CERN: Past performance and future prospects

III. CERN and the future of world high-energy physics

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In a series of three papers, we attempt to evaluate the past scientific performance of the three main particle accelerators at the Geneva-based European Organization for Nuclear Research (CERN) over the period since 1960, and to assess the future prospects for CERN and its users during the next ten to fifteen years.

We concerned ourselves in the first paper (Paper I – Martin and Irvine [29]) with the position of the CERN accelerators in world high-energy physics relative to those at other large laboratories working in the field. We dealt primarily with the period from 1969 to 1978, and attempted to establish how the experimental output from the three principal CERN accelerators, *taken as a whole*, compared with that from other major facilities. In undertaking this comparative evaluation, we drew on the method of "converging partial indicators" used in previous studies of three Big Science specialties.

In contrast, the second paper (Paper II – Irvine and Martin [24]) focused in detail on the scientific performance of each of the CERN accelerators *taken individually*. In particular, it asked, first, how the outputs from the CERN 28 GeV (giga or billion electron-volts) Proton Synchrotron compare with those from a very similar 33 GeV American accelerator at Brookhaven National Laboratory over the past two decades. Second, how great have been the experimental achievements of the Intersecting Storage Rings in world terms? And, third, how do the outputs from the CERN 400 GeV Super Proton Synchrotron and from a rival US machine at Fermi National Accelerator Laboratory compare? Attempts were then made to identify the main factors responsible for determining the relative scientific performance of each CERN machine.

These factors are of relevance to the subject of this third paper (Paper III), which sets out to assess the future prospects for CERN and in particular for LEP, the large electron–positron collider scheduled for completion in the latter part of 1988. What are the construction requirements (financial and technical) associated with LEP, and how easily will they be met? How does the scientific potential of LEP compare with that of other major accelerators under construction or planned around the world? In the light of the previous record of the CERN accelerators, to what extent is this scientific potential likely to be realized? What spin-off is there likely to be from LEP to accelerator physics in general? Finally, how "flexible" is LEP – in other words, what is its potential for future development? The paper concludes with a discussion of the extent to which predictive techniques can be utilized in the formulation of scientific priorities, and of the problems in current science policy-making that such techniques might help address.

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1. Introduction: The need for predictive science-policy tools

The overriding goal of the various studies of basic science specialties that we have undertaken in recent years [1] has been to develop explicit and systematic methods for evaluating the past scientific performance of major research facilities and their associated user-groups. We have argued that such studies have several important policy applications, particularly in helping to determine when existing experimental facilities have begun to ap-

[1] These have been concerned with radio astronomy (Martin and Irvine [28]), optical astronomy (Irvine and Martin [23]), and electron high-energy physics (Martin and Irvine [27]).

proach the end of their useful research lifetimes. However, in the debate that followed, it became clear that there was also a pressing policy-requirement for improved methods for assessing the likely *future* success of new research facilities.

The need for predictive policy tools is greatest in the capital-intensive "Big Sciences". There are two principal reasons. First, the investments involved are now of such a magnitude that discussions of them are central in the formulation of overall national science policies. The cost of the planned new LEP accelerator at CERN (over 900 million Swiss francs) is, for example, appreciably greater than the total British Science and Engineering Research Council budget for basic natural science in 1983/4. At a time when there are severe pressures on funding, it is important that investment decisions are taken on the basis of the fullest possible information, particularly in Big Science where such decisions clearly have major implications for the funding of other specialties. [2]

Second, there are reasons for questioning the extent to which the scientific communities in Big Science specialties are still able to make investment decisions solely, or even largely, on the basis of perceived scientific merit (cf. Irvine and Martin [26]). As scientific activity in such specialties is concentrated in ever fewer research centres (for example, in the United States there are in 1984 only four experimental high-energy physics centres, and in Western Europe just two), so decision-making is increasingly influenced by institutional and political pressures. One possible consequence is that new facilities may not be sited at the research centres best able to exploit them to the full. The underlying problem is that it is becoming more difficult in Big Science to locate neutral peers capable of providing sufficiently disinterested judgements; all potential peers tend either to have some professional interest in a proposed new project, or to be associated with a competing set of interests which would benefit from a negative decision on that project.

The emergence of such imperfections in the peer-review process (the method traditionally used in allocating resources for basic science) has increased the need for other sources of information to aid policy-makers in determining future priorities – particularly where the decisions involve the distribution of funds between specialties as they do indirectly in the case of Big Sciences. In this respect, we would argue that systematic comparisons of past scientific performance (of the sort reported in Papers I [29] and II [24]) can be an important input into decision-making, particularly when generated by analysts outside the social structure and reward system of the reseach community concerned. When resources are heavily concentrated on a single centre or research facility, it becomes all the more important to ensure that they are used effectively and that there is some monitoring procedure for quickly identifying problems limiting the scientific output. Equally important, when centres apply for funds to replace obsolete equipment (for example, an accelerator or telescope), it is desirable to know how successfully the previous research facilities have been operated, in particular compared to similar facilities at rival centres. "External" evaluations are probably more likely to be trusted by sections of the scientific community outside the Big Science concerned; while they may accept the need for major investment in capital-intensive research facilities, they clearly require some assurance that such facilities are actually producing important scientific results or are likely to do so in the future.

This said, it is clear that "track record" is only one element, albeit an important one, in helping to predict likely future research performance. Although perhaps marginally better than no forecasts at all, predictions of the future based upon simple extrapolations from the past are unlikely to be particularly successful (see Miles and Irvine [32]), especially in areas of science characterized by rapid change. This is not to say that extrapolations should never be attempted, but rather that they are more likely to have some validity when based upon an understanding of the factors that have structured past performance, and of the extent to which they are likely to affect future performance. For this reason, an attempt has been made in the present study to extend the previous

[2] For example, Britain spent £83.2 million in supporting basic research in astronomy, space, and nuclear physics in 1981/82. This compared with £21.8 million spent by the Science and Engineering Research Council on peer-reviewed grants for all other areas of natural science, £20.7 million by the Social Science Research Council, £42.1 million by the Agricultural Research Council, £54.3 million by the Natural Environment Research Council, and £101.7 million by the Medical Research Council (see Irvine and Martin [26]).

evaluation methodology (which was concerned with assessing the past performance of various Big Science facilities – see footnote 1) to include an examination of the reasons why the research facilities in question (high-energy physics accelerators) have performed with greater or lesser success (see in particular tables 9, 11, and 12 in Paper II [24]). A range of information has thus been obtained concerning the factors structuring success and failure in the past – for example, whether there has been a strong user-group associated with the accelerator, whether the scientific management of the facility has been effective, or whether from a technical viewpoint the accelerator and subsidiary instrumentation have been of a sufficiently high quality.

Yet even assuming that these factors continue to be important, this is still an insufficient basis on which to make decisions concerning a major new facility. What is needed in addition is information relating to the characteristics of the proposed new facility and the research to be carried out on it – the "ripeness" of the research area, the relation of the instrument to other facilities (for instance, whether it duplicates, or is complementary to, facilities elsewhere), the degree of technological "risk" associated with the instrument (for example, whether it relies on a new and untested technique), and the likelihood of "spin-off" in the form perhaps of instrumental techniques that can be applied in other research activities.

In this way, then, it is possible to envisage the formulation of a set of criteria to help assess the merit of proposals for future research facilities. Certainly, many of these criteria are already used informally by the scientific community. However, such criteria are not always made publicly explicit, and the information required to utilize them tends to be accessible *only* to researchers within the specialty concerned. Hence, in specialties where there is a high degree of concentration of research efforts (and a resulting formation of strong interest-groups focused on each of the main centres in the field), there is a danger that the peer-review process may be reduced to little more than a battle between institutional interests (cf. Irvine and Martin [26]) The underlying problem is that, in the present system of decision-making, there are inadequate mechanisms for ensuring accountability to those *outside* the Big Science concerned, even to scientists in neighbouring specialties. The aim in this paper is to consider the possibilities for developing a framework to be used in analyzing future policy options in a systematic and publicly accessible way. If the scope for institutional lobbying is to be limited, then deliberations over costly new research instruments like accelerators or telescopes must be open to scrutiny by a wider body of scientific and public opinion. This would *not* remove ultimate responsibility for the distribution of resources for research from scientists, but merely ensure that scientific decision-making was conducted in a more public arena.

In this paper, we attempt to assess the future scientific prospects for CERN. In particular, we consider whether the set of criteria described below can aid analysis of the likely prospects for CERN's major new accelerator, LEP, relative to those of accelerators elsewhere. If so, to what extent do the results throw into question the adequacy of existing decision-making procedures for determining the future priorities of a major laboratory like CERN? This is especially pertinent since LEP marks something of a departure from previous construction projects in terms of the long-term financial commitment it implies for CERN Member States and its delimitation of future options for the laboratory. Whereas previous accelerator construction programmes at CERN have been essentially discrete efforts with a time-horizon of perhaps six years from inception to commissioning, the LEP project is rather different. Besides Phase I of the project (to reach a beam-energy of 50 GeV) scheduled for completion in the second half of 1988, subsequent phases to reach first approximately 90 GeV and then 130 GeV are also planned, each of which may take several years to complete. Formal approval of these later phases will obviously not be obtained for some time, but there must be confidence at CERN that, once Phase I and the 27-kilometre tunnel it requires have been completed, the arguments for continuing to increase the energy up to 130 GeV will be exceedingly strong. As with Phase I, high-energy physicists will probably be able to argue that these later phases can be carried out without any overall increase (in real terms) in the CERN budget. At such a rate of funding, a 130 GeV version of LEP would probably not come into operation until the early 1990s, i.e. nearly ten years from now.

At that stage, there are likely to be strong arguments that, since the 27-kilometre tunnel already exists, it would be relatively cheap to construct a superconducting proton synchrotron of perhaps up to 10,000 GeV (10 TeV) by placing it alongside LEP. Such a machine could take another five years or so to construct. Moreover, once completed, it might be converted into a large proton–antiproton collider; and, in conjunction with LEP, it could also be used to generate high-energy electron–proton collisions. These latter two projects might each take a further three or four years to bring to fruition. In this way, LEP will provide CERN with a succession of possible new projects well into the first decade of the next century.

Since the choice to embark on LEP will have implications for what CERN is doing twenty years or more from now, it is surprising that there was not more *public* discussion of LEP, and especially of its significance for the future development of the laboratory, before the formal go-ahead for the project was given in 1981. Certainly, extensive discussions took place *within* the high-energy physics community. In addition, there were negotiations between high-energy physicists and research-funding agencies in the Member States. However, these discussions were circumscribed by the decision of CERN management that LEP should be treated not as a "new" accelerator but as an extension of existing CERN facilities, a decision taken to minimize the risk that political debate might delay the project's start. LEP may indeed turn out to be the best accelerator for CERN, but what is not clear is whether the fairly heavy reliance on the internal debates of a scientific community whose research interests centre largely around a single laboratory forms an adequate basis for committing substantial long-term resources – particularly when the funding involved is of such a magnitude that it cannot fail to have implications for other scientific specialties.

In analyzing whether improved procedures can be developed for assessing the future prospects of large capital projects, we shall first examine the history of the LEP project. This is followed by a brief review of the other main accelerator (or collider [3]) projects planned or underway around the world. The central sections of the paper then draw certain comparisons between these projects. The first focuses on their relative financial and technical requirements, and attempts to assess the extent to which these are likely to affect their completion within the currently planned schedule. The second examines the scientific potential of each new accelerator, considering in turn whether it will have a world lead in a new energy region (and, if so, for how long), the likely capacity of that accelerator to generate new physics results, and the ability of its user-community to exploit the scientific potential of the new facility to the full. The third and final set of comparisons centres on the spin-off, or contribution to accelerator physics and technology, likely to be generated by each machine, and on its potential for development at a later stage to yield a new research facility. The results of the comparisons along these three dimensions are then used to arrive at an overall assessment of the future prospects for CERN over the next ten to fifteen years. The paper concludes with a brief discussion as to the likely utility of the methodological approach described below for science-policy purposes, particularly in those fields of science characterized by heavy capital expenditure on centralized research facilities.

Given the relative lack of experience that exists with predictive science-policy analysis, this attempt at constructing a framework for systematically assessing future prospects should be regarded as no more than a provisional first step, designed to demonstrate – in particular to senior policy-makers concerned with the overall distribution of research funds, but also to the scientific community at large – the potential utility of such external evaluations. The intention is that such assessment excercises be undertaken prior to major research investment decisions, and then repeated periodically during the construction and operation of a facility. It should also be stressed that studies such as this which are concerned with the future may "date" quite rapidly as they are overtaken by unforseen events. Nevertheless, while questions of detail may change appreciably in the period between the writing of this paper (in late 1983) and its publication and circulation, we believe most of the main issues and trends will not. To this extent, there is therefore an element of "testability" about the validity of the approach that we have adopted.

[3] In this and Papers I [29] and II [24], the term "accelerator" is sometimes used generically to cover both accelerators and colliders.

