should be only a temporary expedient.

It is interesting similarly to examine the suggested distribution of our resources for our full "19%" programme as set out in Table 3. In this programme we have sought to optimise the development of the various branches of our study in relation to each other and so it should display a well-controlled movement of resources between the headings.

Table 20

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|---|
| CERN | 2.9 | 4.4 | 5.0 | 6.0 | 8.6 | 10.3 |
| DSIR* | 0.4 | 0.5 | 0.5 | 0.7 | 0.9 | 1.1 |
| Nuc.St. | 1.5 | 1.7 | 2.0 | 2.3 | 2.6 | 3.0 |
| NIRNS* | 9.0 | 10.6 | 11.8 | 13.7 | 16.3 | 19.0 |
| | 13.8 | 17.2 | 19.3 | 22.7 | 28.4 | 33.4 |

and the proportions:

Table 21

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|---|
| CERN% | 21 | 26 | 26 | 26 | 30 | 31 |
| DSIR*% | 3 | 3 | 3 | 3 | 3 | 3 |
| Nuc.St.% | 11 | 10 | 10 | 10 | 9 | 9 |
| NIRNS*% | 65 | 62 | 61 | 60 | 58 | 57 |

We indeed see here a smoother, slower and better-controlled movement of resources from NIRNS* towards CERN over the five-year period in our own plan than in that imposed by the 15% growth rate.

(vii) Summary

On a growth rate of 15% we can:

- (i) Support the CERN basic programme containing improvements that will keep the CERN PS on a par with the Brookhaven AGS;
- (ii) Support ISR to the time scale of SPC/196 (with an additional subvention of M£1.4 over the first five-year period);
- (iii) Support the 300 GeV project on a schedule one year slipped back from that of SPC/196;
- (iv) Support the NIRNS basic programme;
- (v) Support what we are assured will be a vigorous domestic nuclear structure programme in balance with our high energy programme.

We give up:

The NIRNS additional programme. The sacrifice will turn the Rutherford and Daresbury laboratories into second-class powers of their kind instead of keeping them in the world class of their kind. It also weakens our exploitation of CERN.

(viii) Securing the 300 GeV project

We have seen that a 15% growth-rate will allow us to go ahead with ISR. ISR, however, has considerably lower priority for us than the 300 GeV project which we consider to be critically important for European physics. We have so adjusted our own affairs as to allow us to support ISR at the expense of our domestic programme. In this limited sense we have given ISR a higher priority than the NIRNS additional programme. We should not advocate this, however, unless we were assured that ISR will not jeopardize the 300 GeV project. We are concerned lest the eagerness of some of our CERN partners to launch ISR may affect their ability to secure subsequent acceptance of the all-important 300 GeV project.

We would therefore wish that, if UK participation in the ISR is approved as we have just recommended if we get a growth-rate of 15% or more, the UK should then take steps to encourage continued interest in the 300 GeV project. This might be done by the UK reactivating and playing a full part in the deliberations of the Steering Group for the 300 GeV machine which the CERN Council set up in 1964 but which has since been dormant.

(ix) Recommendation

If we are granted a growth rate of 15% or more we recommend, subject to arrangements to secure that escalation of international high-energy experimentation would not be to the further detriment of our national programme, that CERN be immediately informed that the UK would be prepared to participate in ISR on the time-scale in SPC/196 and in the 300 GeV project on a time-scale one year behind that of SPC/196.

## III H 10%

(i) Principles

When we discussed the 15% problem we saw a rather clear-cut line of action which, though painful, was not disastrous and gave hope for later recoupment. A 10% growth rate would, however, carry much graver consequences and it is not clear to us which particular form of disaster we should choose. We here examine three possibilities and comment on each:-

(i) We approach our problem by noting that we have a prior commitment to DSIR and to the CERN basic programme: see III C (i), (a) and (b).

