CERN: Past performance and future prospects

II. The scientific performance of the CERN accelerators

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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 a previous paper (Paper I – Martin and Irvine [51] 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, this paper (Paper II) focuses in detail on the scientific performance of each of the CERN accelerators *taken individually*. In particular, it asks, 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 American machine at Fermi National Accelerator Laboratory compare? Attempts are 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 a third paper (Paper III – Martin and Irvine [52]) 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 the 1980s. 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 around the world? And, in the light of the previous record of the CERN accelerators, to what extent is this potential likely to be realized? 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.

1. Introduction

Having summarized in Paper I (Martin and Irvine [51]) the methodology used to evaluate the past performance of the CERN accelerators, and obtained an overall picture of their position in world high-energy physics, we now turn to focus in more detail on the scientific outputs of the individual CERN machines. [1] The time-horizon taken in

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[1] As in Paper I [51], it must be stressed that what is being assessed here is the scientific performance of the CERN accelerators and those who have used them, rather than CERN *per se*.

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this paper is the period from 1961[2] to 1983. However, given that this spans a number of qualitatively very different stages in CERN's history, it is convenient to break it down into a number of four-year periods. We shall concentrate in turn on each period, and, with the aid of various "partial indicators" based on bibliometric and peer-evaluation data, attempt to establish how successful each CERN accelerator has proved. Finally, using the results of interviews with a large number of high-energy physicists in North America, and Eastern and Western Europe, we shall discuss in detail the factors that appear to have determined the relative scientific contributions of the CERN accelerators and how these have changed over time.

2. 1961 to 1964: The commissioning and early work of the PS

1960 must have been a year of some excitement at CERN. The small 600 MeV (million electron-volts) synchro-cyclotron had been operating for three years, and had already claimed a notable discovery (the electron decay-mode of the charged pi-meson). It had also been used to carry out a pioneering "g-2" experiment (to measure the anomalous magnetic moment of the muon), the first in a series of ever more precise experiments spanning two decades for which CERN was to become famous within the world high-energy physics community. Moreover, the race to complete the construction of the 28 GeV Proton Synchrotron (PS) ahead of the rival 33 GeV Alternating Gradient Synchrotron (AGS) at Brookhaven National Laboratory on Long Island, New York, had been won by several months.[3] However, despite these early successes, the total research output from the PS during the period 1961 to 1964 was rather limited, as we shall see below. Perhaps the main problem was that, although the first experimental run on the PS took place in 1960, overall the preparations for experiments were generally still at a very early stage. Thus, while the PS was capable of accelerating particles and producing a beam, it was

> not yet ready to participate fully in research. This is why the first rich harvest in regard to these new questions was not reaped here. This is why the first quantum states, the first new particles, were in fact not discovered here (Weisskopf [73, p. 13]).

The full effects of not being ready to mount a comprehensive experimental programme can be seen in table 1. Certainly the PS began to yield a large number of research papers relatively quickly, accounting for 10 percent of the total world output of journal articles in the field during 1961–62, and nearly 30 percent in 1963–64. This was not only more than the Brookhaven AGS produced during this time (4.5 percent and 11 percent of the world output, respectively), but also more in the latter period than the well-established Bevatron accelerator at Berkeley in California (38 percent and 23 percent). However, the CERN papers reported data from relatively simple experiments, and consequently their overall impact appears to have been considerably less than that of publications from the other front-line proton accelerators. Table 1 shows that PS publications received 14.5 percent of all citations made in 1964 to experimental high-energy physics papers published between 1961 and 1964 – appreciably less than the AGS (23 percent) and the Bevatron (35 percent). The figures on average citations per paper show even more clearly the differences between the accelerators: PS publications earned an average of 1.9 citations per paper, which is very low for a new accelerator and compares rather unfavourably with the figure of 8.0 for the AGS (a typical value for a new accelerator) and that of 3.4 for the Bevatron (not unreasonable for an accelerator that had been operating for ten years). In addition, in terms of highly cited papers, the Bevatron (with 33 papers cited 15 or more times, and 5 cited 30 or more) and the AGS (with corresponding figures of 24 and 5) both seem to have been appreciably more successful than the CERN PS (with figures of 9 and 1, respectively). In his foreword to the *CERN*

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[2] The very first papers from the CERN PS were in fact published in 1960. However, they were so few in number that their inclusion would make no difference to the results reported below. We chose to exclude them because no citation data are available in the *Science Citation Index* [62] for 1960.

[3] One of the main reasons for this was that Brookhaven built an electron analogue to test the new alternating-gradient technique. The CERN team apparently lacked the staff and funds needed for this. However, because of the close links that had been forged with Brookhaven, they were nevertheless able to learn almost as much from the American analogue as they would have from their own facility. Consequently, CERN was able to concentrate its efforts more immediately on the construction of the PS, giving it several months' head-start over Brookhaven, a lead which it maintained right until the accelerator was commissioned.

Table 1
Experimental high-energy physics, 1961–1964 [a]

	Numbers of experimental papers [b] published in past two years		Citations [c] to work of past four years	Average citations per paper	Highly cited papers (Number cited n times)			
	1962	1964	1964	1964	$n \geqslant 15$	$n \geqslant 30$	$n \geqslant 50$	$n \geqslant 100$
CERN PS (28 GeV)	35 (10.0%) [d]	155 (29.5%)	380 (14.5%)	1.9	9 (10.0%)	1 (6.0%)	0	0
Other W. Europe	30 (8.5%)	45 (8.0%)	130 (5.0%)	1.8	4 (4.5%)	1 (6.0%)	0	0
Total W. Europe	70 (18.0%)	200 (37.5%)	510 (19.5%)	1.9	13 (15.0%)	2 (12.0%)	0	0
Brookhaven AGS (33 GeV)	15 (4.5%)	60 (11.0%)	600 (23.0%)	8.0	24 (27.0%)	5 (29.5%)	1 (25.0%)	1 (100%)
Brookhaven Cosmotron (3 GeV)	45 (12.5%)	30 (5.0%)	280 (11.0%)	3.8	9 (10.0%)	4 (23.5%)	1 (25.0%)	0
Berkeley Bevatron (6 GeV)	145 (38.0%)	120 (23.0%)	910 (35.0%)	3.4	33 (37.5%)	5 (29.5%)	2 (50.0%)	0
Other US	25 (6.5%)	50 (9.0%)	210 (8.5%)	3.0	9 (10.0%)	1 (6.0%)	0	0
Total US	230 (61.5%)	255 (48.0%)	2000 (77.5%)	4.1	75 (85.0%)	15 (88.0%)	4 (100%)	1 (100%)
Dubna (10 GeV)	70 (18.5%)	65 (12.0%)	70 (3.0%)	0.5	0	0	0	0
Moscow ITEP (7 GeV)	5 (2.0%)	15 (3.0%)	10 (0.5%)	0.5	0	0	0	0
Other – rest of world	–	–	–	–	–	–	–	–
Total – rest of world	75 (20.5%)	80 (15.0%)	80 (3.0%)	0.5	0	0	0	0
World total	375 (100%)	535 (100%)	2590 (100%)	2.8	88 (100%)	17 (100%)	4 (100%)	1 (100%)

[a] The notes to tables 7 and 9 in Martin and Irvine [51] also apply.
[b] All publication figures have been rounded to the nearest 5.
[c] All citation figures have been rounded to the nearest 10.
[d] All percentage figures have been rounded to the nearest 0.5%.

Annual Report 1965, J.H. Bannier, then President of the CERN Council, referred to certain decisions which

> will enable CERN to maintain its position as one of the world's three leading laboratories in the field (Bannier [4, p. 3]),

and these various bibliometric indicators would suggest that, at the end of 1964, the CERN PS did indeed rank in third position, signficantly behind the Berkeley Bevatron and the Brookhaven AGS accelerators.

In opening up a new energy range in high-energy physics, experience shows that there are often a number of discoveries to be made, some predicted in advance and others completely unexpected. The data on highly cited papers suggest that most of the discoveries in this particular energy range were made not by the PS but by the Brookhaven AGS, as the CERN Director-General himself admitted at the time:

> Certainly, the results could have been more impressive. Some of the recent sensational discoveries were made elsewhere (Weisskopf [73, p. 18]).

Three discoveries at the AGS stand out in particular:

(1) the discovery that there are two different types of neutrinos;
(2) the discovery of the omega-minus particle;
(3) the discovery of charge-parity or CP-violation.