2. The historical background to the LEP project

Until the start of the 1970s, the history of experimental high-energy physics had been largely dominated by the contributions of proton accelerators. With the exception of deep-inelastic scattering, which was discovered on the Stanford (electron) linear accelerator in 1968 (see Paper II [24]), virtually all the major advances in the field over the previous twenty years had come from proton accelerators – generally from the highest-energy machine operating at the time. Even in 1970, most senior high-energy physicists saw no obvious reason why this trend should not continue, and the thoughts of many of the more ambitious accelerator-builders began to turn to proton synchrotrons with an energy of 1000 GeV (1 TeV), and to proton–proton colliders with a beam-energy an order of magnitude higher than the 30 GeV of the CERN ISR, then nearing completion. Over the next few years, examination of these possibilities advanced furthest at Brookhaven in the United States and at Serpukhov in the Soviet Union, both of which were considering replacements for their existing facilities. Less progress was made at the two other major world proton centres, CERN and Fermilab (near Chicago), where scientists were preoccupied with bringing into operation and exploiting their new accelerators (the ISR, SPS, and 400 GeV machines).

Then, in 1974, high-energy physics was shaken by the so-called "November Revolution." [4] The discovery of the J/psi (followed quickly by those of other related particles) not only completely reoriented the direction of mainstream thinking in particle physics; it also had a dramatic impact on the plans of accelerator-designers. Suddenly, the previously "unfashionable" technology of the electron–positron collider was thrust into the limelight as experimentalists in the United States using the Stanford accelerator, SPEAR, unearthed a succession of crucial discoveries – the psi-prime, the heavy lepton tau, charmed mesons, and so on – that until then had eluded the much higher-energy proton machines. One fairly immediate consequence was the race between the German laboratory, DESY (the largest of the European electron accelerator centres) and Stanford to construct a higher-energy electron–positron collider, [5] with the latter quickly dropping its original plans for a proton–electron facility. A longer-term outcome was that West European physicists began in the mid-1970s to consider the possibility of constructing an enormous 100 GeV electron–positron storage-ring, a proposal which eventually materialized as the LEP project.

Besides the discovery of the J/psi, there was another notable advance within high-energy physics at about the same time that came to exercise a significant influence on the thoughts of accelerator physicists. This was the considerable success of the Weinberg-Salam unified theory of electromagnetic and weak interactions, in particular the confirmation at CERN in 1973 of the theory's prediction of neutral currents (see Paper II [24]). However, the findings of this and subsequent experiments were regarded by physicists as giving only corroborative rather than conclusive evidence in favour of the theory; the crucial test was whether the "intermediate vector bosons" (the neutral Z_0, and the charged W^+ and W^- particles) postulated by the theory as the carriers of the weak force could actually be detected. The eventual formulation of a theory unifying the four types of force encountered in nature has been aptly characterized as "like the Holy Grail to particle physicists" (Robinson [34, p. 192]), and the discovery of the predicted Z and W particles rapidly came to be seen as the vital first step towards that elusive goal. The problem, however, was that the predicted masses of these two types of particle (approximately 90 and 80 GeV respectively) required an accelerator producing a centre-of-mass energy of nearly 100 GeV. This was an energy region far beyond the reach of accelerators then operating in the mid-1970s. As it became clear that it might take up to ten years to construct a purpose-built machine (like ISABELLE in the United States or LEP) capable of attaining this energy, certain physicists began to consider in 1976 whether existing accelerator facilities might be modified to bring the new particles within range somewhat sooner. From this grew the idea for converting the CERN SPS into a proton–anti-

[4] This and the other dramatic discoveries made in the subsequent three years are described in detail in Paper II [24].

[5] Though the Stanford electron–positron collider is named PEP, the first "p" in the acronym originally stood for "proton". See Metz [30, p.853] for details of this earlier project.

proton collider. Since the history of this project is closely interwoven with that of LEP, it is worth looking at it briefly.

During the mid-1970s, concern was being expressed within CERN that the SPS was coming into operation over four years behind the virtually identical Fermilab accelerator (cf. Van Hove [41, p.31]). Hence, when the Italian physicist, C. Rubbia, began to advocate the conversion of the SPS (and the Fermilab accelerator) into a proton–antiproton collider capable of reaching the intermediate vector boson region, he found a receptive audience within the European high-energy physics community. A further incentive to proceed with the project came with the realization that the SPS had been so constructed that it could be converted relatively cheaply and – perhaps of greater importance to CERN – far more quickly than the Fermilab accelerator, giving European physicists the chance of turning the tables and gaining several years lead over their American rivals. One further factor that may have contributed to the attractiveness of this project concerned its role in the longer-term budgetary plans of CERN. In the absence of a major capital-construction project to fill the hiatus between the completion of the SPS in 1976 and commencing the building of LEP in the early 1980s, CERN would have found it politically difficult, if not impossible, to prevent its annual budget being cut from the "ceiling" figure of about 600 million Swiss francs (MSF) to something closer to the "base load" value of approximately 400 MSF required for experimental operation and routine investment purposes. Once reduced to the "base load" figure, it might have proved even harder for CERN to convince all the Member States at a later date that the budget needed to be substantially increased again in order to begin the construction of LEP. [6] The proton–antiproton collider, with its relatively modest cost (200 MSF) for a new high-energy physics facility, neatly filled the five-year "gap" between the completion of the SPS and commencement of investment on LEP. [7]

[6] Certainly, CERN would then have found it difficult to argue that LEP constituted no more than an "extension" to the existing experimental programme (a point discussed further below).

[7] Similarly, one of the main arguments for proceeding with the construction of the ISR in the late 1960s (according to senior physicists interviewed at CERN) had been to fill the "gap" between refurbishing the PS and beginning work on the politically delayed, but higher priority, SPS.

The main uncertainty associated with the proton–antiproton collider concerned the technical difficulty of achieving sufficient luminosity (one of the factors determining the number of particle collisions per second) to make the physics accessible. However, the results of various tests of the technique of stochastic cooling (pioneered at CERN) between 1976 and 1978 were sufficiently encouraging to suggest that a luminosity of 10^{30} cm^{-2} s^{-1} (the eventual design figure) might be achievable – sufficient to generate tens of Z_0 particles and hundreds of W particles per day. Even if a luminosity of approximately an order of magnitude less than this were achieved, it was assumed that this would still be high enough to permit the new particles to be discovered some years ahead of ISABELLE, the Brookhaven proton–proton collider then scheduled to begin operation in about 1983. Even so, the proposed collider was not without its opponents; some users of the SPS accelerator in particular were alarmed that the conversion of the machine into a collider would cause the loss of up to a year from the fixed-target experimental programme (which was already labouring under the handicap of having started over four years behind that of Fermilab), that it might affect the reliability of the original synchrotron (as it in fact did), and that, when completed, the collider would occupy a significant proportion of the SPS's time, leaving insufficient for fixed-target experiments. Nevertheless, the CERN management decided in 1978 that this major sacrifice of prime research time was a price worth paying in return for the chance of discovering the all-important intermediate vector bosons. Once the go-ahead had been given, construction of the collider proceeded rapidly, the first collisions being observed in July 1981, and, as we saw in Paper II [24], the W and Z intermediate vector bosons were discovered amid much publicity in 1983.

In the meantime, there had been significant progress with the LEP project. Between 1975 and 1978, considerable debate had taken place within the West European high-energy physics community over the facilities that would best meet their experimental needs in the period up to the end of this century. Among the options considered for CERN were a very high energy fixed-target proton synchrotron, a proton–proton collider, an electron–proton collider, and a large electron–positron collider, LEP. The European Committee for Future Accelerators (ECFA) played a prominent role in this discussion (much of it was carried out in the closed sessions of the "Restricted ECFA"), and eventually in 1977 came down strongly in favour of LEP. Their decision appears to have been based on a widely accepted set of scientific arguments – in particular the successes of existing electron–positron colliders in contributing to the "new physics" of quarks and leptons, and the prediction by a large body of theorists that the energy of the Z_0 was such that LEP would be the ideal research instrument on which to mount a systematic investigation of the particle's properties (its width, decay-channels, and so on). Another strongly influential factor was the notion of complementarity: Fermilab and Serpukhov were planning proton synchrotrons of 1 TeV and 3 TeV respectively; and Brookhaven was about to embark upon the construction of a large proton–proton collider. By opting for LEP, so the argument went, CERN would be guaranteed a unique experimental facility, thus avoiding the direct competition that had limited the relative scientific impact of both the PS and SPS (see Paper II [24]). A final factor that appears to have influenced the choice of LEP was the potential of such a facility for future development – the so-called "real-estate argument" – a feature of the decision we shall discuss later.

By 1978, the *CERN Courier* [7, p.431] was able to report that, after much discussion within the European high-energy physics community,

> The consensus has come out strongly for an electron–positron machine to take colliding beam energies well beyond those which are accessible with PETRA at DESY and PEP at Stanford.

The main technical parameters of the machine had also been determined by this time (in particular, a ring of 30 kilometres in circumference had been agreed, rather than the 50-kilometre and 20-kilometre options considered previously), while cost estimates were calculated shortly afterwards. By adopting a "missing-cavity design" (in which radio-frequency cavities were progressively added), it was proposed to build LEP in several stages, reaching a beam-energy of 50 GeV for a cost of just over 900 MSF. Energies of 65 GeV and 90 GeV would subsequently be achieved for a total cost of 1065 MSF and 1275 MSF respectively (cf. [8, pp.10–11]). The final design figure of approximately 130 GeV would be reached by replacing the conventional radio-frequency cavities with elements based on new superconducting technology. [8]

Agreeing upon the design and costings for a new project is one thing; obtaining the funds and political agreement from Member States to go ahead with the construction programme is another, as we saw in the discussion of the history of the SPS project (see Paper I [29]). The SPS was severely delayed because of lack of agreement over both finances (with Britain threatening withdrawal) and the site of the new accelerator. According to senior physicists whom we interviewed, similar political infighting among Member States began to develop over the LEP project, with strong pressure being exerted in particular by the Germans to site the new accelerator in the Federal Republic. It also soon became apparent from preliminary discussions with funding bodies in the various Member States that it would be politically difficult, if not impossible, to finance the new accelerator by an increase in the overall CERN budget. With the experience of the SPS behind them, the CERN management this time took clear and decisive action. First, they decided to propose to Member States that the new machine be financed from the existing CERN budget (some 600 MSF per annum at 1978 prices), even though this would entail closing the ISR [9] and perhaps the small synchrocyclotron, and curtailing the SPS experimental

[8] Robinson [35, p.531] has quoted an approximate figure of 400 MSF for the cost of replacing the conventional cavities by superconducting elements and for construction of the four remaining experimental areas. This project is referred to below as LEP-130.

[9] This decision resulted in a vocal compaign by a significant minority of CERN physicists against closure of the ISR. Arguing that the machine had a good track record and was still a unique facility, many physicists claimed in interviews with us that, when their support for LEP was first sought, it was not made clear that proceeding with this project might involve the closure of the ISR. Instead, many would have preferred to see the LEP project spread over a longer period, thus freeing resources for continuing the ISR programme. Some admitted that closure of the ISR was the political sacrifice that probably had to be made to guarantee the future for LEP, but were bitter about the lack of public discussion on the matter. In a similar vein, the Scandinavian countries rapidly mobilized against the possi-

programme. This also involved trimming the costs of LEP, in particular by using the SPS to inject electrons and positrons into the main ring rather than building a new 22 GeV electron synchrotron. Although this further reduced the SPS programme by up to 10 percent, it slightly decreased the financial and manpower commitments for LEP. Second, it was decided that LEP could now be treated as "an extension of existing CERN facilities" [9, p.192], to be absorbed under the "basic programme" of CERN, rather than as a new accelerator [10] requiring the authorization by Member States of a supplementary programme – authorization which might not be immediately forthcoming. Third, in response to worries by the smaller countries about possible cost over-runs in the LEP project as a result of technical problems, [11] CERN had to agree that, in future, any proposed real increase in the laboratory's annual budget could be vetoed by a single Member State, instead of the previous situation where agreement could be obtained by a simple two-thirds majority (cf. Walgate [42, p.275]). This means that any unforseen difficulties requiring a major increase in expenditure will have to be met by either postponing the date of the first experiments on LEP or curtailing even further the existing research programmes on other facilities. We shall return to discuss the significance of this later. Although there were initially certain reservations among three of the Member States about the effect of LEP on other experimental programmes, by the end of 1981 all twelve had agreed to proceed with the project. As matters stand at the time of writing, it is anticipated that Phase I (to achieve a beam-energy of 50 GeV and to construct four out of the eventual eight experimental halls) will be completed by the second half of 1988 (cf. [22, p.228]). The estimated cost in 1981 prices is 910 MSF; to this must be added between 250 and 280 MSF for building the four planned detectors (cf. Robinson [38, p.722]), of which CERN will contribute about 60 MSF, the remainder being provided by the national funding agencies which will support the research activities of the predominantly university-based LEP users (including many from non-Member States of CERN).

ble closure of the synchro-cyclotron (which many of their physicists use) and eventually obtained agreement for the continuation of the experimental programme on this accelerator on a limited basis.

[10] This would seem to be a somewhat artificial distinction and one that might prove rather hard to defend if subjected to wider public scrutiny. According to this definition, there have been virtually no major "new accelerators" since 1972, nor are any planned; the ISR and SPS were merely "extensions" of the CERN PS, SPEAR and PEP were extensions to the Stanford linear accelerator, ISABELLE would have been just an extension of the Brookhaven AGS, and so on. While this reclassification of the status of LEP may have been admirably suited to CERN's purposes in that it greatly reduced the risk of political disagreement among Member States over the project, it constrained the options of Member States as regards discussions of other types of new facility. It might have been expected that such a terminological "sleight of hand" would give rise to some controversy about high-energy physicists' arrogation of government funds while seeking to limit the degree of public accountability, but none seems to have arisen.