Table 22

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| CERN Basic | 2.7 | 3.1 | 3.2 | 3.6 | 4.0 | 4.4 | 4.8 | 5.3 | 5.8 | 6.4 |
| DSIR | 1.4 | 1.2 | 1.2 | 1.4 | 1.6 | 1.8 | 2.1 | 2.0 | 2.3 | 2.6 |
| Total | 4.1 | 4.3 | 4.4 | 5.0 | 5.6 | 6.2 | 6.9 | 7.3 | 8.1 | 9.0 |

(ii) We attach the highest importance to the 300 GeV project and we do not want to see it delayed; but with very restricted resources, we should have to consider doing so. We take the costs of a delayed programme to be M£0.1 per year until the project gets going followed by:

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| M£ | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 | 5.4 | 5.4 |

(iii) We must sustain some sort of domestic programme in high energy physics. We do not believe that we can work effectively entirely at second hand at this stage of the subject with a substantial fraction of all university high-energy physics staff always in semi-permanent residence at the remote accelerator.

(ii) Options

We first determine how much is available after the prior commitment to CERN and DSIR:

Table 23

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| 10% | 13.8 | 15.2 | 16.7 | 18.4 | 20.2 | 22.2 | 24.4 | 26.9 | 29.6 | 32.6 |
| Prior | 4.3 | 4.3 | 4.4 | 5.0 | 5.6 | 6.2 | 6.9 | 7.3 | 8.1 | 9.0 |
| Residue I | 9.5 | 10.9 | 12.3 | 13.4 | 14.6 | 16.0 | 17.5 | 19.6 | 21.5 | 23.6 |

(We have increased the prior commitment by M£0.2 in 65/6; that sum is already additionally committed to pilot work on ISR and the 300 GeV project)

We now ask for what is left if we support the NIRNS basic programme:-

Table 24

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Residue I | 9.5 | 10.9 | 12.3 | 13.4 | 14.6 | 16.0 | 17.5 | 19.6 | 21.5 | 23.6 |
| NIRNS Basic | 9.5 | 10.7 | 11.5 | 12.5 | 12.9 | 14.1 | 15.3 | 16.7 | 18.1 | 19.7 |
| Residue II | 0 | 0.2 | 0.8 | 0.9 | 1.7 | 1.9 | 2.2 | 2.9 | 3.4 | 3.9 |

We can now support the 300 GeV project only by a considerable displacement:

Table 25

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Residue II | 0 | 0.2 | 0.8 | 0.9 | 1.7 | 1.9 | 2.2 | 2.9 | 3.4 | 3.9 |
| 300-4 | 0 | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 |
| Residue III | 0 | 0.1 | 0.7 | 0.8 | 1.6 | 1.8 | 2.0 | 1.9 | 0.2 | -0.7 |

(The M£0.1 for 65/6 is already taken account of in Residue I)

The considerable residues in the early years could be used for supporting a much attenuated NIRNS additional programme or for launching some nuclear structure project if special help could be provided through the difficult years at the end of the period. In either event we must note that we have now impinged upon the nuclear structure programme hitherto preserved.

Option 10% A : DSIR support
CERN basic programme
NIRNS basic programme
300 GeV programme delayed by
5 years beyond SPC/196

Plus sub-options : Some NIRNS additional programme, or
Some nuclear structure programme

We have lost ISR which we very much regret as being the only fully-novel technical venture in our programme. We have also had to recommend a complete, or perhaps partial, sacrifice of the nuclear structure programme. We have no authority to do so, and we regret it since we consider that a reasonable balance should be kept between nuclear structure and high-energy work. Perhaps worst of all, the 300 GeV project has had to be delayed by 4 more years so that it is now 5 years behind the schedule of SPC/196. So long a delay may begin to cast doubt on the viability of the project, particularly in view of the probable early start on the US 200 GeV machine and authorisation of the US 800 GeV project which is suggested for Fiscal Year 1971 in the 1965 US White Paper; i.e. about the same time as the effective start on the 300 GeV machine if it were delayed 4 more years as in this Option. So long a prospective delay coupled with the abandonment of ISR would be likely to cause complete demoralization in CERN. It could lead to the eventual abandonment of the 300 GeV project and bring to an end significant high-energy research in Europe for our generation.