It is worth examining the histories of these three discoveries since they reveal much that can be used to explain CERN's record during the early 1960s.[4]

The idea of undertaking an experiment to determine whether the electron-neutrino and the muon-neutrino are the same or different particles seems to have occurred more or less simultaneously to experimenters at Brookhaven and CERN in about 1960. It was clear that the higher energies of the AGS and PS would, for the first time, permit neutrino experiments to be carried out on a particle accelerator, and the two-neutrino experiment was, as one of the physicists involved has pointed out (Schwartz [61, p. 42]), *the* obvious experiment to do with neutrinos.[5] Such an experiment was attempted at CERN in 1961, but without success (Weisskopf [71, p. 16]). As the Director-General of the time later observed:

> we tried early to construct a beam of neutrinos – perhaps too early – and the first experiment did not lead to any results because of lack of intensity (Weisskopf [73, p. 16]),

and in subsequent years, this failure was attributed by the director of one of the CERN Departments to

> a still inhomogeneous and inexperienced group ... [who] made some mistakes in evaluating the fluxes of particles available (Cocconi [34, p. 349]).

One of those involved in the rival American experiment recollected his experience of the situation as follows:

> [The PS] had a head start on us at CERN, and it was clear that they would be at least six months ahead of us, maybe even a year ... [T]he question then was: Would CERN beat us? And indeed the entire experiment was designed at CERN and was almost ready to mount and then we heard ... that Von Dardel had discovered a mistake in the calculations and indeed it turned out that the beam as it was planned for CERN would give very low intensity because the beam was planned for a 5-foot straight section and the defocusing effect of the magnets right after the straight section would have essentially demolished the beam ... The other mistake, which is in a sense much deeper and is really an indication of the very real difference in physics philosophy in [the U.S.] compared to what it was there [at CERN], was that rather than saying this experiment is so important let's move it to the 10-foot straight section, the hell with all the other junky little experiments that are going on, they cancelled the experiment, and of course we knew they would because that was just the style (Schwartz [61, p. 45]).

The clear implication is that, if a similar mistake had been made at Brookhaven, it would have rapidly been rectified by moving less important experiments out of the way. This would not have been too difficult to arrange since

> there were no committees to decide whether you run or not. It was just Maurice [Goldhaber, the Laboratory Director] and ... his great wisdom prevailed [*ibid.*].

The history of the second major discovery, that of the "omega-minus" particle, also suggests a significant difference in experimental approach between Brookhaven and CERN. The existence of the omega-minus had been predicted in 1962 by theorists who showed that in the SU(3) classification scheme – which successfully accounted for all the known hadron resonances – there was a "gap" corresponding to a new, previously unobserved, particle. The subsequent search for the predicted omega-minus began at CERN and Brookhaven towards the end of 1962, and again assumed the form of a "race" between the user-groups of the two rival accelerators. At CERN, experimenters initially used a beam of K mesons of momentum 3.5 GeV/c, but found no sign of the particle. They therefore decided to raise the K^- momentum to 6 GeV/c,

> but, due to a number of difficulties, this experiment was delayed as late as the summer of 1964. Meanwhile the Brookhaven group set up a 5 GeV/c K^- beam and early in 1964 they found a truly remarkable photograph of a Ω^- production and decay ... This was a great triumph for the new [SU(3)] theory involving particle symmetries (Butler [8, p. 767]).

One of the major difficulties appears to have

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[4] In discussing experimental work here and elsewhere in the paper, we have inevitably been forced to resort to a certain amount of high-energy physics "jargon" in giving names of particles, energy parameters, details of equipment, and so on. We have attempted to keep this to a minimum. The interested reader is referred to articles such as those by Georgi [37], Jacob and Landshoff [45], Polkinghorne [56], Salam [59], 't Hooft [42], and Weinberg [70] for recent popular commentaries on high-energy physics.

[5] Theory suggested that the only way to explain the apparent failure of the muon to decay into an electron plus a gamma ray was to posit the existence of two different types of neutrinos (Schwartz [61, p. 42]).

involved the British 1.5-metre bubble-chamber that was to be used as the particle detector in this experiment. It was reported in 1959 that this would be ready for installation at CERN during 1961 [54, p. 944]. In the event, its arrival from Britain was delayed until 1963, and it then failed to pass its initial operational trials on the PS in the early part of 1964 [9, p. 76]. The first experimental run with the bubble-chamber and the high-energy K^- beam was therefore delayed until June 1964. [6]

The third and probably most important of the early Brookhaven AGS discoveries was that of CP-violation. [7] Two points should be noted in connection with the planning and execution of this experiment. The first is that the experimental proposal was only one and a half pages long, and it freely admited that no detailed calculations had yet been made (cf. Fitch [35, pp. 49–51]). Whereas such a proposal was acceptable at Brookhaven, where decisions on experiments were generally made by the Director alone, its chances of negotiating the formal committee system at CERN would probably have been somewhat slimmer. Instead of trusting the intuition of the two principal physicists involved (both were highly respected experimenters), a committee would almost certainly have insisted that the calculations were first made, thus delaying the experiment. In the event, the experimenters at Brookhaven were able to start taking data within a mere two months of the proposal being written. A second point to note is that a suitable detector already existed and this further shortened the time needed to mount the experiment. This detector had previously been used for an experiment on the lower-energy Cosmotron accelerator at Brookhaven, and therefore could be moved very quickly to the AGS. Indeed, the detector was installed *before* the experiment was approved (cf. Robinson [58, p. 620]), again illustrating the relatively informal system for planning and executing experiments then prevalent at Brookhaven. The fact that the 3 GeV Cosmotron had been operational at Brookhaven for the previous ten years obviously gave the laboratory a considerable advantage over CERN.

It is clear that one of the main problems at CERN during this early period was the lack of experimental equipment. Nowhere was this more apparent than in relation to bubble-chambers which, it was beginning to be recognized, had clear advantages in many areas of high-energy physics over more traditional experimental techniques (such as emulsions or cloud-chambers). As a result, a great deal of effort was directed in the United States to the development of this new technology. Initially, Europe failed to match this pace of development, as the Director-General of CERN admitted in 1963:

> CERN has lagged behind the American laboratories with respect to the use of big bubble-chambers (Weisskopf [72, p. 13]).

At the time of the first experiments on the CERN PS in 1960, the largest bubble-chamber at the laboratory was a mere 300 cm (12 inches) in size, compared with the 72-inch instrument in use at the Berkeley Bevatron accelerator and the 80-inch chamber very shortly to become operational at the AGS. The situation improved at CERN in 1961 when the CERN 1-metre propane bubble-chamber was completed and a French user-group brought an 81-cm hydrogen chamber from Saclay. However, as we have seen, the large British 1.5-metre bubble-chamber did not start running experiments until the summer of 1964, three years after originally planned. Similarly, the 2-metre hydrogen bubble-chamber under construction at CERN was not completed until December 1964, having been delayed two years by staff shortages (cf. Weisskopf [71, p. 18]).

Perhaps the other main problem at CERN during these crucial early years of the PS was "a somewhat ill-defined experimental physics programme" [13, p. 345]). In 1962, the Director-General referred to "the haphazard and timid approach which characterized the beginning of research work" (Weisskopf [71, p. 16]), while another senior CERN official later described the preparation for the experimental programme as "abnormally poor" (Cocconi [34, p. 349]). Four main factors can be seen as underlying these problems with the physics programme and the late provision of appropriate experimental equipment at the laboratory.

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[6] Another possible reason for the delay in using the higher-energy K^- beam was that priority was given to a search for the omega-minus using the antiproton beam from the PS. This search proved unsuccessful because the antiproton cross-sections turned out to be lower than predicted.

[7] This was the only high-energy physics experiment of the 1960s to be honoured with the award of a Nobel Prize.

First, there is the question of the overall management of the PS construction programme. In particular, it could be argued that senior CERN officials placed too much emphasis on early completion of the accelerator (preferably ahead of Brookhaven), and too little on developing a suitable experimental programme (and the construction of the necessary chambers and detectors). Obviously, opinions may differ as to whether this was a primary cause of the accelerator's early under-exploitation, but the subsequent observation by Cocconi that

> most of the activity [at CERN] had been devoted to the construction of the accelerator itself (*ibid*., p. 349)

suggests that he for one had doubts about whether the balance between construction and experimental preparations had been optimal.

Second, it seems that CERN's failure to appreciate fully the need for appropriate experimental instrumentation was related to the comparative lack of experience of European experimentalists at that time. Their American counterparts had been carrying out research on the large accelerators at Brookhaven and Berkeley since the early 1950s, and therefore knew full well what was involved in mounting "state-of-the art" experiments. In particular, their greater experience meant that they were better able to adapt their instruments to new physics needs as soon as they arose, rather than having to wait for sophisticated purpose-built instruments to be constructed. It is probably not without significance that what was arguably the most important experiment to be carried out on the PS in the early 1960s (a study of the decay patterns of sigma hyperons – this yielded the most highly cited publication from the accelerator between 1961 and 1964) was carried out by a collaboration which included an American group.