[11] Problems were, for example, anticipated with tunnelling under difficult rock conditions in the Jura mountains. As a result of testborings, the precise location of the LEP tunnel had to be changed: the circumference of the accelerator was decreased from 30 to 27 kilometres and the length of the tunnel under the mountains reduced from 12 kilometres to first 8 and then 3 kilometres (see Walgate [42, p.275]). Severe construction problems still potentially remained to be overcome, however.

3. Likely competitors to LEP

Before we move on in subsequent sections to assess the comparative advantages and disadvantages of LEP, it is first necessary to examine the main characteristics of potential rival accelerators currently in operation at other laboratories or planned for construction over the next decade. These competitor machines have been classified into four categories: (1) fixed-target proton synchrotrons; (2) proton colliders; (3) electron–proton colliders; and (4) electron–positron colliders.

3.1. Fixed-target proton synchrotrons

Details are given in table 1 of the three planned (or possible) major new accelerators which may be commissioned before the end of the century. For comparative purposes, similar data are also given for the largest proton synchrotrons already operating.

3.1.1. 3 TeV UNK (Serpukhov, USSR)

As was seen in Paper II [24], by the time the experimental programme of the Serpukhov 70 GeV accelerator was in full operation, the much

Table 1
Major proton synchrotrons – present and proposed [a]

	Date of first experiments	Maximum beam-energy (GeV)	Centre-of-mass energy (GeV)	Intensity (particles per pulse)	Estimated cost (MSF [b])	Period of world leadership (years)	Future options [c]
Serpukhov	1967	76	12	5×10^{12}	–	5	–
Fermilab	1972	500 [d]	31	2×10^{13}	–	4	–
CERN SPS	1976	500 [d]	31	2×10^{13}	–	8	–
Fermilab 1 TeV	1985	1000 [e]	43	3×10^{13}	~ 250?	~ 6?	$p\bar{p}$, pp
Serpukhov UNK	1990?	3000	75	~ 10^{13}?	~ 400?	~ 5?	pp, $p\bar{p}$
CERN Superconducting Synchrotron?	mid-1990s?	10000?	135	?	?	?	$p\bar{p}$, pp, ep

[a] This lists only the world's highest-energy machines.
[b] These are the approximate estimated costs in millions of Swiss francs based on 1983 exchange rates. (An exchange rate of \$1 = 1 Soviet rouble has been used.)
[c] pp = proton–proton collider; $p\bar{p}$ = proton–antiproton collider; ep = electron–proton collider.
[d] This figure is rarely achieved. The "normal" maximum energy is 400–450 GeV.
[e] Experiments at an energy of about 800 GeV are planned for 1984.

higher-energy machine at Fermilab was nearing completion. Once their world lead in energy disappeared in 1972, Soviet high-energy physicists began planning for the construction of a major new proton synchrotron, UNK, the energy of which was originally set at 2000 GeV (2 TeV). It was reported in 1976 that the Soviet Government had pledged 200 million roubles (about \$200 million) to the project [4, p.401]. By 1980, the final design parameters had been settled and the proposed energy raised to 3 TeV. [12] It was originally estimated that the machine would take approximately seven years to complete (Wilson [43, p.40]) from the time construction began in 1981, although 1990 is now considered to be a more realistic completion date (cf. [45, p.301]).

3.1.2. 1 TeV Doubler / Saver (Fermilab, USA)

Shortly after completing its 400–500 GeV accelerator in 1972, Fermilab began work on the Energy Doubler project to increase its energy to 1000 GeV (1 TeV) by installing superconducting magnets. As with LEP, the history of decision-making over this new accelerator has been rather complex. The project was initially classified by Fermilab as an internal R&D programme, for which no special authorization was required; at this stage, the cost was roughly estimated at \$35 million [3, p.13], which the laboratory management felt could be found from within its general operating budget. In 1977, however, the United States Office of Management and Budget insisted that the work had to be defined as a construction project. Since the procedure for obtaining authorization for projects costing under \$50 million is considerably less severe than for those requiring higher expenditures, efforts were made to keep the estimated costs below this limit. When it eventually became clear that this was no longer feasible (in part because of greater than expected technical problems with superconducting magnets), the project was split into two parts: (1) the Energy Doubler (or Energy Saver [13]) – the construction of a "bare" superconducting magnet ring; and (2) the Tevatron – the provision of all the subsidiary instrumentation to make possible fixed-target experiments at an energy of 1 TeV, and proton–antiproton collisions at a centre-of-mass energy of 2 TeV. Furthermore, when the estimated costs of the Tevatron in turn rose above the \$50 million mark, this project too was divided into two parts – Tevatron I and Tevatron II. By 1981, the estimated costs of con-

[12] The existing 70 GeV accelerator, suitably upgraded, will be used to inject protons into a conventional 400 GeV booster synchrotron, and subsequently into a 3 TeV superconducting-magnet proton synchrotron (cf. [11, p.147]).

[13] Although Fermilab's primary consideration was to build a 1 TeV accelerator, the Department of Energy (the main funding body in the US for high-energy physics) was more concerned that the laboratory should make efforts to reduce its electricity bill, a goal towards which superconducting magnets would contribute: hence the alternative project title, the Energy Saver.

structing the 1 TeV fixed-target accelerator (Tevatron II) had risen to between $110 and $120 million,[14] to which must be added the cost of upgrading the beams and experimental areas for the higher-energy experiments. Experiments at an energy of between 800 and 900 GeV are to be run in 1984, with the maximum energy being achieved a year later (cf. Robinson [39, p.816]). It will probably then remain the world's highest-energy accelerator for the rest of the 1980s.

3.1.3. 10 TeV Superconducting Synchroton (CERN)

The 27-kilometre tunnel for LEP is designed to be large enough to accommodate one or more additional rings of magnets. Consideration has already been given at CERN to the possibility of installing a ring of high-field superconducting magnets (fields of up to 10 tesla should be feasible by the mid-1990s) to yield a proton synchrotron with a maximum energy of approximately 10 TeV (cf. Wojcicki [44, p.26]).

3.2. Proton colliders

Details are given in table 2 of the main proton (and antiproton) colliders likely to become operational over the next decade; as before, the table includes comparative data on currently existing facilities. In addition, information on the proposed Colliding Beam Accelerator (formerly called ISABELLE) is included, even though this project was halted in 1983, the reason being to ascertain the likely prospects for this collider had it been completed.

3.2.1. 400 GeV Colliding Beam Accelerator/ ISABELLE (Brookhaven, USA)

When Brookhaven completed the AGS Conversion Programme (see Paper II [24]) in 1971, the laboratory began to plan a superconducting proton–proton collider with a beam-energy of 200 GeV. Early tests with the prototype superconducting magnets proved very encouraging; [15] the field-strength of 4 tesla required for a 200 GeV machine was easily attained, and, by 1976, a figure of 5 tesla seemed to be within reach. The US High Energy Physics Advisory Panel (HEPAP) was pleased with the progress achieved in this technologically demanding development programme, and, urged on by theorists eager to see experimental data from even higher energies, recommended that the planned beam-energy be increased to 400 GeV. With hindsight, this decision is now regarded by many physicists as a mistake. Although an industrial contract was awarded in 1978 for winding the magnet coils, by 1980 it was clear that the magnet design (the so-called "braid" magnet) chosen by Brookhaven was incapable of achieving the necessary field-strength. In 1981, after considerable internal organizational upheaval at the laboratory, these plans were finally dropped to be replaced by a design much closer to that being developed at other accelerator centres, including Fermilab.

Inevitably, these severe technical problems resulted not only in a dramatic escalation in the estimated costs of ISABELLE, as it was then called, but also in the likely completion date being pushed ever further into the future. In 1976, the cost of the 200 GeV collider was estimated as $166 million, and a year later, after the energy had been increased to 400 GeV, a figure of $245 million was quoted, together with a likely completion date of 1983. Construction of the accelerator tunnel started later that year, and, by the time the original magnet design was scrapped, a total of $130 million had already been spent (cf. Broad [1, p.1089]). By 1982, it was clear that the collider would not be completed until 1988 and that a further $500 million would be required (cf. Trilling [40, p.26 and p.49]). In view of the parlous state of US high-energy physics funding, serious doubts were expressed about whether, despite the major expenditure already incurred, it was worth completing ISABELLE (cf. [40, p.50]).

In response to this situation, various alternatives to ISABELLE were considered, including an electron–proton collider and a heavy-ion collider. However, the cost-savings associated with these options were found to be modest and were deemed insufficient to justify the significant reduction in physics potential associated with them. As a result, the Brookhaven management decided in February 1983 to persevere with the original plans for a

[14] Figures provided to the authors during a site-visit to Fermilab in late 1981.

[15] As early as 1973, it was reported that, "The performance of the [prototypes] seems to have answered several outstanding questions concerning superconducting magnets. One concerns the reproducibility of magnets." [2, p.374]. In the light of the major problems with magnets experienced later (described below), this conclusion seems to have been a little premature.

Table 2
Major proton colliders – present and proposed

	Date of first experiments	Maximum beam-energy (GeV)	Centre-of-mass energy (GeV)	Luminosity (cm^{-2} s^{-1})	Estimated cost (MSF [a])	Number of experimental areas	Period of world leadership (years)	Future options
CERN ISR	1971	31	62	10^{32}	–	8	10	–
CERN $p\bar{p}$ [b]	1981	270	540	10^{30}? [c]	200 [d]	2	5	–
Fermilab $p\bar{p}$ [b] Collider	1986	1000	2000	10^{30}?	~ 350? [d]	2	~ 5–10?	–
Brookhaven ISABELLE [e]	1987/8	400	800	~ 10^{33}?	~ 1300?	6	0	ep, heavy-ion collider
Serpukhov UNK	early 1990s?	400 + 3000	2200	~ 10^{32}?	?	?	?	3000 + 3000 GeV $\bar{p}p$ and pp
Fermilab Dedicated Collider?	early 1990s?	2000	4000	~ 10^{31}?	~ 900?	6	?	ep
CERN Superconducting Collider?	mid-1990s?	10,000	20,000	?	?	?	?	ep
US Superconducting Super Collider?	mid-1990s?	20,000	40,000	~ 10^{33}?	6000–8000? [d]	?	?	?

[a] See note b to table 1.
[b] Proton–antiproton collider.
[c] This is the design-figure. The highest value obtained up to the end of 1983 was 2×10^{29}.
[d] This includes the costs of the detectors.
[e] Project discontinued in 1983.

proton–proton collider (now renamed the Colliding Beam Accelerator or CBA) (cf. [21, p.127]). In an effort to regain the support of the US high-energy community for the project, an accelerated construction schedule enabling the first colliding beams to be obtained in October 1987 was drawn up (cf. Wojcicki [44, p.31]). However, in July 1983, HEPAP recommended that the CBA be discontinued (cf. [44, p.7]) and the project was finally halted by the Department of Energy a few months later. By then, a total of some $200 million had been spent on the accelerator.

3.2.2. 1 TeV proton–antiproton collider – Tevatron (Fermilab, USA)

The Fermilab proton–antiproton collider project began in late 1976, replacing earlier plans for a proton–proton machine. As with the laboratory's 1 TeV fixed-target accelerator, the projected cost has risen appreciably over time – from an initial value of some $40 million to an estimated $70 million in 1981. In addition, the decision in 1982 to develop a new and more efficient antiproton source was reported as increasing the cost by a further $40 million. To this must be added the outlay on the two detectors, giving an overall total of around $170 million. [16] The first proton–antiproton experiments are planned for 1986, which will give CERN five years lead with its (lower energy) collider – the same advantage previously enjoyed by Fermilab with respect to the 400 GeV proton synchrotrons.

3.2.3. Serpukhov collider (USSR)

Once the 3 TeV proton synchrotron (UNK) has been completed at Serpukhov, it is intended to collide 3 TeV protons with 400 GeV protons from the booster synchrotron (see footnote 12 above), giving a centre-of-mass energy of approximately 2200 GeV. There are also preliminary plans for phase II of UNK which include 3 + 3 TeV proton–antiproton colliding beams and, with the addition of a second superconducting-magnet ring, 3 + 3 TeV proton–proton collisions.

3.2.4. 2 TeV Dedicated Proton–Antiproton Collider (Fermilab, USA)

In 1983, Fermilab put forward a proposal to build a proton–antiproton colliding facility (called the Dedicated Collider) with planned beam-energies of up to 2 TeV and a design luminosity of over 10^{31} cm^{-2} s^{-1}. Although it would use the 1 TeV Tevatron as an injector, it would have the advantage of freeing the Tevatron to run almost entirely for fixed-target physics. The necessary preparatory R&D and construction were estimated as likely to take some six years to complete, and to cost between $380 million and $450 million (cf. Wojcicki [44, p.34]). However, the US High Energy Physics Advisory Panel (HEPAP) decided in July 1983 that Fermilab should not proceed with the Dedicated Collider for the time being, and that efforts should instead be concentrated on the Superconducting Super Collider (see below).

3.2.5. CERN Superconducting Collider

As already mentioned, the tunnel for LEP can accommodate one or more additional rings of magnets. By installing a ring of high-field superconducting magnets, a proton–antiproton colliding-beam facility with beam-energies of up to 10 TeV could be constructed at CERN. It has been estimated that, with appropriate funding, such a facility could be built over a period of about five years (cf. [44, p.26]).