We feel that Option 10% A could never be adopted as a serious policy.

We feel that the 300 GeV machine must be moved forward by some means or other. So far we have resisted breaking our forecasting formulae, except trivially, and we clearly must continue to do so, in the spirit of III E (i). We feel that we might reasonably ask, however, to be allowed to break the formula in some years if the books balanced over the 10-year period. If this were allowed we could bring the 300 GeV project forward to 300-2:

Table 26

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Residue II | 0 | 0.2 | 0.8 | 0.9 | 1.7 | 1.9 | 2.2 | 2.9 | 3.4 | 3.9 |
| 300-2 | 0 | 0.1 | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 |
| Residue III | 0 | 0.1 | 0.7 | 0.8 | 1.5 | 0.9 | -1.0 | -1.7 | -1.5 | -1.1 |

This shows a total shortfall over the period of M£1.3

Option 10% B : DSIR support
CERN basic programme
NIRNS basic programme
300 GeV project delayed by
3 years beyond SPC/196

Even this delay on the 300 GeV project is extremely unpalatable, and would lose Europe all the advantage gained over the US in the quick preparations made for the high energy project. ISR remains cancelled, of course, and the NIRNS programme is denied the chance of making up the ground lost by being permanently reduced to the basic programme. NIRNS would slowly wither during the 10-year period but with a rapid and damaging loss of morale at the beginning because it would be known that there was no plan for proper support later. This would lose men. We have also had again to appropriate all new resources for the nuclear structure programme.

If we have to provide for some new nuclear structure project, and the pressure for this will be heavy, we can only do it on the shorter 300 GeV time-scale of 300-2 by cutting down the NIRNS programme below its basic level. Consider a 7% growth rate for NIRNS rather than the 8.8% represented by the basic programme:

Table 27

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Residue I | 9.5 | 10.9 | 12.3 | 13.4 | 14.6 | 16.0 | 17.5 | 19.6 | 21.5 | 23.6 |
| NIRNS 7% | 9.5 | 10.2 | 10.9 | 11.6 | 12.5 | 13.3 | 14.3 | 15.3 | 16.4 | 17.5 |
| Residue II | 0 | 0.7 | 1.4 | 1.8 | 2.1 | 2.7 | 3.2 | 4.3 | 5.1 | 6.1 |
| 30-2 | 0 | 0.1 | 0.1 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 |
| Residue III | 0 | 0.6 | 1.3 | 1.7 | 1.9 | 1.7 | 0 | -0.3 | 0.2 | 1.1 |

These residual sums suffice for a significant new nuclear structure programme, if some redistribution over the years were allowed, although it only amounts to 60% of NSP; see III C (i) (g).

Option 10% C : DSIR support
CERN basic programme
NIRNS growth rate of 7%
300 GeV project delayed by
3 years beyond SPC/196.
Some nuclear structure programme

We have paid for this Option by reducing NIRNS to so low a rate of growth that the continued viability of the two laboratories must come into question. A 7% growth rate would, in our opinion, move us dangerously close to making fools of ourselves in the eyes of the world of physics. Both NIRNS laboratories could be kept going but in a manner that would entail the rapid and continuous slippage of their position relative to those of other world-class laboratories. This would be a painful and dismal process of strangulation and one for which none of us would wish to be responsible.

All these Options lose ISR and so we must repeat the warning of III C (ii) that this will entail pressure for an increase in the CERN basic programme and may possibly lose us the 300 GeV machine too.

## (iii) Summary

There are, of course, other options of a more drastic kind involving shutting down a NIRNS Laboratory but we have not considered them seriously since they would be completely unjustified scientifically and would represent a dramatic failure of UK physics.

The clear consequences of a 10% growth rate are :

(i) ISR should not be supported. This is very bad as it loses us the chance to make a unique contribution. It could also lead to the demise of the 300 GeV project and so to the end of high energy physics in Europe for our generation.