Third, as in any new multinational research venture, it inevitably took time for an effective organizational structure to develop within CERN, particular problems arising in defining the role of the laboratory *vis-à-vis* user-groups. The original idea was that, while CERN was responsible for the machine, the beams and detectors would be the responsibility of individual groups, with "national truck teams," composed of university physicists from each Member State, arriving at the laboratory with the equipment needed to carry out their experiments. Under this approach, CERN would be expected to provide only a very limited range of experimental equipment for outside user-groups. This may explain why in the early years most of the emphasis at CERN seemed to be on the construction of the accelerator, with far less on beam equipment,[8] and very little on the provision of subsidiary experimental instrumentation and the planning of a coherent and systematic experimental programme that integrated the research activities of central staff and outside users and permitted full exploitation of the new facilities. Very quickly, however, it was found that the "truck team" approach "did not work out successfully" (Adams [2, p. 86]). This necessitated the laboratory having to develop a better integrated research policy, with experimental teams being brought together more by their common interest in a particular experiment and by the existence of complementary technical skills than by their nationality. Not only did this result in a greater internationalization of effort, but it also imposed heavier demands on the laboratory in providing both administrative and technical support. Inevitably, CERN took time to adjust to these greater demands placed upon it.

A fourth and final factor contributing to the problems of the PS during its early years of operation was the commitment of significant sections of the European high-energy physics community to existing research programmes on national accelerators. Three of the four large Member States – Britain, France and Italy – were each operating (or in the process of building) two national accelerator laboratories, while the fourth – West Germany – had one major centre. Consequently, many European physicists were engaged in planning or running experiments on these accelerators, with much of the available technical support channelled into constructing the necessary detectors and instrumentation. (Moreover, in the United Kingdom, there had been a total lack of co-ordination between the domestic experimental efforts[9] and those at CERN, at least until the early 1960s

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[8] There were, for example, severe difficulties with separated beams in the early 1960s. In addition, there was initially a tendency to distinguish work into the categories of "accelerator," "beam," and "detector," with the result that these three elements did not always work well together.

[9] For an evaluation of the scientific outputs from one of the British national laboratories (Daresbury), see Martin and Irvine [50].

(cf. Wilkinson [74, pp. 8–19]) – hardly the best way of trying to ensure that the available resources were utilized as effectively as possible.) Perhaps with greater support from within its Member States, CERN might have been able to mount a comprehensive experimental programme on the PS somewhat sooner than it did. Further discussion of the factors affecting the scientific performance of the PS will, however, be left until later when we have seen how it fared in subsequent years.

3. 1965 to 1968: The CERN PS comes of age

CERN and the PS accelerator entered 1965 in a better prepared state than had been the case four years earlier. In particular, the accelerator had since 1963 been providing a neutrino beam with 100 times the intensity of that originally available on the Brookhaven AGS. Equally important, the new 2-metre bubble-chamber had finally come into operation at the end of 1964. Last, CERN had begun in 1964 to plan a PS Improvements Programme, the aim being to increase the intensity and repetition-rate of the accelerator and to extend the experimental areas. The first effects of Phase I of this programme began to be felt in the latter half of the four-year period under consideration. To what extent was this greater state of preparedness reflected in the scientific outputs from the PS over the four years up to 1968?

Table 2 shows that during this period the PS accounted for just under 30 percent of all experimental high-energy physics papers, considerably more than the Brookhaven AGS (19.5 percent) and indeed all other accelerators in the world. However, the total numbers of citations for the papers from the AGS and PS were virtually identical (about 27.5 percent of the world total). This suggests that the two machines had a very similar overall impact on the advance of high-energy physics during the period, and significantly more than their nearest competitor, the Berkeley Bevatron. The Bevatron's world share of publications and citations was by now declining rapidly because of its low energy relative to the PS and AGS, although it was still well ahead of newer machines of similar or slightly higher energy at Rutherford and Argonne.

In terms of "advances" – that is, papers cited 15 or more times in one year – the PS had managed to catch up and perhaps even overtake the AGS, producing 46 such papers compared with 42. It had been particularly successful in discovering several new resonances, and in investigating the properties of others more thoroughly than had previously been achieved. Yet the more important "major advances" (papers cited 30 or more times in a year) still eluded the PS – it had only one such paper to its credit during the four years compared with 12 for the AGS. Moreover, some of the results reported in this single highly cited CERN paper were subsequently shown to be "mistaken." These results were obtained using a missing-mass spectrometer, and appeared to show a double-peak structure for the A_2 resonance. This was the very first indication of the "split A_2," as it came to be known. The finding was "confirmed" in several other papers by the same collaboration, by no less than four other collaborations working on the PS, and even by a team at the Brookhaven AGS. By the time of the major high-energy physics conference of 1969 (at Lund), it was accepted that

> everyone is now agreed that the A_2 is split [12, p. 233].

It was only in 1970 that experiments at the Stanford Linear Accelerator Center (SLAC), and later at Brookhaven and CERN, began to indicate that the A_2 was not split after all. By the time of the major conference of 1972 (held at Chicago), it was reported that the split A_2

> now seems to have been definitely swept under the carpet [15, p. 316].

All this is not to imply that the split A_2 was the only "mistake" of experimental high-energy physics during the 1960s. Far from it. Most of the major accelerators had at least one "mistake" to their credit. In the case of the CERN PS, there were Maglic's S, T and U resonances that other experimenters found great difficulty in corroborating; there was also the early high value for $|\eta_{00}|$, a result "confirmed" by an experiment at about the same time on the Princeton-Pennsylvania Accelerator. At Berkeley, the Bevatron gave an erroneously high value for the $(K_L - K_S)$ mass difference, as well as results that apparently violated the $\Delta S = \Delta Q$ rule. Finally, at Brookhaven, one AGS experiment claimed to find C-violation in the decay of the η particle. However, the split A_2 was by far the longest-lived mistake. Whereas the others

Table 2
Experimental high-energy physics, 1965–1968 [a]

	Numbers of experimental papers [b] published in past two years		Citations [c] to work of past four years		Average citations per paper		Highly cited papers (Number cited n times)			
	1966	1968	1966	1968	1966	1968	$n \geqslant 15$	$n \geqslant 30$	$n \geqslant 50$	$n \geqslant 100$
CERN PS (28 GeV)	210 (32.5%) [d]	215 (25.5%)	1280 (28.5%)	1330 (26.0%)	3.5	3.1	46 (30.5%)	1 (5.5%)	0	0
DESY (7 GeV)	10 (1.5%)	40 (4.5%)	20 (0.5%)	300 (6.0%)	1.6	6.1	10 (6.5%)	1 (5.5%)	0	0
Rutherford Nimrod (7 GeV)	15 (2.5%)	20 (2.5%)	100 (2.5%)	180 (3.5%)	6.0	4.6	9 (6.0%)	0	0	0
Other W. Europe	50 (7.5%)	50 (5.5%)	220 (5.0%)	190 (3.5%)	2.4	1.9	2 (1.5%)	0	0	0
Total W. Europe	290 (44.5%)	325 (38.5%)	1620 (36.0%)	1990 (39.0%)	3.3	3.3	67 (44.5%)	2 (11.0%)	0	0
Argonne ZGS (12 GeV)	15 (2.0%)	55 (6.5%)	50 (1.0%)	250 (5.0%)	4.2	3.6	6 (4.0%)	0	0	0
Brookhaven AGS (33 GeV)	125 (19.5%)	165 (19.5%)	1270 (28.0%)	1350 (26.5%)	6.8	4.6	42 (28.0%)	12 (66.5%)	1	0 (100%)
Brookhaven Cosmotron (3 GeV)	30 (4.5%)	20 (2.5%)	200 (4.5%)	160 (3.0%)	3.6	3.2	3 (2.0%)	0	0	0
Berkeley Bevatron (6 GeV)	80 (12.5%)	95 (11.5%)	780 (17.5%)	500 (10.0%)	3.8	2.8	9 (6.0%)	0	0	0
CEA (6 GeV)	15 (2.5%)	35 (4.0%)	150 (3.0%)	200 (4.0%)	5.4	3.9	7 (4.5%)	0	0	0
SLAC (20 GeV)	–	20 (2.5%)	–	100 (2.0%)	–	4.5	4 (2.5%)	1 (5.5%)	0	0
Other US	35 (5.0%)	60 (7.0%)	260 (6.0%)	410 (8.0%)	3.7	4.4	11 (7.5%)	2 (11.0%)	0	0
Total US	300 (46.5%)	450 (53.5%)	2710 (60.0%)	2950 (58.0%)	4.9	3.9	82 (54.5%)	15 (83.5%)	1	0 (100%)
Dubna (10 GeV)	40 (6.5%)	35 (4.0%)	120 (2.5%)	60 (1.0%)	1.1	0.9	0	0	0	0
Moscow ITEP (7 GeV)	15 (2.5%)	25 (3.0%)	50 (1.0%)	30 (0.5%)	1.7	0.6	0	0	0	0
Other – rest of world	–	10 (1.0%)	–	50 (1.0%)	–	4.2	1 (0.5%)	1	0	0
Total – rest of world	60 (9.0%)	70 (8.5%)	170 (3.5%)	140 (2.5%)	1.2	1.1	1 (0.5%)	1 (5.5%)	0	0
World total	645 (100%)	845 (100%)	4500 (100%)	5080 (100%)	3.8	3.4	150 (100%)	18 (100%)	1	0 (100%)

