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THE CERN LONG-TERM PROGRAMME

(by Directorate Member for Applied Physics)

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At the SPC meetings in April and July papers were presented (CERN/SPC/125 and 131) setting out the conditions in which Europe and CERN would probably find themselves during the next decade, pointing out that new major facilities for high-energy physics should undoubtedly be built in this period, and trying to select the more promising projects for further study. In addition, the Accelerator Research Division put forward in detail a proposal for one such facility, viz. a set of storage rings to be fed by the CERN PS, and a theoretical conference on very high energy phenomena was held in CERN with one of its aims to see if our present theoretical understanding gave any clear lead in the choice between different possible machines.

No detailed technical evaluation of large accelerators was provided in the SPC papers, as several groups in the USA were working hard on just this during the summer, and there was to be an International Accelerator Conference in Brockhaven in September where these matters were the main subject, and also where information was to be exchanged with the Russians on the possible International Accelerator project. A senior physicist from CERN, Dr. K. Johnsen, has worked with the USA groups during the summer, and a large contingent from CERN attended the Brookhaven conference. Section I of this paper describes the situation of the international machine, while sections II and III contain a report on the activity in the USA, and our suggestions as to how it affects the European situation.

I. PROGRESS TOWARDS AN INTERNATIONAL ACCELERATOR PROJECT

Also at the July SPC meeting the Director-General mentioned that there was support both in the USA and the USSR for an international accelerator laboratory, equipped with a machine of 500-1000 GeV energy, and that further discussions on this between USA and USSR technical groups would take place at the time of the Brookhaven conference, and also at the meeting of the IUPAP High-Energy Commission. In August CERN was invited by Professor Seaborg, head of the US Atomic Energy Commission, to nominate European participants for the former meeting, which we did after consultation with Mr. Willems and other members of Council. In fact, since the Russians did not appear at either the technical or the IUPAP meeting, no further steps towards an international machine could then be taken. Since then, at the recent meeting of the IAEA in Vienna, Dr. Haworth and Professor Rabi talked to Professor Emelyanov of the USSR Atomic Energy Commission, and have told the Director-General what was said there. Professor Emelyanov apparently confirmed that the USSR was still interested in the possibility of an international accelerator and that, although the USSR wished to open the discussions only with the USA, under the terms of the 1959 McCone-Emelyanov agreement, the project should be broadened to include other countries at some suitable moment. Professor Emelyanov suggested that a technical meeting between the USA and USSR official study groups be held soon (probably before Christmas), to compare technical proposals for machines with energies in the 500-1000 GeV region, and to try and start more detailed work leading perhaps to the establishment of the nucleus of a future international laboratory.

This situation is consistent with the results of the recent Pugwash conference at Stowe, Vermont, where several very senior Russian physicists were present, and where it was concluded that "the field of high-energy physics is an excellent one for co-operation between all countries of the world. This co-operation would centre round the establishment of a laboratory whose main research tool would be an accelerator of not less than 300 GeV or higher, and of a design which would achieve success in the shortest possible time".

The Pugwash subcommittee on high-energy physics went further in concluding tentatively that this laboratory should be in Europe.

Dr. Haworth indicated that the policy of the USA remained unchanged: they were anxious that Western Europe should join actively in the building of such a laboratory, and that it would therefore be highly desirable that a concerted policy should be formed by the countries concerned on how they would react to an invitation to participate, and that a good channel of communication be set up for this purpose.

II. AMERICAN CONCLUSIONS ON VERY LARGE PROTON SYNCHROTRONS

a) During this summer two accelerator studies took place in the USA. At Berkeley the possibility of an accelerator in the range 100-300 GeV was studied with the main emphasis on 300 GeV, and at Brookhaven the range 300-1000 GeV was considered with the main emphasis on 1000 GeV. CERN had people taking part in both studies. In addition to this there was the International Conference on High-Energy Accelerators in New York and Brookhaven in September. This conference produced very little that was new and not already covered by the American studies, in part because the Russians were not present.

The conclusions from these studies can be summed up as follows. There is no new, revolutionary, principle or technical invention for immediate application to high-energy accelerators. Any accelerator above 25 GeV to be finished in the next decade or so must be based on the alternating gradient principle, taking full advantage of the experience gained on the CERN PS and the Brookhaven AGS.