3.2.6. Superconducting Super Collider (SSC) (USA)

The year 1983 marked something of a turning point in US particle physics. After strong pressure from the Administration, the High Energy Physics Advisory Panel (HEPAP) was finally able to reach a consensus agreement that the Colliding Beam Accelerator project at Brookhaven be discontinued. According to those interviewed by us, this agreement would never have been reached had high-level promises not been forthcoming that support would be likely for an even larger project. As a result, frantic activity took place in early 1983 to discuss possibilities for the new facility. A decision was soon made to opt for an accelerator/storage-ring complex to accelerate protons to an energy of 10 to 20 TeV, and to collide them. Outline plans for the facility were discussed at a workshop held at Cornell University, HEPAP unanimously endorsing in July 1983 (three months later) a recommendation by its Sub-panel on New Facilities for the immediate initiation of the project. Provisional estimates suggest that the construction of such a machine together with the necessary preliminary

[16] Robinson [39, p.816] quoted a figure of $300 million in 1982 for the combined cost of the 1 TeV accelerator (including its upgraded experimental areas) and the collider.

R&D could take between nine and fifteen years and cost a total of up to $3 billion (cf. [47, p.390]), while four or five detectors could cost as much as $ 1 billion (cf. [46, p.19]). Budgetary plans have already been laid for a preliminary R&D phase for the project. If the United States then decides to proceed further, the Department of Energy annual budget for high-energy physics would have to be approximately double the 1983 figure during the five to six years of construction.

3.3. Electron–proton colliders

Nearly every major accelerator centre has at one time or another considered building an electron–proton collider, although this has tended to be regarded as a possible second stage in (and a subsidiary justification for) the construction of a proton or electron accelerator. Until recently, however, these schemes have rarely progressed beyond the conceptual stage. Table 3 lists details of the colliders in various stages of planning.

3.3.1. 30 GeV electron and 820 GeV proton collider – HERA (DESY, West Germany).

The West German national accelerator centre, DESY, has perhaps been the most active of the major high-energy physics laboratories in pressing for the construction of an electron–proton collider, and they appear to have been the first to prepare a detailed proposal (cf. Robinson [36, p.530]). Initially, the planned project (PROPER) involved adding a proton synchrotron to the existing electron–positron ring (PETRA) to yield collisions between beams of electrons at 20 GeV and protons at 280 GeV (cf. [5, p.364]). This scheme attracted strong support from the European Committee for Future Accelerators, who argued that it should be Europe's main priority after LEP. By 1980, however, DESY – at the time flushed with success as a result of completing PETRA two years ahead of a similar collider then being built by their great rivals at Stanford – adopted a more ambitious project for colliding 30 GeV electrons with 820 GeV protons. It was estimated that HERA, as it was named, would cost about 650 million Deutschmarks (some $265 million or 530 MSF) over a construction period of seven years (cf. Robinson [36, p.530]). [17]

The year 1980 also saw the German Federal Ministry for Research and Technology (BMFT) establishing an Advisory Committee (chaired by K. Pinkau to review and prioritize various Big Science projects then under consideration in the country. The Pinkau Committee, which reported in spring 1981, recommended that, in high-energy physics, continued German participation in CERN, and hence in LEP, be given first priority. In addition, it recommended approval in principle of the HERA proposal, but subject to certain reservations – in particular, urging that the initial electron–positron option be dropped, thus going some way towards assuaging the fears of those who believed that the energy of the electron ring might

[17] The electron ring would be built first, giving an option for carrying out 30 GeV electron–positron experiments in about 1986. This gave rise to fears among certain LEP supporters that the energy of this electron–positron collider might be "stretched" using superconducting radio-frequency cavities to achieve 50 GeV, and hence to reach the Z_0 region a year or so ahead of LEP.

Table 3
Major electron–proton (ep) colliders – present and proposed

	Date of first experiments	Maximum beam-energy (GeV)	Centre-of-mass energy (GeV)	Luminosity ($cm^{-2} s^{-1}$)	Estimated cost (MSF [a])	Number of experimental areas	Period of world leadership (years)	Future options
DESY HERA	1990	30 + 820	315	3×10^{31}	~ 540 [b]	4	5 + ?	?
KEK TRISTAN	~ 1990?	25 + 300	175	?	?	4?	0?	?
CERN ep [c]?	mid-1990s?	130 + 270		?	?	?	?	?

[a] See note b to table 1.
[b] This is based on the most recent estimate of 660 million Deutschmarks at 1981 prices (cf. [20, p.90]).
[c] This project might involve colliding 130 GeV electrons from LEP with 270 GeV protons from the SPS, one of the possible long-term options for CERN.

be stretched to investigate the detailed properties of the Z_0 particle ahead of LEP (see footnote 17). The other main condition was that construction of HERA should not commence until 1984, partly for financial reasons (the limited funds available should first be used to exploit the existing collider, PETRA), and partly to give time for the further research and development work on the superconducting magnets required for the proton ring.[18] In 1983, BMFT decided to include in its planned budget for 1984 funds to enable construction of HERA to begin, provided that sufficient foreign participation in the project could be attracted (cf. [20, p.90]). It was estimated that construction would take seven years.

3.3.2. 30 GeV electron and 300 GeV proton collider – TRISTAN (KEK, Japan)

As we shall see below, the Japanese National Laboratory for High-Energy Physics (KEK) has embarked upon the construction of a 30 GeV electron–positron collider, TRISTAN. In a second stage of the project, it is planned to add a ring of 4.5 tesla superconducting magnets capable of accelerating protons to an energy of 300 GeV. Collisions of the proton beam with 25 GeV electrons will yield a centre-of-mass energy of about 175 GeV. If authorized in 1984, this project should be completed by about 1990 (cf. Trilling [40, p. 19]).

3.3.3. Other possible electron–proton colliders

As in the past, many of the new accelerators now being proposed or built envisage some form of future electron–proton option. For example, the Dedicated Collider project proposed by Fermilab (see above) incorporated an option for colliding 10 or 20 GeV electrons with 2 TeV protons (cf. Wojcicki [44, p.33]). CERN and Serpukhov also both have a future electron–proton option under consideration. At the former, electrons from LEP could be collided with protons from the SPS (cf. [8, p.5]). However, since it is now more difficult to obtain authorization for increasing the CERN annual budget (in view of the pressures on scientific funding in most Member States), the construction of such a facility could probably not be undertaken until the completion of LEP (and the achievement of a beam-energy of 130 GeV) in the early 1990s. Similar financial considerations suggest that Serpukhov's preliminary plans for colliding 20 GeV electrons with 3 TeV protons could also not be realized until well into the 1990s.

[18] Brookhaven's traumatic experiences with ISABELLE showed all too clearly the dangers of proceeding with the construction of a new accelerator before all the necessary R&D on superconducting magnets had been completed.

3.4. Electron–positron colliders

Because of the very great interest in ascertaining whether the Z_0 actually existed, there was tremendous eagerness in the high-energy physics community during the late 1970s and early 1980s to reach this energy region as quickly as possible. Although the CERN proton–antiproton collider achieved this goal first, its relatively low luminosity means that it will be able to probe only a small part of the physics of the Z_0. Hence a great deal of effort has been put into planning the construction of electron–positron colliders, both at CERN with LEP, and at a number of other laboratories. Table 4 lists the main details of LEP and its likely competitors.

3.4.1. 30 GeV TRISTAN (KEK, Japan)

In 1981, the Japanese Government approved the construction of the first phase of TRISTAN, an electron–positron collider capable of reaching a centre-of-mass energy of 60 GeV. It was estimated that the project would take five years to complete and cost a total of 7.5×10^{10} yen (about $350 million or 700 million Swiss francs) (cf. [10, p.103]). The collider is being built at KEK, the Japanese National Laboratory for High-Energy Physics, in the "Science City" of Tsukaba. KEK, like DESY in West Germany, is eagerly seeking active participation by overseas groups in the experimental exploitation of the machine (cf. [18, p.3]).

TRISTAN will operate initially with conventional radio-frequency cavities, but superconducting cavities are being developed at KEK and elsewhere which could permit the centre-of-mass energy to be raised at a later stage to approximately 90 GeV, bringing Z_0 physics within range (cf. [18]). In addition, as we saw earlier, there are plans to add a proton ring, thereby generating electron–proton collisions.

3.4.2. 50 GeV Stanford Linear Collider – SLC (SLAC, USA)

In 1977, when the European Committee for

Table 4
Major electron–positron colliders – present and proposed

	Date of first experiments	Maximum beam-energy (GeV)	Centre-of-mass energy (GeV)	Luminosity ($cm^{-2} s^{-1}$)	Estimated cost (MSF [a])	No. of experimental areas	Period of world leadership (years)	Future options
DESY PETRA	1978	19 [b]	38	10^{32}? [c]	–	4	8	–
SLAC PEP	1980	18	36	10^{32}? [c]	–	6	0	–
KEK TRISTAN	1986	30	60	$\sim 10^{31}$	~ 700	4	1	TRISTAN-45, ep
SLAC Linear Collider (SLC)	1987	50	100	6×10^{30}	240 [d]	1	1?	SLC-70
Cornell CESR-II [g]	1988	50	100	3×10^{31}	680	4	$\frac{1}{2}$	–
CERN LEP-50	1988	50	100	3×10^{31}	~ 1200 [e]	4	~ 2?	LEP-90/130, p, $\bar{p}$p, ep
CERN LEP-90	~ 1990?	90	180	10^{32}	~ 350?	4	~ 2?	LEP-130, p, $\bar{p}$p, ep
CERN LEP-130	early 1990s?	130	260	?	~ 400? [f]	8	3–5?	p, $\bar{p}$p, ep
Novosibirsk VLEPP	mid-1990s?	150	300	$\sim 10^{32}$	?	5	3–5?	VLEPP-500
SLAC Large Linear Collider?	mid/late 1990s?	1000	2000	$\sim 10^{32}$	~ 6000	6	?	?

[a] See note b to table 1.
[b] Upgraded to 20 GeV in 1982, and 22.5 GeV in 1983.
[c] Yet to achieve the design figure.
[d] An additional 100 MSF will be needed to provide a second interaction region and new detectors (cf. Trilling [40, p. 52]).
[e] The collider itself will cost 910 MSF (in 1981 prices) and the four detectors a further 250–280 MSF.
[f] This excludes the cost of the four additional detectors (perhaps another 250 MSF).
[g] Project discontinued in 1983.

Future Accelerators decided that LEP should be CERN's next major facility, it could give as one of the justifications for this choice the argument that LEP would be unique, and therefore complementary to other new accelerators planned around the world. Now, however, this argument is less valid in view of the 50 GeV Linear Collider currently under construction at the Stanford Linear Accelerator Center (SLAC). In 1979, B. Richter began to explore the possibilities of modifying the laboratory's linear accelerator, upgraded in energy from the original 20 GeV to 50 GeV, [19] to collide electrons and positrons. Although technically extremely difficult (the principal problems is in developing accurate beams to ensure sufficient collisions), Richter was confident that this novel accelerator technology would provide a quick and cheap way of reaching the potentially important 100 GeV centre-of-mass energy region. The cost of the SLC was estimated in 1981 to be only $63 million [11, p.146]. Although revised to about $120 million in 1982 (cf. Trilling [40, p.52]), this is still only about one quarter of that being spent on LEP to achieve the same energy (although LEP can be subsequently extended to attain much higher energies). Stanford did not obtain formal governmental authorization for the project to begin until Fiscal Year 1984 (cf. [19, p.81], but it had been funding the necessary R&D since 1980 (using the laboratory's operating budget), with actual construction beginning in 1981 (cf. [15, p.8]). It was estimated in 1983 that the first collisions will be obtained by early 1987 (cf. Wojcicki [44, p.25]), just over a year ahead of LEP.

[19] If there was sufficient physics interest, the energy of the SLC could subsequently be increased to 70 GeV (this is referred to below as SLC-70).

3.4.3. 50 GeV CESR-II (Cornell, USA)

Encouraged by their success in the late 1970s with the CESR electron–positron collider (see Paper II [24]), Cornell scientists began to press their case to enter the race to discover the Z_0 particle, proposing the construction of a second-generation electron–positron storage-ring (CESR-

II). By using superconducting radio-frequency cavities (see footnote 20 below), it was estimated that a beam-energy of 50 GeV might be reached in early 1988, some months ahead of LEP, and for just over half the cost ($340 million – cf. Trilling [40, p.40]). However, the scientific arguments advanced for CESR-II were virtually identical to those for the Stanford Linear Collider; and, in the opinion of the large majority of scientists interviewed in the course of our study, the latter had at least three major advantages; it was approximately a third the price of CESR-II; it provided a test-bed for the new technology of colliding linear accelerators – a development of great potential importance for the future of accelerator design (see below); and CESR-II required a new and thus far unproven technology (superconducting cavities) that may not yet be suitable for mass production. [20] In view of this last point, Cornell made it clear in 1982 that they would not submit a firm proposal until they had satisfactorily demonstrated the operation of superconducting radio-frequency cavities and that mass-production techniques for their fabrication had been established (cf. [40, p.39]). At that stage, Cornell expected to be in a position by 1984 to submit a proposal for commencing construction of CESR-II in Fiscal Year 1985. However, given the straitened financial circumstances confronting US high-energy physics, it was decided in 1983 not to proceed with CESR-II. Instead, funds were provided to upgrade the existing collider (in particular, to improve its luminosity) and to continue R&D on superconducting cavities for possible use in future machines.