(ii) NIRNS is reduced to its basic programme with little or no hope, depending on the option, of recovery. We believe that this will have a major stultifying effect on UK high-energy physics. NIRNS may have to go below its basic programme in which case its continued survival as a 2-laboratory system becomes questionable;

(iii) The 300 GeV project must be delayed at least a further 2 years i.e. to a time scale 3 years beyond that of SPC/196. We regard this as highly calamitous and it could lead to the abandonment of the project;

(iv) Nuclear structure developments are stopped or are severely attenuated.

The 10% growth rate, although it would allow us to go ahead with the

300 GeV project, should that still be possible after our abandoning ISR, imposes so serious a delay that the project may be thought questionable in view of the rapid start on the analogous US project. We should still consider it critically necessary although we should regard the delay as deplorable. But the 10% rate also weakens NIRNS, undermines the structure of UK high-energy physics, and so diminishes the use that we can make of CERN. On a 10% growth-rate UK high-energy physics would go rapidly downhill.

## III I 5%

At a growth rate of 5% not a single activity can be properly supported in the long-term, if the rate were to continue indefinitely. In this sense we cannot take it seriously as a long-term proposal, but we shall examine the consequences during our 10-year period on the assumption that the rate must pick up later. This is important in discussing survival.

A growth rate of 5% presents us with:

Table 28

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| 5% | 13.8 | 14.5 | 15.2 | 16.0 | 16.8 | 17.6 | 18.5 | 19.4 | 20.4 | 21.4 |
| Prior | 4.3 | 4.3 | 4.4 | 5.0 | 5.6 | 6.2 | 6.9 | 7.3 | 8.1 | 9.0 |
| Residue I | 9.5 | 10.2 | 10.8 | 11.0 | 11.2 | 11.4 | 11.6 | 12.1 | 12.3 | 12.4 |

We cannot now begin by writing down a NIRNS programme and seeing how the 300 GeV project could fit into it; if we wish to keep the 300 GeV project, it must be the other way round. As soon as this is admitted there is no longer any reason for not starting the 300 GeV project to its realistic time scale of one year behind SPC/196:

Table 29

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Residue I | 9.5 | 10.2 | 10.8 | 11.0 | 11.2 | 11.4 | 11.6 | 12.1 | 12.3 | 12.4 |
| 300 GeV | 0 | 0.1 | 0.2 | 1.0 | 3.2 | 4.6 | 4.9 | 5.0 | 5.4 | 5.4 |
| Residue II | 9.5 | 10.1 | 10.6 | 10.0 | 8.0 | 6.8 | 6.7 | 7.1 | 6.9 | 7.0 |

We believe that it would be quite wrong to attempt to use Residue II to maintain a "thread of life" in the two NIRNS laboratories: they would both rapidly become moribund. One would have to go. We also believe that a grossly under-supported laboratory is worse than nothing and since the Rutherford Laboratory's basic programme runs at over M£9 67/8 through 70/71 it would have to be the one to be sacrificed.

The present Daresbury 5-year forecast including its additional programme is:

Table 30

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 |
|---|---|---|---|---|---|---|
| Daresbury | 3.0 | 3.3 | 2.9 | 3.4 | 4.3 | 4.8 |

Since Daresbury would now represent our only high-energy laboratory we should want to support is as strongly as possible which we should presumably do through a healthy growth-rate of, say, 12% after its additional programme of the next 5 years. This gives:

Table 31

| | 65/6 | 6/7 | 7/8 | 8/9 | 69/70 | 70/71 | 1/2 | 2/3 | 3/4 | 4/5 |
|---|---|---|---|---|---|---|---|---|---|---|
| Residue II | 9.5 | 10.1 | 10.6 | 10.0 | 8.0 | 6.8 | 6.7 | 7.1 | 6.9 | 7.0 |
| Daresbury | 3.0 | 3.3 | 2.9 | 3.4 | 4.3 | 4.8 | 5.4 | 6.0 | 6.7 | 7.6 |
| Residue III | 6.5 | 6.8 | 7.7 | 6.6 | 3.7 | 2.0 | 1.3 | 1.1 | 0.2 | -0.6 |

Out of Residue III we should phase out the Rutherford Laboratory and meet the pressure for an increase in the CERN basic programme consequent upon the disappearance of ISR, see III C (ii), but we see that no new project, nuclear structure included, is possible. At the end of the period we see that the Rutherford Laboratory has been completely consumed, and the other elements of the programme have overtaken the 5%. Unless there were an increase in growth-rate beyond this period, another element would have to be phased out.