The following short list of possible parameters gives an indication of what accelerators in the range 100-1000 GeV would look like:

Maximum energy	100 GeV	300 GeV	1000 GeV
Average machine radius	400 m	1250 m	2800 m
Acceleration time	1 s	1 s	1 s
Period	2-3 s	2-3 s	2-3 s
Energy gain/turn	0.85 MeV/ turn	7.7 MeV/ turn	57 MeV/ turn
Injection energy	600 MeV	3 GeV	6 GeV
Frequency swing	20 %	3 %	1 %
Injection linac length	250 m	500 m	1100 m
No. of individual magnets	440	1100	1440
Aperture	7 x 14 cm^2	3.5 x 7 cm^2	4 x 8 cm^2
Total steel weight	14 000 ton	11 000 ton	31 000 ton
Cost estimate	400 M Fr.	900 M Fr.	3000 M Fr.

As the figures for magnet size and weight show, the three machines reflect different design philosophies, the higher energy machines showing a more optimistic view on the tolerances which can be held in the position and uniformity of the magnet units. The figures used here for the 1000 GeV machine involve accuracies as good as, or better than, those achieved in practice with the AGS and CERN PS, over distances and quantities 40 times as great. It is right for the designer nowadays to ignore some of the less rational fears of the designers of the first big alternating gradient machines, but it may not be sound to make no allowance at all for the difficulty of maintaining laboratory quality standards over a large industrial production.

Quite apart from this question of safety factors, it may seem surprising that the necessary magnet aperture seems to be roughly independent of energy. This economical result can only be achieved by an increase in focusing strength, which entails tighter tolerances on the constancy of magnetic field gradient, and this also may be expensive to achieve.

The cost estimates are given with all reservations. They are, for instance, made under the assumption that the intensities of the machines are low enough not to require any special precautions and handling equipment to deal with induced activity. This will certainly not be the case, as there is much demand for intensity from the nuclear physicists, and one can see ways of getting high intensities if one is prepared to make the effort involved in solving the accompanying problems. It is believed that intensities in the range 10^{13} - 10^{14} p/s can be obtained, but that special precautions due to radio-activity etc. are needed well below this figure. The figures given probably do allow to some extent for the expenses involved in the adoption of optimistic attitudes on tolerances.

The cost estimates have been criticized for being low also for other reasons. A machine in this energy range, perhaps with the exception of the very lowest part of the range, means setting up a completely new laboratory, and the cost of this will probably be charged against the machine by those finding the money, and the same applies to much of the experimental equipment. These extra expenses usually are about equal to the machine cost. This attitude, however, means breaking somewhat with current practice in estimating machine cost, and the cost estimate presented is done in the "old-fashioned" way, and it is believed to be a fair estimate on that basis.

The corresponding man-power figures have not been worked out as carefully as the costs. Most recent estimates for a large machine were in the region of 1000-1500 staff, over about 10 years, exclusive of general laboratory services and work on experimental equipment.

b) This design work on new accelerators was accompanied, especially at Berkeley, by discussions by theoreticians and experimenters on the possible future needs for high-energy or high-intensity accelerators, and on the ways of carrying out experiments in this energy region.

The theoretical work covered much the same ground as was discussed in the Theoretical Conference at CERN in June, and did not seem to arrive at any different conclusions. Not many reasons could be given for high energies as such; asymptotic variation of cross-sections, problems of neutrino events in weak interactions are the obvious ones. A much stronger practical reason for a high-energy accelerator to do physics of the kind which can now be discussed seriously by theoreticians is the great gain in intensity of secondary beams in the 5-50 GeV region which a high-energy machine provides.

A more general view was expressed strongly by Professor Serber at the Brookhaven conference; he said that the best case for an increase in machine energy was that this step had always paid off handsomely in the past, and warned that asking theoreticians to justify it in detail might do more harm than good.

The main effort in studying practical ways of doing experiments was made in Brookhaven by a group led by Dr. L. Yuan. They considered both the production and separation of beams at high energies, and also possible detectors for use in these beams. This kind of work suffers from an unavoidable defect, in that if done in terms of known apparatus or methods, one can, perhaps, believe the results but at the same time be fairly sure that no beam or detector in 10-12 years' time will look remotely like what is proposed. If only the fundamental properties of beams and interactions are assumed, the proposed layouts are purely hypothetical, and depend on advances in technique and technology which may possibly not materialize. The Brookhaven descriptions fall very definitely into the first class, and therefore at best show what could be done if no appreciable further advance is made in experimental apparatus over the next decade.