3.4.4. 150–500 GeV VLEPP (Novosibirsk, USSR)

The Soviet high-energy physics centre at Novosibirsk in Siberia has, as we have noted elsewhere (Irvine and Martin [25]), developed into a world centre of excellence for electron–positron accelerator technology, even though the actual research output from the laboratory has not been significant in international terms. Their latest proposal is to construct a machine capable of colliding beams, each initially of 150 GeV energy, from two linear accelerators. At a later stage, the length of the linear accelerators could be increased to give beam-energies of up to 500 GeV (cf. [17, pp.417–18]). However, in view of the Soviet Union's existing commitment to the construction of UNK, it would seen doubtful whether substantial funding for VLEPP could be made available until the late 1980s. [21] Consequently, even if the project is eventually authorized, it is unlikely to be completed before the mid-1990s.

3.4.5. 1000 GeV Large Linear Collider (SLAC, USA)

Although the Stanford Linear Collider (see above) is still some way from completion, staff at SLAC have already begun to consider a scheme to extend current linear-accelerator technology to the 1000 GeV (1 TeV) energy range. No definite proposal has yet been prepared, but such a project was put before the US Subpanel on New Facilities in 1983. It was estimated that beam-energies of 1 TeV could be achieved for a cost of about $3 billion (cf. Wojcicki [44, p.37]). Given SLAC's current commitment to the early completion of the SLC and the fact that the necessary preproposal R&D will take at least five years, such a facility could probably not be completed until the latter part of the 1990s.

4. A framework for comparing the future prospects of new experimental high-energy physics facilities

Having looked in some detail at the accelerators currently planned or under construction around the world, we are now in a position to begin identifying the comparative advantages and disadvantages of LEP and its likely competitors. As noted earlier, our aim has been to develop a framework for systematically assessing, from an outsider's perspective, the future prospects of capital-intensive projects in basic science. The case for such assessments in high-energy physics has perhaps been reinforced by the evidence presented in the previous section concerning not only the

[20] It should be noted that CERN also considered employing superconducting radio-frequency cavities in the first phase of LEP, but decided against this because of the attendant technological risks: "Superconducting cavities are not yet sufficiently mastered. LEP Phase I must incorporate 'conventional' technology ..." [16, p.63].

[21] Strictly, the Novosibirsk Institute belongs to the USSR Academy of Sciences (Siberian Branch), while Serpukhov is an Institute of the State Committee for Atomic Energy, so VLEPP would be funded from a different source than UNK. Even so, it seems unlikely that the Soviet Union could afford to commit itself to providing the resources for two very large high-energy physics projects simultaneously.

very substantial costs involved (and indeed the possibility of failure as in the case of ISABELLE), but also the somewhat *ad hoc* way in which such facilities have tended to be planned, authorized and constructed, something which must be a source of some concern given the trend towards increasingly powerful institutional pressure groups discussed at the start of the paper.

In what follows, we apply a set of thirteen criteria to assess the future prospects for LEP in relation to those for other new experimental facilities. The criteria were identified on the basis of our studies of the factors determining the relative success and failure of high-energy physics accelerators (see Paper II [24]); they relate mainly to future scientific potential, but also take into account factors such as relative resource requirements and potential for the development of future research facilities. Table 5 evaluates the various new accelerators planned to come into operation over the next decade or so against these criteria. In addition, the recently completed proton–antiproton collider at CERN and the now discontinued ISABELLE and CESR-II projects have been included for comparative purposes. Each machine is assessed in terms of the various criteria on the basis of material and data discussed in the text below. As will be seen, this table provides a convenient means of summarizing the patterns of comparative advantage (indicated by + signs) and weakness (− signs) that exist among the various projects, patterns that can then be used in arriving at an overall assessment of their future prospects. We begin by reviewing the first set of four criteria relating to the relative construction requirements of these various accelerators.

4.1. Comparison of construction requirements

4.1.1. Financial criteria

Relative costs. In assessing the merits of different projects, relative cost is clearly one of the principal criteria. The estimated costs (in millions of Swiss francs or MSF) of the main accelerators and colliders under consideration were given in tables 1–4. Of these, the cheapest are the CERN proton–antiproton collider (200 MSF), the Stanford Linear Collider (240 MSF), and the Fermilab 1 TeV accelerator (about 250 MSF), their low costs reflecting the fact that they all make significant use of already existing facilities. In constrast, the 50 GeV first phase of LEP (910 MSF plus a further 250 to 280 MSF for four detectors) and ISABELLE (estimated to cost a total of some 1300 MSF at the time the project was discontinued, although there was no guarantee that this figure had ceased escalating) are both considerably more expensive. However, even their costs are dwarfed by that of the proposed US Superconducting Super Synchrotron and its associated detectors (some 6000–8000 MSF). These relative levels of cost are summarized in the first row of table 5.

Accessibility of resources. One of the major areas of uncertainty in assessing the future prospects for an accelerator is the likely funding position of rival facilities. This can determine not only whether competing accelerators are built, but also their date of completion – a factor which is often crucial in structuring success and failure. In the case of the first phase of TRISTAN, UNK, and HERA, the respective national governments have already authorized the necessary expenditure. For the Fermilab 1 TeV accelerator and collider, the Stanford Linear Collider, and the first phase of LEP, [22] the resources have been promised, but for all these projects there is little leeway for cost over-runs (apart from extending the construction period). These conclusions on the probable accessibility to financial resources are summarized in the second row of table 5.

4.1.2. Technical criteria

Degree of technical difficulty. As accelerator construction has increasingly become a sophisticated high-technology activity, so the technical difficulty of constructing and operating such a facility has become a more important factor in determining its likely future success. This is particularly true for very advanced technologies like superconductivity. Recognizing the difficulties involved, Fermilab adopted a relatively cautious approach to the design and construction of the superconducting magnets required for its 1 TeV accelerator. Even then, the task proved far from simple and it was only in 1980, after a long development process, that they were able to begin manufacturing mag-

[22] Once this first phase of LEP has been completed, CERN will in all likelihood be able to convince the Member States that, rather than decreasing the annual budget to the "base-load level", any free funds should be used to increase LEP's energy to 90 GeV and later to 130 GeV.

Table 5
Comparison of major accelerator projects: Summary of main relative comparative advantages (+) and disadvantages (−)

			CERN $p\bar{p}$	Fermilab 1 TeV	Fermilab $p\bar{p}$	KEK TRISTAN	SLAC SLC-50	Cornell CESR-II [a]
		Estimated date of first experiments	1981	1985	1986	1986	1987	1988?
Construction requirements	Financial	Cheapness	+ +	+ +	+	−	+ +	−
		Accessibility of resources	+ + +	+	+	+ +	+	− − −
	Technical	Technology required relatively simple/undemanding	− −	−	−	+	− −	−
		Technical track-record of laboratory	+ +	−	−	.	+	.
Scientific potential	World lead in a new energy region	Relative increase in centre-of-mass energy	+ + +	.	+ +	.	+	−
		Period of world leadership	+ +	+ +	+ + +	.	.	−
	Other factors governing ability to generate physics results	Event rate	− −	+ +	− −	.	−	.
		Number of experimental areas	−	+ +	−	.	− −	.
		Variety of experiments possible	.	+ +	.	−	− −	−
		"Cleanness" (ease of interpretation) of data	−	.	−	+ +	+ +	+ +
	Ability of users to exploit accelerators	Scientific track record of laboratory users	+	.	.	−	+ +	.
Future potential		Potential spin-off to accelerator physics	+	+	.	.	+ +	+
		Flexibility/potential for future development of accelerator	−	+	−	+	.	.

[a] Project discontinued in 1983.
[b] This assumes that a superconducting proton collider is not completed at CERN ahead of this US machine.
[c] '.' signifies no particular comparative advantage or disadvantage.

nets on a mass-production basis. At Brookhaven, where the ISABELLE project required magnets with significantly higher field-strengths, the problems were far more severe, and were largely responsible for the delays to the facility's construction programme, [23] and hence ultimately for the project's cancellation. The technical difficulties faced by the CERN accelerator builders in attempting to achieve a luminosity of 10^{30} cm^{-2} s^{-1} in the proton–antiproton collider have also been extreme, the machine initially operating with a peak luminosity a factor of 200 below the design figure, although this was rapidly improved by one and a half orders of magnitude over the following two years. The builders of the Fermilab collider will enjoy the considerable advantage of being able to benefit from the experiences at CERN, making their task markedly simpler. In the same way, Serpukhov in their collaborative work with Saclay on the superconducting magnets for UNK will be able to profit from Fermilab experience with the magnets for the 1 TeV fixed-target accelerator, as indeed will the builders of HERA. [24]

[23] As noted earlier, one of the factors contributing to this delay was the fact that the original design for the ISABELLE magnets had eventually to be scrapped after a long period of unsatisfactory development and testing. Part of the problem may have stemmed from the Brookhaven design being so different from that adopted at other high-energy physics centres that the laboratory was unable to profit from the results of work elsewhere (cf. [11, p.146]).

[24] The proposed magnet designs for both UNK and HERA are reportedly similar to that adopted at Fermilab (cf. [11, p.147] and [13, p.206]).

Table 5 (contd.)

			CERN LEP-50	Brookhaven ISABELLE [a]	Serpukhov UNK	DESY HERA	KEK ep	CERN LEP-90/130	US SSC
		Estimated date of first experiments	1988	1987/8?	1990?	1990?	1990?	early 1990s?	mid-1990s?
Construction requirements	Financial	Cheapness	– –	– –	+	. [c]	.	–	– – –
		Accessibility of resources	+	– – –	+ +	+ +	.	+	.
	Technical	Technology required relatively simple/undemanding	+ +	– –	.	.	.	+	–
		Technical track-record of laboratory	+ +	– –	– –	.	.	+ +	?
Scientific potential	World lead in a new energy region	Relative increase in centre-of-mass energy	–	– –	+	+ + +	– –	+	+ + + [b]
		Period of world leadership	+	– –	+ +	+ +	– –	+ +	+ +
	Other factors governing ability to generate physics results	Event rate	.	+	+ +	.	.	–	+
		Number of experimental areas	.	.	+ +	.	.	+	?
		Variety of experiments possible	–	+	+ +	+	+	–	+
		"Cleanness" (ease of interpretation) of data	+ +	–	.	+	+	+ +	–
	Ability of usersto exploit accelerators	Scientific track record of laboratory users	+	+ +	– –	.	–	+	?
Future potential		Potential spin-off to accelerator physics	.	+	.	.	.	+	+
		Flexibility/potential for future development of accelerator	+ +	.	+	.	.	+	?

In the case of electron–positron colliders, the technical problems associated with the SLC are felt by most physicists to be particularly severe, and it will be a major engineering feat to achieve a luminosity of 6×10^{30} cm^{-2} s^{-1} with the new and untested technique of the linear collider. In contrast, the task facing the builders of TRISTAN and LEP seems more simple. For the latter, one of the main problems – that of producing low-field magnets relatively cheaply – has already been largely solved (cf. [8, pp. 8–9]), and the difficulties anticipated in tunnelling under the nearby Jura mountains have also been considerably diminished by various changes in the size and location of the accelerator ring. Finally, although there may be inherent problems in reaching the design luminosity with a circular collider like LEP, [25] CERN will be able to profit from the wealth of experience gained over a ten-year period from smaller elec-

[25] In 1981, it was reported that "CESR, PEP and PETRA are all having great difficulty climbing anywhere near their design luminosity" [11, p.144]. One commentator concluded somewhat pessimistically that this "low luminosity also cast a shadow on the performance prospects of larger colliding beam machines still on the drawing board" (Robinson [33, p.1488]). There are perhaps two underlying causes to the disappointing results so far. One is that calculations of the design luminosity for the newly completed colliders were based on extrapolations from a previous generation of machines, and these extrapolations have subsequently proved to be invalid. (It is therefore somewhat alarming that "Published CERN plans for [LEP] still use the old tune-shift value in calculating the expected luminosity" [33, p.1490] rather than the values actually obtained at PETRA and PEP.) The other factor is that, in the rush to produce high-energy physics results as quickly as possible, those responsible for the operation and development of electron–positron colliders have given too little attention to the machine physics involved (cf. [11, p.144]).

tron–positron colliders at DESY, Cornell, and Stanford. (If and when LEP's energy is increased to 130 GeV, superconducting cavities will be required, but by then the technical uncertainty associated with their production and use should be fairly modest, and CERN will be able to draw on the many years of work currently being undertaken at Cornell, Karlsruhe, KEK, and elsewhere.) These comments on the relative technical difficulty associated with the various accelerator projects are again reflected in the relevant row of table 5.

Technical track-record of the laboratory. One of the best indicators of the likelihood of overcoming the technical diffculties associated with designing, building, and operating a new facility is the previous record of the accelerator-builders concerned. CERN has perhaps had the best technical record of the major accelerator centres over the last decade with the successful commissioning of the ISR [26] and the SPS, [27] a fact widely acknowledged by the physicists we interviewed in both Europe and the USA. In view of this, it was perhaps not unreasonable to assume when CERN embarked upon the construction of the proton–antiproton collider that the very considerable technical problems would eventually be solved, and this assumption has been fully justified by subsequent events. Similarly, the technology required for LEP should prove easily within CERN's competence, although, if the tunnelling encounters any major difficulties, the tightness of the budget may result in the date of the first experiments being postponed.