[a] The notes to tables 7 and 9 of Martin and Irvine [51] also apply.
[b] All publication figures have been rounded to the nearest 5.
[c] All citation figures have been rounded to the nearest 10.
[d] All percentage figures have been rounded to the nearest 0.5%.

were corrected within two to three years, the split A_2 took five or six years to rectify. During this time, it caused "consternation" to theorists (Jentschke [46, p. 23]), occupying a considerable part of their attention because it cast doubt on the otherwise very successful SU(3) classification scheme for hadrons. The high number of citations earned by the original split A_2 paper suggests that at the time it had a major impact on the scientific community, stimulating much experimental and theoretical work that might not otherwise have been carried out. However, as with the "mistaken"

Fermilab papers discussed in Paper I (Martin and Irvine [51, p. 201]), it is debatable whether such a publication can be regarded as a positive contribution to the advance of scientific knowledge.

To sum up, although the PS had a higher publication output than the AGS over the four-year period up to 1968, papers from the two machines earned a similar total number of citations. However, the main difference between the two accelerators was that the AGS had several "major advances" to its credit while the PS had only one (and one which was later to prove of somewhat dubious merit). In terms of these bibliometric indicators, then, the overall scientific contribution of the PS would appear to have been smaller than that of the AGS [10], but somewhat greater than both the Berkeley Bevatron and the new accelerators at DESY and Argonne.

What reasons can be given for the apparently superior record of the Brookhaven AGS over this period? First, although the difference was less pronounced than hitherto, users of the CERN PS continued on average to be less experienced in using large accelerators than their Brookhaven counterparts. Second, the AGS was by all accounts the better instrumented of the two accelerators; the CERN PS was still several years from obtaining the very large 25-cubic metres hydrogen bubble-chamber, and the 10-cubic metres heavy-liquid bubble-chamber, recommended by the Amaldi Committee (the forerunner of the European Committee for Future Accelerators) as far back as 1963. Finally, the attention of at least some CERN staff was beginning to switch to the Intersecting Storage Rings project and to the even more distant 300 GeV (as it then was) Super Proton Synchrotron. Brookhaven staff, by contrast, had no such major new facility to which they could look forward. [11] However, as with our previous consideration of the period 1961–64, we shall leave further discussion of the factors determining the performance of the PS until later.

4. 1969 to 1972: CERN achieves world pre-eminence

As mentioned previously, the Amaldi Committee had in 1963 recommended a major 200 million Swiss franc programme at CERN to: (1) build large hydrogen and heavy-liquid bubble-chambers together with a range of other detectors; (2) improve the technical performance of the Proton Synchrotron; and (3) increase the number of experimental halls. Although Phase I of the PS Improvement Programme had been completed in 1967, it was not until 1969 that the effects of Phase II began to be reflected in a considerably increased accelerator intensity (with a mean intensity of 1.3×10^{12} protons per pulse for the year, nearly twice the figure for 1965, and ten times greater than that for 1961 – see [10, p. 77]). Then, in 1971, the new heavy-liquid bubble-chamber finally came into operation. These two major developments together constituted a considerable upgrading of the PS facility, and for perhaps the first time gave CERN user-groups a significant technical advantage over physicists undertaking experimental work on the Brookhaven AGS. Moreover, 1971 also saw the completion of the Intersecting Storage Rings (ISR), within the allocated budget and six months ahead of schedule, as well as the go-ahead (after several years of complex intergovernmental negotitations) for construction of the Super Proton Synchrotron. All this gave rise to "a state of unaccustomed euphoria" at the laboratory [14, p. 31]. The first observation of interactions at the ISR was described in the following terms:

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[10] Even at CERN, this conclusion would probably not now be disputed, as this commentator's assessment makes clear: "For a number of years in the 1960s, Brookhaven was the finest high-energy physics laboratory in the world." [26, p. 247]. However, whether this assessment would have been accepted in 1968 is less obvious. At the end of that year, L. Van Hove (formerly Head of the Theoretical Physics Department at CERN and a future Director-General) undertook a review of recent contributions from CERN accelerators to high-energy physics. He identified five major contributions, one on the synchro-cyclotron (the latest g-2 experiment) and four on the PS: (1) leptonic decays of vector mesons; (2) the discovery of several new mesons; (3) the split A_2; and (4) tests of Cabibbo theory. It was somewhat unfortunate that, of the four contributions from the PS, the split A_2 should later be found to be "mistaken," while several of the new resonances (the S, T and U mesons) subsequently came under considerable doubt.

[11] Brookhaven did put itself forward as a possible site for the planned American 200 GeV proton synchroton which eventually went to the Mid-West (Fermilab). However, the involvement of Brookhaven staff in this project was slight, the design-study having been carried out at Berkeley (another unsuccessful contender for the site).

> Never before in accelerator history has commissioning of a machine gone so smoothly, despite the fact that the ISR is the most complex machine ever built and, in the sense of doing something new whose successful operation could not be guaranteed, is among the most adventurous ever built. To be with the jubilant physicists that day when they were clocking up particles coming from interactions in the colliding beams was a rare experience. Not every day does excitement break through the surface so openly [14].

To what extent was this mood of optimism about the future for the ISR subsequently matched by significant experimental outputs from the facility? And what effect did the Improvement Programme have on the scientific contributions made by the PS at the start of the 1970s?

Data on the scientific publications arising from the world's main accelerators over the period 1969–72 are given in table 3. As in 1968, the CERN PS continued to produce about 25 percent of the world total of experimental publications. This was a considerably greater proportion than that achieved by the Brookhaven AGS, whose share dropped from 19 percent to 14.5 percent in 1972, principally because a major conversion programme [12] for the accelerator proved rather more troublesome than that undertaken at the PS and required the total shut-down of the facility for an extended period. The effects of this can be seen very clearly in the citation data. While papers for the two accelerators earned virtually the same number of citations in 1970, the AGS figure had by 1972 dropped appreciably below that of the PS, largely because of the decline in the AGS publication rate (AGS papers were still gaining more citations per paper – 2.9 compared with 2.3 for the PS). The figures on highly cited papers were also very similar, although both accelerators managed only one "major advance" (papers cited 30 or more times in a year) between 1969 and 1972. Overall, it would seem that the PS had finally drawn level with the AGS (it had been appreciably behind in the previous four-year period in terms of "major advances"), and perhaps had even begun to edge slightly ahead.

However, for this period, the most highly cited papers came neither from the PS nor the AGS, but from other newer accelerators. In particular at Serpukhov, the first indications of a rising total cross-section in hadron collisions were found by a joint Soviet–CERN experiment. The discovery of deep inelastic scattering was made by researchers at the Stanford Linear Accelerator Center; this result, presented in the two SLAC papers [13] cited 50 or more times in a year, was important because it provided the first direct evidence that protons were composed of smaller constituent "partons" (subsequently identified with "quarks"). The very first results from Fermilab on proton–proton total cross-sections were published in the latter part of 1972, and these too were highly cited. And, finally, there were two [14] important early results from the ISR: (1) measurements of inclusive reactions, which showed the same "scaling" behaviour as had first been seen in the SLAC deep inelastic scattering experiments; and (2) the discovery of the diffraction minimum in elastic proton–proton scattering, contradicting the predictions of the previously fashionable Regge theory (Jentschke [46, pp. 20–22]). It is evident from table 3 that the SLAC, Serpukhov and ISR machines all achieved a higher impact in terms of citations per paper than either the PS or AGS. Indeed, given the relatively high publication output of SLAC over this period, it is probably fair to conclude that it ranked as the most successful accelerator over this period, closely followed by Serpukhov and the ISR. With SLAC taking over top position from the Brookhaven AGS, the Americans thus retained their record of operating the accelerator contributing most to the advance of high-energy physics. However, if the contributions from the ISR and PS are taken together, then the bibliometric data suggest that the CERN research facilities contributed more to the field than those at any other laboratory in the world. CERN was probably followed by SLAC, Serpukhov, and Brookhaven, with the Berkeley and Argonne accelerators sharing fifth place, a little ahead of DESY. Thus, just over a decade after the PS had come into operation, CERN finally achieved the distinction of becom-

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[12] As with the CERN PS Improvement Programme, the aim was to increase the intensity of the proton beam. However, the problems experienced at Brookhaven with the conversion programme were so great that "the AGS operated initially with beam intensities somewhat lower than pre-shutdown values" [7, p. 47].