For example, for the 1000 GeV machine, a two-stage separated beam is described, with the same optics as is used to-day, but with some 300 m of electrostatic separator in each stage and an overall length of about 1 km. With this, the flux of antiprotons and K-mesons is estimated to be useful for bubble chambers (assuming they still exist in 1975) up to energies of 50 GeV, 33 GeV respectively, but that twice these energies would be the limit.

For a 300 GeV machine, a 50 GeV pion beam was designed with a flux of 0.03 pions per proton hitting the target, in an energy range of $\pm$ 10%. Used to make neutrinos, with a flight path of 0.5-1 km, and with 10^{12} protons/sec, the pion flux is 3.10^{10}/sec and the total neutrino interaction rate is about 1/ton-hour.

Since the production cone for high-energy secondaries is so sharply peaked forwards (most of the 100 GeV pions lie inside a cone of 3 mrad half-angle), if a zero degree emitted beam can be extracted from the machine, a very high collection efficiency can be achieved, in contrast to figures like 10^{-4} for a good beam with the present 25 GeV machines.

The gas Čerenkov counter appears capable of discriminating between protons and pions of up to 200 GeV, though it would be about 30 m long to give the same total light output as now used. One suggestion which shows promise also of discriminating between particles is a multiple xenon scintillation counter, using the relativistic rise of ionization as the distinguishing parameter. The length and upper energy limit would be similar to those of the Čerenkov counter. Other proposals using secondary emission and synchrotron radiation were considered but found either difficult or hopeless.

A 3 m hydrogen bubble chamber with a magnetic field of 100 kG was considered feasible and very useful, despite the low event rate and the necessity of feeding it with a separated beam. Tracks of up to 500 GeV energy could be measured with good accuracy in such a chamber.

c) One other important step was also taken in the USA at about this time, when Congressional approval was finally given to the appropriation of $ 114 M for the Stanford 2 mile electron linac. According to the programme for construction which has been prepared by the planning group for this project (already 160 strong, before the Congressional approval was obtained), the linac should be built by the end of 1966.

d) The impression given by the great majority of American physicists, whether machine builders or nuclear physicists, is one of confidence that the large proton synchrotron is not only a practical engineering proposition, but has also the good points of the present CERN and Brookhaven machines greatly magnified by the increased energy and intensity. There was no feeling that such a machine had to be justified by asking what particular important problem in physics it would solve; it was considered to be a general purpose accelerator of vast capabilities, and justifiable as such. Although the groups on the West Coast proposing a 300 GeV machine thought of it as an addition to their local facilities, and the Brookhaven group were given the study of even larger machines for international construction as their terms of reference, all parties seemed to expect a machine of about 300 GeV to be built somewhere in the USA in the foreseeable future, whether or not an international machine was also built.

The rather negative reactions from official quarters to any talk of such a project were felt to be only temporary and due to the difficulty of getting the Stanford linac approved; potential builders apparently imagined that two or three years' hard pressure and propaganda in Washington would eventually produce the money, though probably only for a national laboratory, and not to satisfy any particular local interest. The most pessimistic delay for starting the new machine seemed to be about five years from now.

III. RELEVANCE OF THE AMERICAN STUDIES TO POSSIBLE EUROPEAN MACHINES

At the technical level, the American work suggested some improvements in technique which are applicable to any new alternating gradient synchrotron:

a) A rather simple way of making appreciably longer straight sections than now used which can be very helpful for the injection and target regions, especially when coupled with the use of kicker magnets to get zero degree charged beams out of the machine.

b) The magnet alignment problem has been shown to be not so serious in a large machine as had been imagined from rather superficial considerations.

c) For accelerators where injection is at nearly relativistic energies, a new method has been proposed by which the frequency swing of the acceleration system can be reduced almost to zero, so that less radio-frequency power is needed.

A more important result of recent work is a better understanding of what are likely to be the <u>limitations on the accelerated current</u>, and secondary beam intensities, of alternating gradient machines. This has important implications for the choice of energy in the next machine to be built for European use. We have not yet had time to draw conclusions here, but we shall be looking at this in CERN as one of our immediate tasks. In general, it is clear that machines designed for a high energy, or rather with a large radius, are intrinsically high-intensity machines, both because of the greater circumference to fill with protons and, more importantly, because one is forced to a higher injection energy. Further, for many secondary beams, the available fluxes will be increased considerably by going to higher proton energy, because of the shape of the production spectrum of secondaries from a target.