SLAC also possesses an excellent track-record with the linear accelerator and particularly with SPEAR, which was built under very adverse financial constraints (funds had to be found from within the laboratory's existing budget after the project application had been turned down by the US Department of Energy). The more recent experience with PEP, however, was markedly less successful, principally because of delays caused by outside civil-engineering contractors, a problem that CERN may face in connection with the LEP tunnel. However, on balance, SLAC's record suggests that it will probably be able to solve the problems associated with the SLC and complete the facility by 1987 as planned. In recent years, DESY has had almost as strong a technical track-record as SLAC. It has grown into a major international laboratory, and with this has come the breadth of technical and engineering expertise that will probably ensure it is able to cope successfully with the demands of HERA even though this project is rather more ambitious than previous construction programmes. Fermilab, in contrast, has a somewhat patchy technical record with its 400 GeV accelerator, as was seen in Paper II [24]. Nevertheless, there are signs that the laboratory has since taken note of the lessons learnt, particularly in magnet construction, and neither the 1 TeV accelerator nor the proton–antiproton collider appear beyond its technical capabilities.

One cannot be quite so sanguine about the prospects for UNK. As we have discussed elsewhere (Irvine and Martin [25]), Serpukhov does not have a particularly strong record for building front-line research facilities, probably for reasons more to do with the structure of East European science than with the ability of individual accelerator physicists. Although the 70 GeV accelerator first operated in 1967, it was several years before a full experimental programme was mounted, and, even then great difficulty was encountered in providing the sophisticated detectors and powerful computing facilities needed to progress beyond the simplest "first generation" experiments. Perhaps because of the limited access of East European high-energy physicists to the services of high-technology industry, it has been decided that the superconducting magnets for UNK will be built at Serpukhov itself (cf. [11, p.147]). While this may give the laboratory greater control over the construction of the accelerator than might otherwise have been the case, it is still difficult to be optimistic that all the problems encountered in Western attempts to mass-produce such magnets will be quickly and efficiently overcome by the Soviet team, even with the aid of their French collaborators from Saclay.

Similar reservations applied to Brookhaven with respect to the ISABELLE project while it was still proceeding. Although Brookhaven had a good record with the AGS two decades ago, the AGS

[26] See the quotation in section 8 of Paper II [24] on the ISR being "widely regarded as the most perfect example to date [1979] of the accelerator builder's art."

[27] In Paper II [24], it was seen how the SPS's technical superiority over the Fermilab accelerator was one of the main factors explaining the differences between the scientific performance of the two machines in the late 1970s and early 1980s.

Conversion Programme undertaken ten years later experienced severe problems (see Paper II [24]), and the record of the laboratory with developing superconducting magnets between 1976 and 1980 can only be described as poor. As has been pointed out, when the proposal for ISABELLE was being formulated, the machine had no obvious

> competitor in its particular regime of physics, and [the project] had time for R&D – more time, due to stringent funding of high-energy physics, than [Brookhaven] was happy with (Metz [31, p.188]).

There must therefore be considerable doubts about the wisdom of the laboratory in embarking upon the construction of the new facility before all the necessary R&D work had been completed, [28] and in continuing a commitment to an inadequate magnet design for several years after its limitations became apparent to those inside the laboratory. Although changes in the senior management at Brookhaven during 1981 and 1982 led to some technological revitalization of the project, [29] they did not completely remove what was probably the root cause of Brookhaven's relatively poor technical record during the 1970s – the failure to recruit capable young staff, especially accelerator physicists and engineers (cf. [6, p.247]) – which was in turn brought about by the absence over a longer period of any major new accelerator project.

4.2. Comparison of scientific potential

The scientific justifications made for all the new high-energy facilities of the 1980s are remarkably similar, and exhibit a degree of convergence of theory and experimental practice seldom previously witnessed in the subject. Analysis of the proposals for each of the accelerators listed in table 5 (and of subsequent review papers and reports summarizing the scientific case for each facility) reveals a common emphasis on the study of the intermediate vector bosons, the search for the Higgs particle and for new leptons and quarks, investigation of the quark–gluon picture and the theory of quantum chromodynamics, the exploration of possible "grand unified theories", and the discovery of unexpected phenomena. How, then, can one attempt to draw distinctions between the scientific potentials of these various accelerators? What criteria can be used?

The history of experimental high-energy physics over the last thirty years shows that two of the main factors determining the scientific potential of an accelerator are whether it opens up a new, unexplored energy-region (i.e. whether it has a significant advantage in terms of increased centre-of-mass energy over other machines), and the length of time it enjoys a position of world leadership for that particular type of accelerator. As was seen in Paper II [24], this was certainly a crucial component in the success of proton machines such as the Berkeley Bevatron, the Brookhaven AGS (with its small but significant advantage in energy over the CERN PS), and the Fermilab accelerator. This pattern is also reflected both in the case of electron machines, notably in the output of the Stanford linear accelerator, and among electron-positron colliders with the success of first SPEAR and then PETRA (see footnote 33 below).

A second set of factors, and one which has become more apparent in recent years, relates to the varying potentials of different types of accelerator for generating new physics results. Several factors are of relevance here: the event-rate (which determines the amount of experimental data that can be collected in a given period), the number of experimental areas or interaction regions (which determines the number of experiments that can be undertaken at any one time), the variety of experiments possible on the accelerator, and the degree of "cleanness" of the experimental data produced (i.e. the ease with which they can be interpreted).

Last, and of great importance, is the ability of the user-group associated with the accelerator to exploit its full potential. As we have argued elsewhere (e.g. Martin and Irvine [28]), one of the few indicators of that ability is their recent track-record, and therefore this criterion has also been included in table 5.

Let us examine in turn each of these criteria relating to the scientific potential of the various accelerators under consideration.

[28] In the attitude survey described in Paper II [24], 74 percent of those interviewed agreed with the statement, "It would have been better if Brookhaven had first spent a few years ascertaining whether superconducting magnets were technologically feasible before investing considerable resources in the construction of ISABELLE", well over three times the number who disagreed (22 percent).

[29] Prototype magnets of the necessary field-strength were successfully developed in 1982, although at the time the project was cancelled it still had to be demonstrated that these were suitable for mass-production (cf. Wojcicki [44, p.30]).

4.2.1. World-lead in a new energy region

Relative increase in centre-of-mass energy. Taking first the proton machines, one can see from table 2 that the centre-of-mass energy achievable with the CERN proton–antiproton collider is 540 GeV. This represents almost a nine-fold increase over that attained on the previous highest-energy proton collider, the CERN ISR – an unusually large jump in energy that has, in the past, not been surpassed by any other accelerator (the ISR came closest with a five-fold increase in centre-of-mass energy over Serpukhov). As for the rival Fermilab proton–antiproton collider, its energy advantage over the CERN facility will correspond to a factor of just over three and a half (2000 GeV compared with 540 GeV), again quite large in terms of previous experience. In contrast, the Fermilab 1 TeV accelerator will reach a centre-of-mass energy less than 50 percent higher than the maximum achieved by the current generation of large proton synchrotrons, while UNK will in turn eventually possess an advantage of about 75 percent over the Fermilab accelerator. Finally, it is worth commenting that ISABELLE, if completed, would have had a centre-of-mass energy very much lower than the collider at Fermilab, although (as is noted below) it would have had the advantage of a higher event-rate.

As for the new electron machines, while TRISTAN's energy will be only 30 percent greater than that of the upgraded PETRA (see table 4, note b), the Stanford Linear Collider will achieve an energy 70 percent greater than TRISTAN – an increase which, as we have seen, could be crucial because it will permit detailed studies to be carried out of the mass, width, and decay-modes of the Z_0 particle. If the SLC is completed in 1987 as planned, then LEP will not have any energy advantage when it begins operating a year later, though it will have other relative strengths (see below). However, if the beam-energy of LEP is eventually increased to 130 GeV, this will give the machine a two-fold energy advantage over other electron–positron colliders – in particular the SLC, even if the latter's energy is subsequently increased to 70 GeV. Finally, the DESY electron–proton collider, HERA, will achieve a centre-of-mass energy over forty times greater than previously achieved in electron–proton collisions – 315 GeV compared with less than 7 GeV at the Stanford linear accelerator – so, like the CERN proton–antiproton collider, it is highly ranked in terms of this criterion in table 5.

Period of world leadership. The penultimate columns of tables 1–4 contain estimates of the number of years each accelerator is likely to produce the highest-energy interactions for that particular type of facility. (It should perhaps be stressed that, particularly for those machines still in the planning stage, these are very approximate estimates only.) The relevant row in table 5 shows which facilities are likely to enjoy a relatively long period of world-leadership, and which are not. Thus the rating reflects the fact that, for example, the CERN proton-antiproton collider is likely to enjoy a lead of some five years over its Fermilab rival, while it may be up to ten years before the latter is overtaken by a yet higher-energy proton collider (perhaps one placed alongside the electron–positron ring in the LEP tunnel or the SSC). The Fermilab 1 TeV accelerator, UNK, HERA, and the final phase of LEP (LEP-130), are all given reasonably positive evaluations due to the fact that they should enjoy a world lead of about five years (± two years). This is considerably more than the TRISTAN and SLC machines, both of which are likely to be overtaken by more powerful electron–positron colliders within a year or so. Finally, it should be stressed that the ill-fated ISABELLE project again ranked relatively low in terms of this criterion because it would have come into operation with a marked energy disadvantage relative to other machines as a result of the delays to the construction programme discussed earlier.

4.2.2. Other factors governing ability of accelerator to generate new physics results

Event-rate. The gradings of the various accelerators listed in table 5 in terms of this criterion reflect substantial differences among accelerators in the event-rates they are able to generate. The fixed-target machines (such as the Fermilab 1 TeV accelerator and UNK) have by far the highest event-rates, followed by proton–proton colliders (the proposed SSC is likely to have a design luminosity of approximately 10^{33} cm^{-2} s^{-1}) and electron–positron storage-rings like TRISTAN and LEP (with design luminosities in the region of

$10^{31}-10^{32}$ cm^{-2} s^{-1}).[30] The event-rates of the proton-antiproton colliders at CERN and Fermilab are appreciably lower (of the order of 10^{30} cm^{-2} s^{-1}), with the result that experiments carried out on these machines take correspondingly longer, and consequently there are fewer of them. Conversely, proton machines can be used to make relatively quick scans of large energy regions.

Number of experimental areas. Fixed-target accelerators like the Fermilab 1 TeV and UNK also possess a marked advantage over other sorts of experimental facility in that several beams can be obtained simultaneously. As a result, large numbers of experiments can be run concurrently – far more than are possible on a collider where the number of interaction areas is strictly limited. In the final phase of LEP, for example, eight experimental areas are planned, while in the first phase (LEP-50) there will be only four. At the CERN and Fermilab proton-antiproton colliders, the number of experiments is even more constrained, with just two intersection regions, while at the Stanford Linear Collider there will probably be only one collision point, at least initially. [31]

Variety of experiments. As is highlighted in table 5, fixed-target accelerators possess yet another important advantage over colliders; because they can be used to produce a wide range of secondary beams (of kaons, neutrinos, etc.), the variety of different types of experiments – or what is often termed the "physics menu" – is much broader than for electron-positron colliders (and especially so in the case of the SLC with its relatively low luminosity [32]). This has a number of important policy implications which should perhaps be briefly mentioned at this point, though they are treated in more detail in the conclusions. The first is that, with electron-positron colliders, there is a crucial advantage to be gained by being first into a new energy region; [33] a lead of just one or two years for such a machine can be more important than a head-start of three or four years for a fixed-target accelerator where it takes considerably longer to exhaust the range of experimental possibilities. Hence, the fact that the SLC is due to reach the 100 GeV centre-of-mass region a full year or so ahead of LEP could prove a major handicap during this first phase of the CERN machine. A second policy implication has been described in the following terms in relation to LEP:

> There is a natural tendency when investigating a new area of physics to be the "firstest with the mostest." Since it is possible to define all the characteristics we would like to know about the products of electron-positron collisions, this tendency could lead to all experimental proposals being built around nearly identical "universal" detectors. [14, p.241]

However, CERN management seem to be aware that such a tendency, if not checked, could leave LEP particularly vulnerable to a sudden dramatic change in physics interest, since they have urged that the differences between the various detectors proposed for LEP be made rather greater than first seemed likely (cf. Robinson [38, p.722]). Even so, the variety of experiments that can be handled on an electron-positron machine like LEP is appreciably less than that possible on some of the other new accelerators planned to come into operation in the next ten years.

"Cleanness" of data. Where electron-positron colliders do score highly over other types of accelerator is in terms of the ease with which the resulting experimental data can be interpreted, this facet of their performance being strongly reflected in table 5. The reason for this is as follows: collisions between electrons and positrons involve an interaction between two apparently point-like objects, each with precisely known energies. When these particles collide, total annihilation takes place, producing "pure" energy which is then available for the creation of new particles that have a known

[30] In the case of LEP-90, the design luminosity is 10^{32} cm^{-2} s^{-1}. The luminosity in the earlier phase (LEP-50) is expected to be 3×10^{31} cm^{-2} s^{-1}, giving it a probable advantage in this respect of about 5 over the similar energy Stanford Linear Collider (SLC-50).

[31] It is envisaged that this may subsequently be increased to two.

[32] The SLC will, however, have the compensating advantage that it is uniquely suitable for polarized beam experiments (cf. [12, p.201]).