[13] These papers were published in 1969, although the experiment had actually been carried out in 1968.

[14] The discoveries of high transverse-momentum events and of rising total cross-sections for proton–proton collisions were made in 1972, but, since the results were not published until 1973, they are considered in the discussion of the period 1973–76 in the next section.

Table 3
Experimental high-energy physics, 1969–1972 [a]

	Numbers of experimental papers [b] published in past two years		Citations [c] to work of past four years		Average citations per paper		Highly cited papers (Number cited n times)			
	1970	1972	1970	1972	1970	1972	$n \geq 15$	$n \geq 30$	$n \geq 50$	$n \geq 100$
CERN ISR (28 + 28 GeV)	–	20 (2.0%) [d]	–	270 (5.0%)	–	12.9	13 (10.5%)	4 (19.0%)	2 (22.0%)	0
CERN PS (28 GeV)	240 (25.0%)	255 (25.0%)	1200 (22.5%)	1150 (21.5%)	2.6	2.3	19 (15.5%)	1 (5.0%)	0	0
DESY (7 GeV)	25 (2.5%)	35 (3.5%)	280 (5.5%)	250 (4.5%)	4.4	4.0	3 (2.5%)	0	0	0
Rutherford Nimrod (7 GeV)	25 (2.5%)	25 (2.5%)	140 (2.5%)	100 (2.0%)	3.2	1.9	0	0	0	0
Daresbury NINA (4 GeV)	10 (1.0%)	20 (2.0%)	20 (0.5%)	70 (1.5%)	2.1	2.7	0	0	0	0
Other W. Europe	45 (5.0%)	45 (4.5%)	240 (4.5%)	250 (4.5%)	2.5	2.7	6 (5.0%)	2 (9.5%)	0	0
Total W. Europe	340 (36.0%)	405 (39.5%)	1870 (35.5%)	2090 (39.5%)	2.8	2.8	41 (33.5%)	7 (33.5%)	2 (22.0%)	0
Argonne ZGS (12 GeV)	90 (9.5%)	95 (9.5%)	400 (7.5%)	490 (9.0%)	2.8	2.6	7 (5.5%)	0	0	0
Brookhaven AGS (33 GeV)	180 (19.0%)	150 (14.5%)	1200 (23.0%)	960 (18.0%)	3.5	2.9	22 (18.0%)	1 (5.0%)	0	0
Berkeley Bevatron (6 GeV)	85 (9.0%)	60 (6.0%)	520 (10.0%)	350 (6.5%)	2.9	2.5	8 (6.5%)	1 (5.0%)	0	0
CEA (6 GeV)	20 (2.5%)	10 (1.0%)	170 (3.0%)	110 (2.0%)	3.0	3.3	2 (1.5%)	0	0	0
Cornell (12 GeV)	15 (1.5%)	15 (1.5%)	130 (2.5%)	70 (1.5%)	4.8	2.2	4 (3.5%)	0	0	0
SLAC (20 GeV)	55 (5.5%)	80 (8.0%)	460 (8.5%)	670 (12.5%)	6.1	4.9	21 (17.0%)	4 (19.0%)	2 (22.0%)	0
Other US	50 (5.0%)	30 (3.0%)	270 (5.0%)	150 (3.0%)	2.5	1.9	6 (5.0%)	4 (19.0%)	3 (33.5%)	0
Total US	490 (51.5%)	445 (43.5%)	3140 (59.5%)	2800 (53.0%)	3.3	3.0	70 (57.0%)	10 (47.5%)	5 (55.5%)	0
Serpukhov (70 GeV)	30 (3.0%)	60 (6.0%)	140 (2.5%)	270 (5.0%)	4.8 (10.7) [e]	3.0 (6.4) [e]	12 (9.5%)	4 (19.0%)	2 (22.0%)	1 (100%)
Other – rest of world	90 (9.5%)	110 (11.0%)	120 (2.0%)	140 (2.5%)	0.7	0.5	0	0	0	0
Total – rest of world	120 (12.5%)	170 (17.0%)	260 (5.0%)	420 (8.0%)	1.4	1.4	12 (9.5%)	4 (19.0%)	2 (22.0%)	1 (100%)
World total	950 (100%)	1020 (100%)	5270 (100%)	5310 (100%)	2.9	2.7	123 (100%)	21 (100%)	9 (100%)	1 (100%)

[a] The notes to tables 7 and 9 of Martin and Irvine [51] also apply.
[b] All publication figures have been rounded to the nearest 5.
[c] All citation figures have been rounded to the nearest 10.
[d] All percentage figures have been rounded to the nearest 0.5%.
[e] This is the figure for papers in Western journals only.

ing the world's foremost high-energy physics laboratory. This elevation into a position of pre-eminence was clearly sensed at CERN, as the following quotations from the *1972 Annual Report* make apparent:

> The results from European physicists, working at CERN or

> using its facilities, were recognized to be of outstanding quality at the International Conference on High-Energy Physics which took place at Chicago. Many scientists at the Conference acknowledged that, in its field, the CERN Laboratory is second to none (Jentschke [47, p. 11]).

> For particle physics at CERN, 1972 proved to be an outstanding year, the pre-eminent place of the Organization's research being generally recognized, in particular at the Batavia conference. Not only was the PS physics programme at least as interesting as in the past but also there were the first exciting results from the ISR [11, p. 26].

5. 1973 to 1976: The American revolution

To what extent were CERN users able to maintain and build upon the pre-eminent position achieved in 1972? On the technical side, various developments took place that served to strengthen the laboratory's research capability. On the Proton Synchrotron, a new Booster Injector permitted greatly increased intensities of particle beam to be obtained. This was to prove of crucial importance for the investigation of weak interactions, in particular those involving neutrinos, enabling the full potential of the Gargamelle heavy-liquid bubble-chamber to be exploited. [15] In addition, the major new Omega Spectrometer came into operation in 1973, being used in PS experiments from 1973 to 1975 and (with some modification) on the Super Proton Synchrotron (SPS) thereafter. However, one disappointment was that the very large hydrogen bubble-chamber (BEBC – the Big European Bubble-Chamber) could only be used for a few experiments on the PS in 1973 [16] and again in 1975. Virtually the whole of 1974 was lost when the BEBC magnet had to be completely dismantled to rectify a short-circuiting fault. As a result, this major experimental facility – first recommended in 1963 as part of the programme to exploit the PS – was in the event only used for a short time on the accelerator (although, as we shall see later, it proved invaluable in the SPS experimental programme). Besides these improvements in the research capability of the PS, the Intersecting Storage Rings (ISR) were by now coming into their own as a powerful experimental facility, partly because of continual increases achieved in luminosity (and hence in the number of collisions per second), and partly because of the large Split Field Magnet detector which began operating in 1973.

With the benefit of all these technical advances, CERN witnessed in 1973 what was probably the most successful year in its history (at least until 1983). Not only did European researchers make the fundamental discovery of neutral currents with the Gargamelle bubble-chamber on the PS, but several other major advances also arose from experiments on both the PS and ISR. (The details are discussed below.) Thus, at the end of the year, the Director-General was able to report with some pride that,

> In 1973 CERN led major advances in our knowledge of the fundamental properties of matter and of the forces determining the behaviour of matter in our Universe (Jentschke [48, p. 11]).

However, the period from 1973 to 1976 was one of the most tumultuous in the history of high-energy physics, ushering in the revolutionary era of what became known as the "new physics." Despite the promising start made by CERN users in 1973, the central role in these developments was subsequently filled by accelerators elsewhere, as can be seen from table 4. True, the CERN PS and ISR were still able between them to account for some 25–30 percent of the world's publications and citations in the field, with the PS continuing to outperform the Brookhaven AGS in this respect – there was a rapid decline in both the publication and citation totals for the AGS compared with the late 1960s (the citation figure, for example, dropped from 28 percent of the world total in 1966 to 10 percent in 1976). However, in terms of "advances" (papers cited 15 or more times in a year), the CERN PS (9 percent) and ISR (14 percent) were some way behind Fermilab (36 percent) and SLAC (19 percent). Moreover, most of the crucial discoveries which revolutionized the field were made elsewhere, particularly on SPEAR, the storage-ring operated by the Stanford Linear Accelerator Center, SLAC. This highly innovative and relatively cheap accelerator was responsible for no less than 5 out of the 8 papers cited 100 or more times in a year that were published during this period.