Rather little progress has been made, on the other hand, in the study of <u>induced radio-activity</u> and its implications on the design and especially on the cost of new machines. By itself the direct shielding of even a 1000 GeV accelerator does not seem to be a difficult problem, except that the muons produced by decay of pions and kaons near the target are now very energetic, and may need up to 150 m of steel to stop them, though fortunately only along a narrow path nearly tangential to the target. The figures for health hazard near the machine and targets due to induced activity, however, are frightening, especially with the high currents which have been shown possible, but so far no detailed thinking has been done on how this will change the layout and mechanical design of the machine. One rather obvious rule will be to keep as many components as possible away from the magnet, possibly, as in the Stanford linac, in a separate tunnel. This sort of change from present practice may not be inexpensive. It is clear, though, that these problems must be faced and solved, because the reasons for building any accelerator larger than the present machines will entail also a large increase (x 100-1000) in beam power, and this is also the significant parameter in creating the radio-activity problem.

The American work may effect our thinking on the <u>site requirements</u> for a new accelerator in two ways:

i) We can have more confidence that the necessary ground stability is similar to that already obtained at CERN and Brookhaven in practice, though this will be needed over much larger areas than used now.

ii) To obtain a good utilization of an accelerator, especially one with high intensity, it will perhaps be necessary to have several experimental areas, which can be used for experiments or for setting up, independently of whether the machine is running or not. The "beam siding" described in the Brookhaven study report, whereby a complete target area can be by-passed by the accelerated beam, is a way of achieving this. In addition, the beam lengths discussed are considerable, and the experimental areas should therefore be well separated.

The subject of large storage rings and intersecting beam experiments was only covered incidentally in the American work, but even so there was positive interest on the part of some experimentalists there in the virtues of, for example, the CERN proposal. It was suggested that the very negative attitude of experimenters inside CERN was based on only a superficial study of the possibilities of using storage rings, and that with even small extrapolations in present-day detector technique they looked quite fruitful. The criticism of colliding beam experiments on grounds of low intensity was answered by claiming that with rings reactions could be detected which had cross-sections down to 10^{-8} or 10^{-9} times the basic p-p total cross-section, and that Nature could reasonably be expected to provide plenty of interest in such a range in the detailed study of strong interactions. The cost estimates for large accelerators confirm the view in an earlier paper that the smallest worth-while medium or high-energy accelerator will cost at least twice as much as a pair of storage rings for the CERN PS.

In conclusion to this review of the effects of recent American work on future plans for CERN and Europe, one can say that the analysis we have presented in previous papers to the Scientific Policy Committee on the world situation remains valid. With corrections from our most recent information, and in some cases with reasonable conjecture, the time-scale and beam intensities for the major USA and European machines is shown in the attached table. The superiority of American facilities, which can perhaps be borne at the moment, will become overwhelming by 1970 unless action is taken fairly soon in Europe.

IV. WORK FOR CERN IN 1962

Long-term plans for CERN are, as has been stated before, intimately linked to the general problems of new European facilities, and of the proposed Intercontinental Accelerator. It is clear that we are some way off reaching agreement on what should be built next to satisfy the needs of Europe, and the intercontinental project is even further away from any decision. In the meanwhile, we can work technically in CERN on two broad lines of long-term interest:

i) Preparation for participation in the intercontinental project; this will be equally useful if a purely European large machine is finally decided upon.

ii) Studies relevant to our strictly local conditions in CERN.

We have already picked out some topics which CERN could usefully work on next year, but there has not been enough time yet to make definite choices. With these reservations, we can list:

ad i)
- a) Tolerances and non-linear effects in large synchrotrons.
- b) Radio-frequency systems.
- c) Instrumentation and measurements.
- d) Radio-active contamination.
- e) Beams and experimental apparatus.
- f) Building and site requirements.
- g) Cost, man-power and organization.

ad ii)
- a) Continuing assessment of storage rings, especially of the possibilities of doing experiments.
- b) Studies of possible extensions to the CERN site, relevant to a possible new project for CERN.
- c) Analysis of the advantages and problems of higher energy or intensity in the 50-300 GeV region, considering both accelerators and secondary beams.
- d) Continuation of the work on the possibility of a conversion or replacement of the CERN SC to provide much higher meson fluxes.

Some of these items require the collaboration of experimentalists, which may be difficult to arrange in view of the operation of the CERN PS. We have the advantage of having with us several visitors who are specially well qualified in these matters, and we would consider inviting more if the necessary effort cannot be found already in CERN.

In particular, we hope to continue actively the work on storage rings, which still offers a number of unique possibilities for research at comparatively modest cost, and with a time-scale short enough not to be interfered with seriously by an international project which was not pushed ahead at an unusual rate for definitely non-scientific aims.