We must not forget that a considerable fraction of the support for our CERN work is in the NIRNS figures: it is in the Rutherford Laboratory element. The university film analysis units and the large computer would have to come out of the Daresbury figures which would not then look nearly so healthy as they seem at first sight.

We have not found any more-acceptable solution by delaying the 300 GeV machine: this merely puts off the closing down of the Rutherford Laboratory; we should pay for this by jeopardizing the 300 GeV project by the delay as we have already discussed.

If we go ahead with the 300 GeV project, there is no option and the Rutherford Laboratory goes. If, instead, we drop the 300 GeV machine we return to our remarks of III C (i) (c): the period under review would be one of full exploitation of existing machines but after a little time we should be overwhelmed by the US 200 GeV machine and European high energy physics would die for our generation.

The choice between these two courses of action, each with a hopeless outcome for UK high energy physics as the inevitable consequence, would be a thankless task. We do not believe it can be responsibly contemplated by anyone concerned for the continuation of high-energy physics in the UK.

## III J Education

We should like to return to a point that we stressed in I D: that, to restrict research is to restrict teaching. Unless research is properly fostered, the feedback of high-class teachers into the Universities is inhibited. The better men will, if proper facilities are not provided here, go to, and remain in, the US. Their loss is not just to UK research, it is also to UK teaching. Poorer quality graduates will then emerge from the universities and the vicious circle will have been joined. We attach great importance to graduate training and under-graduate teaching, and we regard our plea for proper support for our research activities as also a plea for better teaching.

We believe that the more the full programme is restricted from that which we regard as desirable, the larger will be the damage to the quantity and, more important, to the quality of that education in physics departments which we provide; and the damage will be more than proportional to the cuts.

## III K Conclusion

At the conclusion of our work, which has forced us to examine closely the present allocation of our resources as well as considering plans for the future, we are of the opinion that hitherto the development of high-energy nuclear physics in the UK has been in satisfactory accord with the development of the subject and the basic needs of the country in science and technology. This has been achieved without any serious perturbation of balance between different disciplines. We have not entered into, or proposed, any commitments that would either distort the general pattern of the expansion of civil science, or provide unnecessarily lavish facilities, or demand a drawing-off of manpower from other fields; or diminish the supply of trained men from nuclear physics to the general community. Our proposed new commitments are contained within a continuation of the overall pattern of expansion of recent years. But, and this is a most important point, the time scales of our operations are, by their very nature, long and to impose a sudden downward tilt on to the pattern of support at an arbitrary moment is bound, as we have shown, to lead to disaster if the programme was properly conceived and balanced in the first instant. It is also important to stress that operations form a whole and should not be looked upon as a number of isolated projects. To carry through a number of projects and then not take the next steps for their exploitation is like a flight of stairs which goes only half way up to the next floor.

We would ~~not~~ most earnestly urge that our present programme, which calls for a rate of expansion of 19% over the next 5 years should be adopted and that ample warning should be given if that growth rate is subsequently to be cut. We recognise that the rate of expansion of civil science of the last few years has been at the rate of 15% only and we could, with regret, conform to that rate if it were imposed. But we do not believe that such a rate is adequate for our needs, given the momentum of the discipline in Europe and on the world scale; we need 19% and we regard a growth rate of 15% as the minimum which could be responsibly imposed; and that for a limited period only.

Our principle conclusions relating to other growth-rates are given in the summary prefacing this part of our report.