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[15] In November 1973, for example, a neutrino experiment with Gargamelle benefited considerably from the newly increased PS intensity of over 5×10^{12} protons per pulse, an improvement by a factor of 3 over the usual PS intensity [16, p. 369].

[16] Various technical problems were encountered during that year: see, for example, [17, pp. 370–71].

Table 4
Experimental high-energy physics, 1973–1976 [a]

	Numbers of experimental papers [b] published in past two years		Citations [c] to work of past four years		Average citations per paper		Highly cited papers (Number cited n times)			
	1974	1976	1974	1976	1974	1976	$n \geqslant 15$	$n \geqslant 30$	$n \geqslant 50$	$n \geqslant 100$
CERN ISR (28 + 28 GeV)	40	60	830	710	14.0	7.4	28	12	2	1
	(3.5%) [d]	(5.0%)	(12.0%)	(9.0%)			(14.0%)	(18.0%)	(7.5%)	(12.5%)
CERN PS (28 GeV)	230	280	1320	1140	2.7	2.2	18	4	2	0
	(20.0%)	(23.5%)	(19.5%)	(14.5%)			(9.0%)	(6.0%)	(7.5%)	
DESY (i) 7 GeV	35	30	180	180	2.6	2.7	3	2	1	0
(ii) DORIS (4 + 4 GeV)	(3.0%)	(2.5%)	(2.5%)	(2.5%)		(8.2) [e]	(1.5%)	(3.0%)	(4.0%)	
Rutherford Nimrod (7 GeV)	35	20	120	60	2.1	1.1	0	0	0	0
	(3.0%)	(1.5%)	(2.0%)	(0.5%)						
Daresbury NINA (4 GeV)	15	10	70	40	2.3	1.8	0	0	0	0
	(1.0%)	(1.0%)	(1.0%)	(0.5%)						
Other W. Europe	60	50	330	340	3.0	3.1	5	2	1	1
	(5.5%)	(4.0%)	(5.0%)	(4.5%)			(2.5%)	(3.0%)	(4.0%)	(12.5%)
Total W. Europe	410	445	2830	2460	3.5	2.9	54	20	6	2
	(35.5%)	(38.0%)	(42.0%)	(32.0%)			(27.5%)	(30.0%)	(23.0%)	(25.0%)
Argonne ZGS (12 GeV)	100	70	470	360	2.3	2.1	5	0	0	0
	(9.0%)	(6.0%)	(7.0%)	(4.5%)			(2.5%)			
Brookhaven AGS (33 GeV)	155	95	760	780	2.5	3.2	15	5	2	1
	(13.5%)	(8.0%)	(11.0%)	(10.0%)			(7.5%)	(7.5%)	(7.5%)	(12.5%)
Fermilab (400 GeV)	105	175	780	1900	6.9	6.7	71	26	9	0
	(9.5%)	(15.0%)	(11.5%)	(25.0%)			(36.0%)	(39.0%)	(34.5%)	
CEA (i) 6 GeV	15	–	140	30	5.9	2.3	2	2	1	0
(ii) BYPASS (3 + 3 GeV)	(1.0%)		(2.0%)	(0.5%)			(1.0%)	(3.0%)	(4.0%)	
Cornell (12 GeV)	15	20	110	100	3.4	3.3	1	0	0	0
	(1.0%)	(1.5%)	(1.5%)	(1.5%)			(0.5%)			
SLAC (i) 20 GeV	80	110	670	1330	4.2	7.1	37	14	8	5
(ii) SPEAR (4 + 4 GeV)	(7.0%)	(9.0%)	(10.0%)	(17.0%)		25.5 [f]	(19.0%)	(21.0%)	(31.0%)	(62.5%)
Other US	50	20	270	80	2.0	1.1	2	0	0	0
	(4.5%)	(2.0%)	(4.0%)	(1.0%)			(1.0%)			
Total US	515	485	3190	4590	3.3	4.5	133	47	20	6
	(45.0%)	(41.0%)	(47.0%)	(59.5%)			(67.5%)	(70.0%)	(77.0%)	(75.0%)
Serpukhov (70 GeV)	110	125	480	490	2.8	2.1	7	0	0	0
	(9.5%)	(10.5%)	(7.0%)	(6.5%)	(4.6) [g]	(3.9) [g]	(3.5%)			
Other – rest of world	115	125	260	200	1.2	0.8	3	0	0	0
	(10.0%)	(10.5%)	(4.0%)	(2.5%)			(1.5%)			
Total – rest of world	225	245	740	690	1.9	1.5	10	0	0	0
	(19.5%)	(21.0%)	(11.0%)	(9.0%)			(5.0%)			
World total	1150	1180	6780	7740	3.1	3.3	197	67	26	8
	(100%)	(100%)	(100%)	(100%)			(100%)	(100%)	(100%)	(100%)

[a] The notes to tables 7 and 9 of Martin and Irvine [51] also apply.

[b] All publication figures have been rounded to the nearest 5.

[c] All citation figures have been rounded to the nearest 10.

[d] All percentage figures have been rounded to the nearest 0.5%.

[e] This is the figure for DORIS papers only.

[f] This is the figure for SPEAR papers only.

[g] This is the figure for papers in Western journals only.

These papers reported the discoveries of the J/psi (a discovery shared with the Brookhaven AGS), the psi prime, the tau heavy lepton, and the charmed mesons D and D* which were generally seen as representing the first conclusive evidence of explicit charm (there had been earlier hints of charm from Brookhaven and CERN).

To set against this, the CERN accelerators had a number of notable contributions to their credit, though none seems to have had quite the same impact. From the ISR, there was the discovery of the rising total cross-section for proton–proton collisions. However, it was the Serpukhov accelerator which had a few years earlier provided the first indications that hadron–hadron cross-sections were rising (in kaon–proton collisions, at least), although many Western physicists seem not to have been convinced by the result until it was subsequently "confirmed" by the ISR experiment.[17] Consequently, the ISR finding would almost certainly have had a greater impact if it had been the first of its kind rather than the second. Another important, and at the time unique, result from the ISR was the discovery of an anomalously large number of high transverse-momentum events (see footnote 14) which provided indirect evidence for the existence of point-like components ("partons") within hadrons. However, again the first hints of this hadronic structure had been obtained elsewhere (in the deep inelastic scattering experiments at SLAC) which somewhat lessened the impact of the ISR findings. Beside these two discoveries, the ISR was responsible for several, slightly lower-level advances, including the first indications of a "jet" structure in the particles produced in proton collisions, and the discovery of the direct production of "prompt" leptons (electrons and muons) from proton–proton interactions.

In addition, the CERN PS made a number of important advances during this period. One of these arose from a 1973 experiment with the Gargamelle bubble-chamber and demonstrated that neutrino (and antineutrino)–nucleon cross-sections rise linearly with energy. When combined with earlier SLAC work on deep inelastic scattering, these results provided strong experimental evidence that "quarks" were more than just an interesting theoretical concept. In other words, "quarks became real" [29, p. 440].

A second, and far more important, result from the Gargamelle programme on the PS was the discovery of "neutral currents." This provided the first vital experimental evidence in support of the unified theory of electromagnetic and weak interactions[18] put forward by S. Weinberg and A. Salam in 1967. However, perhaps because high-energy physicists had become accustomed to major discoveries being made in the United States rather than Europe, the Gargamelle result was not immediately acknowledged by everyone as constituting conclusive evidence for the existence of neutral currents, with some believing that other interpretations of the observations reported in the paper had not been completely ruled out.[19] Confidence in the discovery was not helped by the fact that initially a strong difference of opinion existed within the Gargamelle collaboration itself over what the experimental data really showed (cf. Hammond [39, p. 372]), some of those who had worked in the past in the field of neutrino physics being amongst the more sceptical. Nor were the CERN management particularly quick in lending

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[17] "In 1971, such a growth of the positive kaon–proton total cross-section was reported from Serpukhov but the idea of a cross-section growing again with energy did not, in general, sink in." [18, p. 67]). For a further discussion of the relationship between East European and Western high-energy physics, as well as an evaluation of the scientific performance of the Serpukhov accelerator, see Irvine and Martin [43].