Besides this work aimed at establishing a new long-term programme for CERN, there is also the very important problem of improving the PS and the SC to take full advantage of their unused possibilities. The need for this is obvious, and has always been regarded as falling formally inside the existing programme of CERN for questions of budget and resources.

It is, however, likely to compete for staff to some degree with the longer term projects, and since it is vital for the success of CERN in the next few years that physics is done here as well as it can be , we shall make sure that help is made available from the Accelerator Research Division when necessary for improving the intensity and facilities of the two machines.

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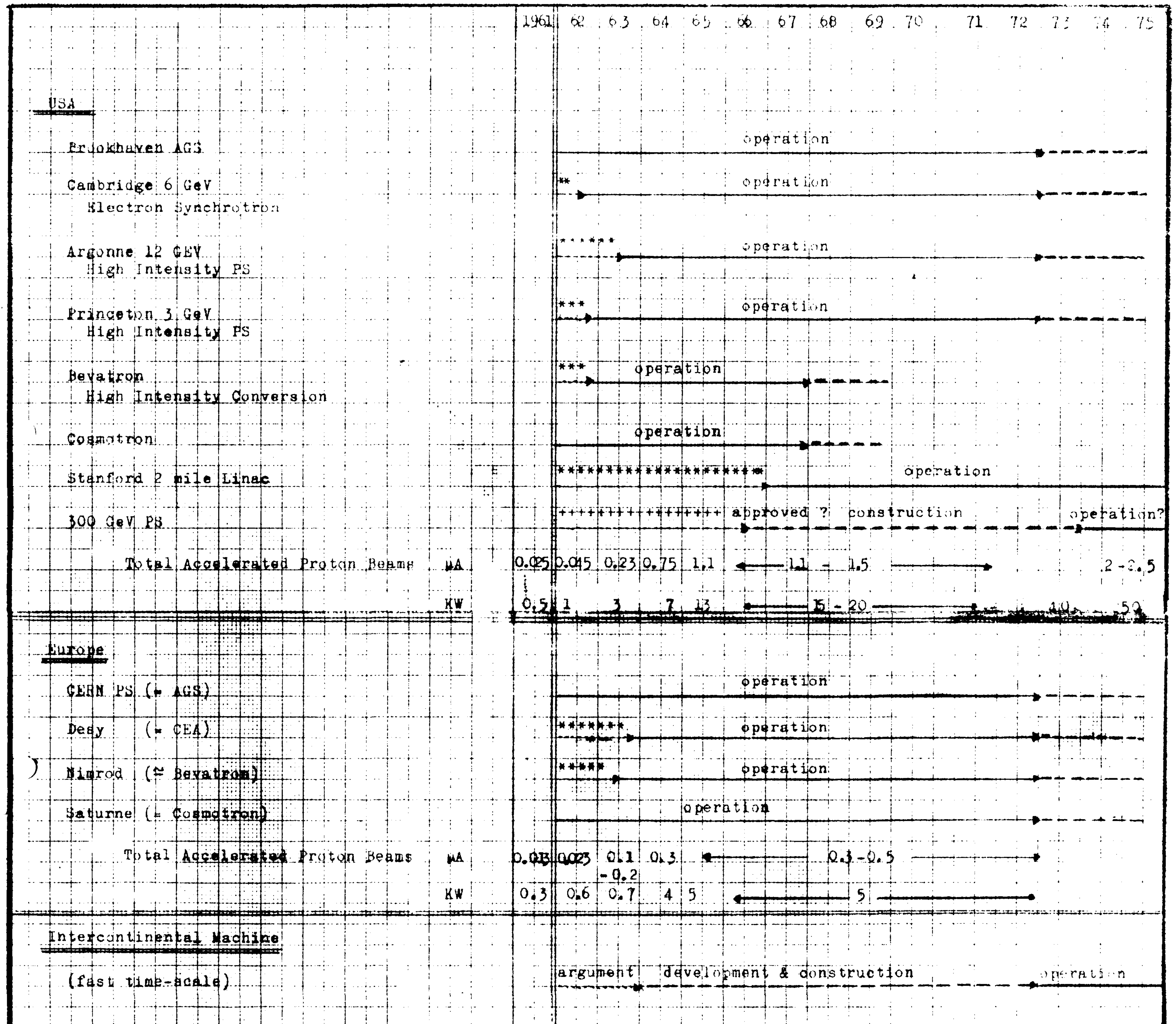

** = construction

++ = argument & development