[33] This was particularly true in respect of the Stanford collider, SPEAR (a year ahead of DORIS at DESY), and later, when the positions were reversed, with PETRA coming into operation two years ahead of PEP. The rapid obsolescence of electron and electron-positron machines is further discussed in Martin and Irvine [27].

total energy. Hence, by successively "tuning" the energy of the collided electrons and positrons to a series of different values, a particular energy region can be comprehensively scanned – if any new particle does exist in the energy-range under study, then it should be produced and thus be observed. [34] In contrast, the collision between two protons (or between a proton and antiproton) is a much more complex affair, [35] since each particle (according to the currently accepted "standard model") consists of three quarks and a number of gluons. In the resulting multi-body interaction, only a fraction of the collision energy is available for the production of new particles; and since the energies of the individual constituent quarks are unknown, it is impossible to "tune" the experiment to investigate a particular energy state. Instead, a whole spectrum of particle interactions of different energies takes place, and the experimentalist has to sift painstakingly through all of these to find the collisions of interest; in other words, to use the jargon of physicists, the events are "dirty" (cf. Robinson [35, p.528]). Hence, despite the development by high-energy physicists of sophisticated computer programs to perform such analyses, and the enormous growth in recent years of available computing power, one can never be entirely sure that nothing has been "missed" in a given energy region. [36] Last, HERA lies somewhere in between electron–positron colliders on the one hand, and proton machines on the other, since it involves using electrons – i.e. apparently point-like objects with reasonably well known properties – as a probe to investigate the structure and properties of the multi-body proton.

4.2.3. Ability of users to exploit accelerator

As noted earlier, the track-record of the user-community associated with a laboratory is a crucial variable in determining whether the scientific potential of a new accelerator is likely to be achieved. The data presented on this subject in table 5 reflect the differences in scientific performance identified in Paper II [24] for the world's principal accelerators over the last two decades. Thus, particularly high credit must be given to the user-communities associated with Brookhaven (the AGS) and Stanford (the linear accelerator and SPEAR), which were judged to have the most successful records in world terms – each was responsible for a comparatively large number of crucial discoveries, as well as a significant volume of lower-level advances. In addition, the successes of the proton–antiproton collider in 1983 have clearly strengthened the record of CERN users. At the other end of the scale, Serpukhov had the least distinguished record of all the major laboratories currently engaged in a major new accelerator project (see Irvine and Martin [25]). However, these data on past performance cannot legitimately be used in prospective evaluation without a number of qualifications first being stated. In the case of Brookhaven, for example, it should be noted that the main achievements of the AGS were made in the 1960s by a group of predominantly US East Coast experimenters, while the user-community for ISABELLE (if it had finally been completed in 1987 or 1988) would have been very different. A similar consideration may apply to Stanford, though probably not to the same extent. In the heyday of SPEAR, most of its principal users were employed either at the laboratory itself or at the Lawrence Berkeley Laboratory. Since then, the Stanford user-community has been considerably broadened, [37] a trend that seems likely to continue. In general, the future will probably witness even greater cross-usage of accelerators than in the past because the growing cost of new experimental facilities means that there will be less duplication of accelerators between continents. So, for exam-

[34] This largely explains the tremendous success of SPEAR in the mid-1970s in exploring the range of "charmonium" particles, and, more recently, of CESR (at Cornell) in investigating the upsilon and related particles.

[35] In Paper II [24], we saw that this was one of the factors limiting the relative scientific performance of the CERN ISR.

[36] It is precisely because of the enormous problems with data-handling in proton–proton physics that critics of ISABELLE argued that its luminosity – the only remaining advantage it had over rival facilities – was only of marginal importance.

[37] This was a strategic decision taken in the latter part of the 1970s by the SLAC management, who became increasingly concerned that, in a period of limited funds, the scientific case for the laboratory's future support might be swamped by the political strength of the wider body of university experimentalists whose research activities were concentrated around the more extensive fixed-target programmes of the two national laboratories at Brookhaven and Fermilab.

ple, large numbers of European physicists (perhaps 400 or so – see Robinson, [39, p.814]) are set to use the new Fermilab machines, especially the proton–antiproton collider, [38] once these come into operation as the highest-energy proton facilities in the world. Similarly, when LEP takes over the mantle as a world's most powerful electron–positron storage-ring, there will be a major movement of American experimentalists to CERN, [39] while LEP and HERA will be drawing users from essentially the same community of West European experimentalists (which may give rise to an element of competition between them). Such migrations of physicists between centres are, therefore, likely to go at least some way towards smoothing out the differences between the ability of traditional user-groups to exploit their experimental facilities, although the users of LEP will still be predominantly West European (perhaps 75 percent), those of the Stanford Linear Collider mainly West Coast Americans, those of UNK primarily East European, and so on. Therefore, track-record is still likely to constitute an important factor in predicting future research performance.

4.3. Comparison of future potential

Finally, we come to the last set of factors listed in table 5 that should be taken into account in assessing the future prospects of a major accelerator project. These concern, first, the likely spin-off from a new machine to accelerator physics more generally – for example, whether a radically innovative technique for collisions is being pioneered that may usher in a new generation of research facilities; and, second, the "flexibility" of the machine – that is, its potential for future development (such as adding a new ring to generate further experimental possibilities) or for deployment in other scientific specialties. Some of the physicists interviewed particularly stressed these factors, arguing that, if a particular new energy-region should prove barren, it is important to be able to make alternative use of what are highly expensive capital facilities.

Potential spin-off to accelerator physics. Of the various accelerator projects considered here, the Stanford Linear Collider probably has the greatest potential in terms of spin-off, since it provides an opportunity for testing the feasibility of colliding linear accelerators. This new accelerator technology is important because it now seems likely that LEP will be the last of the large circular electron–positron colliders. Not only would a larger collider than LEP be prohibitively expensive to construct (the cost of such machines rises approximately with the square of its energy), but the power consumption would be extremely high [40] (to overcome the energy losses through synchrotron radiation). Furthermore, the beams would be subject to a phenomenon know as beamstrahlung, [41] which would seriously limit the accelerator's performance. To achieve electron–positron collisions at centre-of-mass energies greater than 350 GeV or so, the most promising way forward would appear to lie with colliding linear accelerators (the cost of which rises only linearly with energy). According to many of the physicists we interviewed, the SLC will provide an important test of whether this new and potentially important technique is feasible, [42] and it is therefore rated highly in terms of this criterion in table 5.

As for the other new machines, the CERN proton–antiproton collider has been technologically very innovative in that it has involved developing the technique of "stochastic cooling", which has the potential for extensive future applications. The main spin-off from the Fermilab 1 TeV accelerator (and from the ISABELLE project while it was still being funded) has been the technology and expertise required in the mass-production of superconducting magnets. Finally, the last phase

[38] There has been some discussion, for example, about the possibility of moving the very large UA1 detector from the CERN collider to the larger US facility when it nears completion in 1985.

[39] Of the four LEP collaborations given the provisional go-ahead in 1982 by CERN, one is largely American (cf. Robinson [38, p.722]).

[40] Indeed, it will take over 250 MW of electricity to power LEP at 90 GeV, the equivalent energy consumption of a medium-sized city. It was for this reason that a senior American accelerator physicist, intimately involved with the development of the SLC, referred to LEP in an interview as "the last of the dinosaurs."

[41] This is the radiation phenomenon arising from high-field effects as bunches of particles pass through one another at very high energies.

[42] It is significant that the SLC is categorized as a "research and development project," although it will have "a timely physics payoff as well" (Wojcicki [44, p.25]).

of LEP (LEP-130) [43] requires the development of superconducting radio-frequency cavities, as would CESR-II had it been built. However, in none of these cases is the magnitude of the potential spin-off as great as for the Stanford Linear Collider.

Flexibility. The variety of possible options for future experimental facilities is probably greatest in the case of the first phase of LEP (LEP-50). As we saw earlier, once the later phases of the accelerator have been completed (LEP-90 and LEP-130), the possibility exists for placing a proton synchrotron in the same tunnel. [44] (The width of the LEP tunnel has been made sufficiently large to cater for such future developments, thus minimizing the civil engineering costs associated with any major new project of this sort.) As we have already noted, by using superconducting magnets, a proton synchrotron with an energy of up to 10 TeV [45] could be built on the CERN site. This accelerator could in turn be converted into a large proton–antiproton collider and be used, in conjunction with LEP, to collide 130 GeV electrons and 10 TeV protons. This potential for future development was referred to by many of the CERN physicists interviewed in 1981 as the "real-estate" argument for LEP – that, regardless of the scientific potential of the electron–positron collider, the existence of the costly 27-kilometre tunnel will guarantee the long-term scientific future for CERN (and West European high-energy physics) by virtue of the wide range of new accelerator developments that can be accommodated at the Geneva site. However, it is significant that, since these interviews were conducted, US high-energy physicists have put forward plans for the SSC (a 20 + 20 TeV proton collider). In 1983, the High Energy Physics Advisory Panel unanimously endorsed the recommendation of their Subpanel on New Facilities that this project should be given "the highest priority" in the US programme in order to achieve "completion in the first half of the 1990s" (Wojcicki [44, p.4]). If this target is met, the SSC would come into operation at about the same time as the earliest a proton machine could be completed in the LEP tunnel. Since the American facility would be capable of achieving energies up to twice those of the CERN machine, then, given the enormous cost of these machines, the arguments in favour of proceeding with the construction of such a CERN accelerator would be very substantially reduced. In short, the "real-estate" justification for LEP will lose a great deal of its force if construction of the SSC proceeds apace.

As was shown earlier in the right-hand column of table 1, the new proton synchrotrons at Fermilab and Serpukhov also provide scope for several possible future options, although not quite as many as with LEP. With the proton–antiproton colliders at CERN and Fermilab, however, the possibilities for future development are intrinsically more limited. [46] As before, these comparative advantages and disadvantages of the various new facilities have been summarized in table 5.

5. Overall assessment of the future prospects for CERN and its users

Having evaluated in detail the prospects for the various major new research facilities around the world, we are now in a position to come to some more general conclusions about the future for CERN and indeed for West European high-energy physics as a whole. In the short term, CERN's scientific prospects rest primarily upon the continued exploitation of the SPS and the proton–antiproton collider. To take the first of these, the SPS user-community has, since the late 1970s, enjoyed a range of advantages over the experimenters at Fermilab; these include a more reliable accelerator, higher-quality beams, technically more

[43] As was noted earlier, the construction of the first phase of LEP (LEP-50) will involve relatively conventional and well-established technology, with the result that the spin-off to accelerator physics is unlikely to be particularly significant.

[44] As Wilson [43, p.37] has observed, "this potentiality has not yet been publicly mentioned by the proponents of [LEP]", a situation that only began to change in 1984.

[45] The maximum energy would depend on the field-strengths achievable with superconducting magnets in the 1990s. 10 tesla magnets would be required to accelerate protons to an energy of 10 TeV in the LEP tunnel (cf. Wojcicki [44, p.26]).

[46] It should be noted, however, that the development of the CERN collider has paved the way for the low-energy antiproton ring (LEAR) facility. In addition, some thought has been given to the possibility of increasing the energy of the collider. However, this could only be achieved at the expense of substantially reducing the available luminosity, and, as we have seen, this is still rather low for certain types of important experiments.

sophisticated detectors, and a higher level of financial support (including longer operating time – at Fermilab this was drastically curtailed during the early 1980s in order to limit the laboratory's electricity costs). However, now that the new Fermilab fixed-target machine has started to reach energies in excess of 500 GeV, the CERN SPS will gradually lose its position of world leadership for this type of accelerator, and its relative scientific output is likely to be affected accordingly.

As for the proton–antiproton collider, its advantages and disadvantages are well summarized in table 5. When CERN took the decision to embark on this project, the technical problems seemed formidable, and indeed there was an element of risk that the design luminosity would prove unattainable. Balancing this, however, was the fact that CERN had in the past two decades developed probably the greatest body of accelerator expertise among all the major world laboratories. As the project manager for one of the main US accelerator programmes remarked,

> On balance, I suppose our accelerator construction team ranks with CERN's first team, but they have second, third and fourth teams as well ... no-one can match that range of expertise. [Interview, 1981]

Given that CERN had acquired a record second to none in building and commissioning new accelerators, there were good grounds for optimism when the project began that the problems associated with the luminosity of the proton–antiproton collider would eventually be overcome, particularly when the likely lead over the Fermilab collider was stretched to some five years. While the CERN collider's relatively low event-rate might in principle be expected to restrict the range of physics that it can carry out, the big jump in energy it represents, and its comparatively large lead over the rival collider at Fermilab, should ensure that a fairly wide range of experiments is completed during the period up to 1986. Moreover, the CERN collider has proved relatively cheap in terms of the investment now required for a typical major new experimental facility, even if it has been built at the expense of small reductions in the reliability of the SPS and the time available for the fixed-target programme. This combination of relatively large scientific potential and low cost would suggest that, even though a high degree of technological uncertainty was involved, CERN chose wisely in 1978 when embarking upon this project – a conclusion shared by the great majority (93 percent) of the physicists whom we interviewed in 1981 and 1982.

In contrast, the comparative advantages and disadvantages of LEP are almost the exact opposite of these associated with the proton–antiproton collider, as is apparent from the pattern of strengths and weaknesses summarized in table 5. First, the technology required is relatively conservative and simple, apart from the tunnelling where civil engineering problems might possibly give rise to some delays. On the other hand. LEP has good potential for future development.