[18] The unification of the four forces of nature (gravitational, electromagnetic, weak, and strong) into a single unified theory has been an elusive dream of physicists since Einstein, and it is the over-riding goal of high-energy physicists today. This first step, to unify two of the four forces, is therefore of vital importance, and has been described as "a synthesis as profound as that achieved by Maxwell when he united the phenomena of electricity and magnetism in one theoretical framework" (Jentschke [48, p. 13]). The authors of the theory have since been rewarded with a Nobel Prize.

[19] Cf. e.g. Barish [5, p. 313]: "It was difficult to make an absolutely convincing case for neutral currents from any single experiment because of the experimental problem that for neutral current reactions *both* the initial and final state neutrinos are non-observables. This means that the reaction is under-constrained and alternate mechanisms for the observations are possible. Rather than actually 'seeing' neutral current events, other explanations for apparent events of this topology must be explained away. For this reason, it was vital to look for neutral currents in a variety of experiments and reactions with different background problems."

their support to the possible discovery.[20] A senior physicist described the situation at CERN in the following terms during an interview:

> The management at CERN was not confident about the neutral current discovery. Things were definitely held back by the management. They were confused by experiments at Fermilab that first appeared to find neutral currents and then subsequently couldn't find them – they were known as 'alternating' neutral currents for a while! Although this was the major discovery at CERN, many people are still unhappy about the way it was handled. It lost a lot of its value as a result. I talked to friends on the Gargamelle experiment, and there were big divisions inside the collaboration as to whether neutral currents had been established. Those who thought they had, had to argue a lot. If you look at the early announcements of the work in the *CERN Courier*, you will see how hesitant the CERN management was in believing the discovery. Neutral currents were not believed until they were confirmed by Fermilab (Interview, 1981).

These early doubts (which were largely dispelled in 1974 by the publication of a new and more detailed paper on the Gargamelle results) may explain why, despite the significance of the discovery, the main experimental publication reporting the result failed to earn more than 100 citations in a year (although it came quite close), unlike the crucial discoveries made at SLAC and elsewhere over this period. Nevertheless, it remains a discovery of extremely great importance[21], and one which does illustrate the advantages of the methodical approach to high-energy physics adopted at CERN. (We have previously concentrated on the disadvantages.) Taken together, neutral currents and the linear rise of the total neutrino–nuclear cross-section can with some justification be claimed to represent

> well-deserved scientific rewards of very deliberate policies and of efforts pursued over many years. At the PS these policies involved the increase of the PS intensity through the addition of the booster, and the construction of a high quality neutrino beam. The heavy liquid bubble-chamber Gargamelle was built in France and brought to CERN where it was exploited in the neutrino beam (Van Hove and Jacob [67, pp. 67–68]).

One other result from Gargamelle is worth mentioning – the observation of a possible charmed particle announced in March 1975. However, as with several of the other major advances from CERN discussed earlier, this result was not the first of its type to be published. That honour went to a bubble-chamber group working at the Brookhaven AGS accelerator, who, after nine months of attempting to a rule out other interpretations of a candidate event, finally published their result in April 1975. Consequently, the Brookhaven publication seems to have had slightly more impact (at least if judged in terms of numbers of citations), although it too had considerably less impact than the SLAC paper published a year later reporting the discovery of the D meson (since this was generally acknowledged to represent the first *conclusive* evidence for explicit charm).

Despite these notable discoveries from CERN, arguably the most important experimental result[22] of this four-year period, and indeed of the 1970s, was the virtually simultaneous discovery of the J/psi particle on two United States accelerators at Brookhaven (the AGS) and Stanford (SPEAR). In view of the close similarity between the Brookhaven AGS and CERN PS accelerators, there has been much speculation within the high-energy physics community as to why the J/psi was not discovered on the PS (or indeed on the ISR where it was observed very soon after the discovery). In the case of the PS, the reasons were technical as much as organizational. In particular, it should be noted that the J/psi was *only just* detectable at the AGS energy of 30 GeV; because of this, the leader of the research team making the discovery (S. Ting, who is widely acknowledged to be among the best experimental high-energy physicists in the world) had first to go through a very rigorous checking procedure before he was convinced that a new particle had been found (cf. Ting [63]). Undoubtedly it would have been considerably more difficult to make the discovery at 26 GeV, the normal operating energy of the PS accelerator. Hence, in this particular case, the relatively small difference in energy between the two accelerators may well have proved crucial (cf. Van Hove [65, p. 30]) had a similar experiment been attempted on the PS at the same time. Indeed, a second point to note is that a proposal had previously been sub-

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[20] Galison [36, p. 499] reports that they were "afraid that CERN would be publicly embarrassed by the forthcoming American announcement" on the failure by the Harvard-Pennsylvania-Wisconsin-Fermilab experiment to detect neutral currents, and that the neutral current discovery might suffer a similar fate to the recently "buried" split A_2.

[21] A number of physicists interviewed argued that the discovery of neutral currents warranted the award of a Nobel Prize. Why it was not thus rewarded is still a matter of speculation.

[22] See footnote 32 in Martin and Irvine [51].

mitted to carry out on the PS a bispectrometer experiment very similar to that completed on the AGS:

> Unfortunately, its approval was somewhat delayed at CERN as the first version of the proposal was not accepted (*ibid.*)[23]

As was the case with the CP-violation experiment discussed earlier, this missed opportunity seems to have been related in part to the more complex selection procedures then in operation at CERN. At Brookhaven, the approach was comparatively flexible so that an experiment promising important experimental results might still be mounted quickly, even if the formal proposal itself left something to be desired. According to the more critical physicists interviewed by us, the proposed bispectrometer experiment revealed the limitations of the CERN committee procedure system and in particular its tendency, in the past at least, to shy away from taking risks.

As to why the ISR did not discover the J/psi, it should first be noted that it did in fact come very close. One experimental collaboration, which finished collecting data at the ISR in September 1974, two months *before* the discovery was announced, obtained results that were subsequently found to contain five J/psi events (cf. [19, p. 69]). One of those involved in the collaboration, the American, L.M. Lederman, had previously carried out an experiment at Brookhaven that had yielded a somewhat mysterious result (the so-called "shoulder" in the lepton-pair production curve); but instead of profiting from the comparative advantage of the ISR in the experiment to investigate the reason for the anomalous result (which was in fact due to the J/psi), he and the group were side-tracked by the newly discovered phenomena of high transverse-momentum events and prompt leptons (see above). One of the CERN staff members closely associated with the ISR summed up the situation in these terms:

> There was a signal observed here, but people couldn't understand it. I don't know why. Lederman was involved and he is one of the best experimentalists around, but even he didn't see it. The machine was right, and the experiment was right, and even the observation was right. But we still didn't see the J/psi. This is still considered a *tremendous failure* here (Interview, 1981).

We shall, however, leave further analysis of the factors determining the scientific performance of the ISR until later when its outputs up to 1982 have been considered. To sum up, then, it is clear that with a different approach at CERN – in particular, a more flexible decision-procedure on experiments, and a more adventurous attitude on the part of ISR experimentalists – the course of history in experimental high-energy physics between 1973 and 1976 might have been rather different. As it turned out, this was the period when the CERN research facilities lost their relatively short-lived ascendancy over other accelerator centres, and most of the crucial discoveries in these dramatic four years were made by machines on the other side of the Atlantic. In short, the revolution of the mid-1970s which gave rise to the so-called "new physics" was, with the exception of the discovery of neutral currents, largely an all-American affair.

6. 1977 to 1980: CERN consolidates

Probably the main event at CERN between 1977 and 1980 was the completion of the Super Proton Synchrotron (SPS), which began scheduled operation for experimental research on 7 January 1977 after a very rapid and successful commissioning period. This time, unlike with the PS and to a lesser extent the ISR, a full range of sophisticated detectors was ready for immediate experimental use. The four years also saw important developments on the ISR, with the introduction of various second-generation detectors. By now, it had been realized that the early detectors, which had concentrated on small-angle scattering, were inadequate for many important types of experiments, such as the study of "jets" where 4π detectors (covering virtually the complete solid angle) are needed (cf. [21, pp. 394–95]). These developments enabled CERN to enter the late 1970s with the range of sophisticated facilities and equipment needed to take on the task of consolidating the experimental gains made during the previous revolutionary period. In this section, we consider what effects these various developments had on the scientific output from CERN.