Second, the LEP project is extremely expensive compared with most new accelerators, and considerable criticism of the cost-effectiveness of the project was voiced by certain of the American physicists whom we interviewed (although some of those same physicists are now advocating the construction of the SSC costing many times more than LEP). The CERN Member States have agreed that they can find the funds, but only at the expense of substantial cutbacks in the laboratory's other research programmes – cutbacks of a size and significance not readily forseeable in 1977 when the European Committee for Future Accelerators first recommended that LEP should be CERN's next major machine. In particular, the Intersecting Storage Rings were closed at the end of 1983 – a major sacrifice given that the cancellation of ISABELLE meant this would have remained a unique accelerator until the mid-1990s and, moreover, one with relatively low operating costs. [47] Indeed, the evidence from interviews with physicists and from the bibliometric indicators presented in Paper II ([24] – see table 6 in particular) suggests that much worthwhile experimental work may still have remained to be done on the ISR.

Third, and last, whereas the proton–antiproton collider represents a significant increase in centre-of-mass energy, and one which will present its users with a position of monopoly in the energy region for perhaps five years, the first phase of LEP will achieve an energy no greater than that likely to be attained just over a year earlier by the

[47] There would have been a strong case for eventually developing the ISR as a heavy-ion collider, which would have produced another unique facility for a relatively small investment.

Stanford Linear Collider. True, LEP will have a certain advantage over the SLC in terms of luminosity, but it is far from clear how significant that advantage will prove. If the SLC reaches its design luminosity of 6×10^{30} cm^{-2} s^{-1}, it will produce over a million Z_0 events per year: in other words, the SLC should be able to carry out detailed studies (with reasonable statistics) of the production and decay-modes of this crucial particle (cf. [12, p.200]). This latter point has not always been recognized or admitted by the keenest supporters of LEP, as this CERN physicist's comments make clear:

> The local 'party line' at CERN is to doubt the SLC. They hope it won't work at all. But if it works within a factor of 10 or even 20 of the promised luminosity, and works in 1987, it will get a few thousand Z_0 decays. This will be a big psychological burden for LEP. Some people on the LEP project won't face up to this. The story then follows typically that, at least LEP will be able to do precise measurements. Then people start talking about Phase II. But that costs a lot of money, and will have only a very low event rate ... So the usual CERN view is to say that the SLC won't work. Also one of the views on the LEP tunnel, reconciling it, is the 'real estate' view – the possibility of putting a proton accelerator and a proton–antiproton collider down there. This softens the view of the impact of the SLC as well. ...I guess if I'm rational, I'd take measures to stop the crazy international competition. It has been terrible for SLAC, losing the race with PETRA. If LEP is loser [in the competition with the SLC], this will be terrible for Europe. CERN *must* win the competition. But I'm pessimistic – the SLAC machine will probably just spoil LEP. [Interview, 1981]

As has been noted earlier, electron–positron colliders have a rather limited "physics menu" and therefore exhibit a tendency towards rapid obsolescence. Consequently, a lead of just one year for the SLC could prove extremely serious for CERN in that the research programme for LEP could consist very largely of repeating SLC experiments is a more detailed form. In other words, LEP might be restricted to the same sort of role as the CERN SPS performed in relation to the Fermilab 400 GeV accelerator. While such tasks as confirming discoveries, clearing up anomalies, and repeating experiments in order to obtain better statistics are clearly important, such work tends to have less impact than actually making a new discovery or carrying out the first measurement of a particular particle property. Thus, it is difficult for the outside observer to avoid the conclusion that the scientific potential of the first phase of LEP is likely to be limited by the SLC if the latter is completed on schedule and achieves a luminosity close to the design figure relatively quickly. In that eventuality, additional justification for the considerable investment made in the LEP project must be sought elsewhere – for example, in later phases of LEP and in possible proton facilities placed in the tunnel (but see the comments in section 4.5 above on the effect on the latter of the proposed SSC).

In view of these rather uncertain prospects for LEP, it is worth asking why enthusiasm for the project apparently remains so high. One possible explanation is that, just as the ISABELLE project could, to a large extent, be regarded as a historical relic of high-energy physics interests in the early 1970s [48] (before the technology was available to construct large proton–antiproton colliders), so LEP is a "child" of the mid-1970s when enthusiasm for circular electron–positron colliders was at its height. Then, a large storage-ring like LEP appeared to offer the best prospects for, if not discovering the Z_0 predicted by the newly triumphant Weinberg-Salam theory, at least investigating its properties in the "cleanest" and most systematic way. Since that time, the Stanford Linear Collider has been proposed – apparently capable of reaching the Z_0 region more quickly and considerably more cheaply – and the scientific promise of the proton–antiproton collider has been dramatically fulfilled. Nevertheless, much of the early enthusiasm for LEP still persists in public, although some physicists have begun to harbour private doubts (expressed to us during interviews) about whether the scientific case for LEP Phase I is still as strong as it was, say, in 1978. These doubts may at some stage surface in public if the pressure to secure completion of LEP by the planned date of 1988 continues to restrict severely the level of resources available for the laboratory's other experimental programmes.

Before we offer concluding remarks about the relevance of the present evaluation exercise for science-policy purposes, a few more general observations about the future for CERN are perhaps in order. Of special note is the fact that, apart from the Fermilab 1 TeV machine, all the main

[48] There is little evidence in table 5 to suggest that the United States was wrong to halt the construction of ISABELLE in 1983, despite the $200 million already spent.

new Western facilities are colliders. This has profound implications for the extent and structure of experimental high-energy physics activity, since only a very limited number of experiments are possible at any one time on a collider. As a result, the present tendency for the size of collaborations to grow is almost certain to continue. In the case of LEP, where the intention of the CERN management is apparently to let anyone who wants to use the accelerator join one of the first four accredited experimental collaborations (cf. Robinson [37, p.42]), teams of perhaps 250 researchers now seem likely. In other words, a major fraction of Western Europe's 2000 high-energy physics researchers will be working on just four experiments, each with a lead time of six to seven years from the inception of the experiment to the production of scientific results. The same trend is likely to emerge in the United States, particularly if it is decided that the country can no longer afford to support three national high-energy physics laboratories.

The task of managing these vast collaborations will not be without its organizational and sociological problems: increasing bureaucracy; difficulties facing smaller universities in participating in what is an increasingly centralized activity; limitations on individual creativity and participation in the research process; difficulties in integrating postgraduate students into an experiment whose time-horizon stretches far beyond their training period; and so on. Indeed, it is the growing recognition of these problems that may result in increased pressures on CERN to continue supporting a full range of other experimental facilities, in particular to maintain a sizeable fixed-target programme, and in general to cater for those physicists unable or unwilling to work on LEP experiments. Nevertheless, the fact remains that the tasks of organizing the enormous experimental collaborations working on machines like LEP and providing them with the necessary support are likely to cause problems that will require careful management if they are not to threaten the scientific vitality of the large high-energy physics laboratories. In this respect, CERN probably has an advantage over the US centres since it has had more experience in dealing with such problems over the last 25 years. Hence, CERN is likely to suffer less from the effects of these problems on scientific performance. The advantage formerly possessed by the US laboratories over CERN by virtue of being able to react more flexibly and promptly to changing directions in the field (because of not having to cope with the time-consuming political demands associated with attempting to serve thirteen Member States) may disappear. With it may then go the success previously enjoyed by United States researchers in making most of the crucial discoveries in experimental high-energy physics during the 1960s and 1970s.

6. Some concluding remarks

As will already be clear, our overall assessment of the future for CERN is not a wholly optimistic one, particularly with regard to the scientific prospects for LEP. As such, it is likely to be subject to critical evaluation by LEP enthusiasts and others. No doubt, weaknesses in the analysis will be pointed out. This we fully expect, since any discussion of the future is inevitably clouded by uncertainties.

Yet besides analysing the future prospects for CERN, we would also hope that the paper will succeed in focusing attention on what many now believe are crucial deficiencies in the decision-making process in Big Science. A recent US report noted that

> there is some feeling that the high energy physics community does not have adequate opportunity to participate in this planning process which so affects its future. (Trilling [40, p.59])

However, perhaps of even greater concern is that those *outside* the specialty have even less say in decisions that can now involve the commitment of hundreds or even thousands of millions of dollars. It is perhaps somewhat surprising in an age where state expenditures have come under increasing scrutiny that a project like LEP with such long-term implications, and with such ramifications for other areas of science, was given the go-ahead with apparently so little real discussion in public or even within the scientific community (outside of high-energy physics). Moreover, the administrative device of classifying LEP as an "extension" of existing facilities, thereby curtailing debate about the merits of the project and indeed about the future of the CERN laboratory as a whole, is surely a somewhat debatable precedent from the point of view of publicly accountable science policy. Of course, it would be unfair to single out CERN for special attention in this respect; as was noted earlier, similar practices have been indulged in by US laboratories in protecting their institutional interests – for example, by funding new developments out of operating expenses until the resources committed reach such a level that virtually the only option remaining is completion of the project.

Such problems are, we would argue, intrinsic in those areas of Big Science characterized by large expensive central facilities that structure the interests of major sections of the scientific specialty concerned. Traditionally, the regulation of research activities and the formulation of scientific policies have depended on peer-review carried out in a relatively informal and qualitative manner. Yet under the conditions of "oligarchy" encountered in Big Science, it may become increasingly difficult to locate researchers capable of providing – and, more importantly, being seen to provide – the objective and disinterested judgements on which peer-review mechanisms depend.

It is in this context that we have explored the role of external assessments of the sort described above in helping to promote more open and rigorous discussion of policy options in Big Science. Obviously, the framework employed here for assessing the future prospects of major new research facilities is only one of the possibilities that might be considered for introducing some element of wider public and scientific participation into decision-making. Moreover, given that it is very much a first attempt to tackle this task, it may be subject to criticism. It is worth emphasizing, however, that the intention is *not* to replace the peer-review process – this must remain central in scientific decision-making – but to complement it, providing in a systematic and reproducible manner evidence that can inform decision-makers rather than determining their decisions. For example, if data similar to those appearing in table 5 above had not only been collected in 1978 but also made publicly available, it might have strengthened the case of those then arguing in favour of CERN pursuing the proton–antiproton option. Similarly, a comparison of such a table for 1978 with one, say, for 1981 would have revealed to high-energy physicists *and* the lay public alike that the likely scientific potential for LEP had been appreciably altered by the proposal of the Stanford Linear Collider in the intervening three years. This factor could then have been taken fully into account by politicians and science policy-makers in the CERN Member States (and not just by high-energy physicists) before any major expenditure on LEP had been incurred.

It should perhaps be stressed that the aim of the foregoing analysis is not so much to make CERN a stronger competitor in the international "race" with laboratories in the United States and elsewhere – the costs of the wastage involved in having "losers" as well as "winners" in such a capital-intensive activity as high-energy physics are now too great for even industrialized nations to afford – but rather to reduce the level of direct competition and duplication between major research laboratories and to encourage closer international coordination of research efforts. The latter is one of the concerns of the International Committee on Future Accelerators (ICFA), yet up till now this body has been largely thwarted in its efforts by the continued emphasis placed by physicists on "winning" the next international scientific "race". (This argument is generally most prominently deployed when government backing for a new project is being sought.) To take one example, in an ICFA workshop held in 1978, participants discussed new accelerator projects that were (apparently) beyond the means of individual regions (Eastern Europe, Western Europe, and North America) and which therefore required inter-regional collaboration. Most interest focused on a 20 TeV proton synchrotron, and various technical studies of such an accelerator were subsequently sponsored by ICFA. In 1983, however, US high-energy physicists began to urge their Government to build a proton machine of precisely this energy in order to ensure that their previous "dominant role" (Wojcicki [44, p.18]) in this field did not slip permanently overseas. [49] If plans for construction of this American machine do go ahead, then we can expect physicists in Western Europe (and presumably also in Eastern Europe) to counter in a few years time with plans for an even larger accelerator in order to ensure that the "lead" in high-

[49] See also the discussion in Trilling [40, p.3] of the need for the United States to construct a substantial new facility over the next few years in order to maintain its "pre-eminence."

energy physics is not once again lost to the United States.

Just where this process of escalation (which has several features in common with the "arms race" between the super-powers) will end is by no means clear. Yet at some stage, the governments of the three "super-powers" in high-energy physics must surely call a halt on the grounds that the cost of the "next" accelerator (which will be measured in billions of dollars rather than millions) is too great for a single region to bear. High-energy physicists, who have for many years discussed vague plans for a "world accelerator" only for researchers in one region to persuade their government (or governments) to build such a machine and so steal a march on the other regions, will finally be forced to accept their own rhetoric that this particular field of fundamental intellectual endeavour is a truly international one in which national (or regional) considerations (such as attempting to wrest "the lead" from some other region) should play no part. [50] If this paper succeeds in stimulating discussion of this global "escalation" process and more generally of the problems associated with existing decision-making mechanisms in Big Science, it will have performed a useful function.

[50] Despite the public show of apparent unity among US high-energy physicists on the need to make the immediate initiation of the SSC project their top priority, some at least harbour doubts about the wisdom of the United States attempting to "go it alone" with such a vast undertaking: "My view is that high-energy physics should no longer be done on the basis of national facilities. We in the States should agree to help CERN put cryogenic magnets into the LEP tunnel [to make a 10 TeV proton synchrotron], and should collaborate technologically on this .... Then seven or eight years later Europe and the States should agree to do a big project in the US, for example, a big electron–positron collider. Proper *international* planning now needs to be done." [Interview, 1983].

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