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[23] It was probably this which gave rise to the following comment by a former Director of Brookhaven National Laboratory: "At least we cannot be accused of having disapproved an experiment here which then gave exciting results elsewhere." (Goldhaber [38, p. 82])

Table 5
Experimental high-energy physics, 1977–1980 [a]

	Numbers of experimental papers [b] published in past two years		Citations [c] to work of past four years		Average citations per paper		Highly cited papers (Number cited n times)			
	1978	1980	1978	1980	1978	1980	$n \geqslant 15$	$n \geqslant 30$	$n \geqslant 50$	$n \geqslant 100$
CERN SPS (400 GeV)	25	80	320	520	12.7	5.0	19	7	3	0
	(2.5%) [d]	(8.5%)	(4.0%)	(8.5%)			(11.5%)	(14.5%)	(21.5%)	
CERN ISR (31 + 31 GeV)	50	50	590	440	5.4	4.4	11	2	0	0
	(4.5%)	(5.5%)	(7.0%)	(7.5%)			(6.5%)	(4.0%)		
CERN PS (28 GeV)	245	110	1170	770	2.2	2.2	13	2	1	0
	(22.0%)	(11.5%)	(14.5%)	(12.5%)			(8.0%)	(4.0%)	(7.0%)	
DESY (i) 7 Gev										
(ii) DORIS (5 + 5 GeV)	45	60	450	950	5.7	8.8	36	16	4	0
(iii) PETRA (19 + 19 GeV)	(4.0%)	(6.5%)	(5.5%)	(15.5%)		(11.7%) [e]	(22.0%)	(33.5%)	(28.5%)	
Rutherford Nimrod (7 GeV)	20	20	50	90	1.2	2.2	1	0	0	0
	(2.0%)	(2.5%)	(0.5%)	(1.5%)			(0.5%)			
Daresbury NINA (4 GeV)	10	10	20	20	1.0	0.7	0	0	0	0
	(1.0%)	(1.5%)	–	–						
Other W. Europe	35	20	220	130	2.7	2.3	3	0	0	0
	(3.0%)	(2.5%)	(2.5%)	(2.0%)			(2.0%)			
Total W. Europe	435	350	2830	2920	3.2	3.7	83	27	8	0
	(39.0%)	(38.0%)	(34.5%)	(48.0%)			(50.5%)	(56.5%)	(57.0%)	
Argonne ZGS (12 GeV)	55	30	410	310	3.2	3.6	8	3	0	0
	(5.0%)	(3.0%)	(5.0%)	(5.0%)			(5.0%)	(6.5%)		
Brookhaven AGS (33 GeV)	60	50	420	170	2.7	1.6	0	0	0	0
	(5.5%)	(5.5%)	(5.0%)	(3.0%)						
Fermilab (400 GeV)	180	180	2620	1320	7.3	3.6	40	10	5	1
	(16.5%)	(19.0%)	(32.0%)	(21.5%)			(24.5%)	(21.0%)	(35.5%)	(50.0%)
Cornell										
(i) 12 GeV	20	10	80	100	2.3	3.2	4	2	0	0
(ii) CESR (8 + 8 GeV)	(1.5%)	(1.5%)	(1.0%)	(1.5%)			(2.5%)	(4.0%)		
SLAC										
(i) 20 GeV	105	55	1250	690	5.7	4.4	26	6	1	1
(ii) SPEAR	(9.5%)	(6.0%)	(15.0%)	(11.5%)	(12.5) [f]		(16.0%)	(12.5%)	(7.0%)	(50.0%)
Other US	15	5	80	30	2.3	1.4	0	0	0	0
	(1.0%)	(0.5%)	(1.0%)	(0.5%)						
Total US	435	330	4850	2620	5.3	3.4	78	21	6	2
	(39.0%)	(35.5%)	(59.0%)	(43.0%)			(47.5%)	(43.5%)	(43.0%)	(100%)
Serpukhov (70 GeV)	135	130	320	310	1.2	1.2	0	0	0	0
	(12.0%)	(14.0%)	(4.0%)	(5.0%)						
Other – rest of world	115	120	190	240	0.8	1.0	3	0	0	0
	(10.0%)	(13.0%)	(2.5%)	(4.0%)			(2.0%)			
Total – rest of world	250	250	510	550	1.0	1.1	3	0	0	0
	(22.5%)	(27.0%)	(6.0%)	(9.0%)			(2.0%)			
World total	1115	930	8190	6090	3.5	3.0	164	48	14	2
	(100%)	(100%)	(100%)	(100%)			(100%)	(100%)	(100%)	(100%)

[a] The notes to tables 7 and 9 of Martin and Irvine [51] also apply.

[b] All publication figures have been rounded to the nearest 5.

[c] All citation figures have been rounded to the nearest 10.

[d] All percentage figures have been rounded to the nearest 0.5%.

[e] This is the figure for PETRA papers only.

[f] This is the figure for SPEAR papers only.

The figures in table 5 reveal that the three CERN accelerators together accounted for between 25 and 30 percent of the world output of experimental publications in the period, earning a similar proportion of the world total of citations. In terms of individual accelerators, the contributions of the ISR and PS declined somewhat from the levels achieved previously, but nowhere near as dramatically as the Brookhaven AGS (it earned only 3 percent of citations in 1980 compared with 23 percent ten years earlier) which virtually dropped out of contention as a front-line accelerator as staff at the centre concentrated their efforts on the construction of the new collider, ISABELLE.[24] To some extent, the declines of the PS and ISR were compensated for at CERN by the growing output and impact of the SPS which, taking advantage of beams and detectors that represented a significant improvement over those at Fermilab, achieved a particularly high rate of citations per paper. (The figure of 12.7 citations per paper in 1978 was similar to that for SPEAR at Stanford in 1978 and for PETRA at DESY in 1980.) Nevertheless, of all the major accelerators, the Fermilab machine seems to have contributed most during this period, with a large publication output, over one quarter of total world citations, and a relatively high figure for citations per paper. It should be noted, however, that the record of the Fermilab machine did decline markedly in the latter part of the period, not only because of competition from the SPS, but also because of budgetary pressures arising from an ambitious and expensive capital-expenditure programme (on an "Energy Doubler/Saver" extension to the accelerator and a new proton–antiproton collider facility) that severely curtailed the laboratory's experimental activity.

A similar picture is evident when one considers "advances" (papers cited 15 or more times in a year). Here, the CERN accelerators together actually achieved more "advances" than those at any other laboratory – 43 compared with 40 at Fermilab, 36 at DESY, and 26 at SLAC. However, in terms of this indicator, the Fermilab machine was the most successful individual accelerator. For advances of a slightly higher level (papers cited 30 or more times in a year), the DESY colliders (PETRA and DORIS) appear to have done best with 16 such papers compared with 11 for the CERN machines and 10 for Fermilab. At CERN, the main contributions from the ISR included the work on single leptons, and various observations of charmed-particle production. However, as is evident from table 5, the ISR was rather overshadowed by the achievements of the SPS: these included the refutation of the "high-y anomaly" observed at Fermilab (see Paper I [51]); measurements of structure-functions and deviations from scaling which were found to be in agreement with the predictions of quantum chromodynamics (currently regarded as the best candidate theory of strong interactions); observations of prompt neutrinos in "beam dump" experiments which were believed to provide evidence for charmed-particle production and possibly a third type of neutrino (the tau neutrino); and confirmation of the dimuon events first seen at Fermilab, together with a vast amount of new data (dwarfing previous world statistics) on this form of particle interaction sufficient to enable the explanation to be cleared up. (Until then, various competing hypotheses had been proposed, but the data were not adequate to decide between them.) Analysis of these advances made by the SPS shows that a significant part of the accelerator's research programme was essentially concerned with repeating earlier, sometimes crude, "first-generation" Fermilab experiments, either to correct some of the mistakes and remove various anomalies, or to confirm Fermilab findings but with superior statistics. While a certain degree of satisfaction was inevitably derived by CERN experimentalists from the former[25], the latter task of confirming previous results is inevitably seen by most researchers as being much less rewarding than making the initial discovery, however much better the statistics produced in the confirming experiment. As the leader of one of the main neutrino experiments at the CERN SPS observed,

> I would gladly trade them our events for their discovery (J. Steinberger, quoted in Walgate [68, p. 279]).

The citation records of papers reporting dis-

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[24] Cf. [23, p. 260]: "The considerable effort being devoted to the ISABELLE project has necessarily weakened the support of the 33 GeV AGS and its experimental programme."

[25] For example, a report on the 1977 international conference at Budapest described how "CERN ... was eager to spread the word that the 'high-y anomaly' and related abnormalities in high energy neutrino interactions had been wiped out by new [SPS] data" [24, p. 226].

coveries suggest that these generally have more impact on the advance of scientific knowledge than even the highest-quality confirmatory experiment. This probably explains why, in terms of highly cited papers, the Fermilab accelerator continued to have a better overall record in this respect than the CERN SPS. However, these figures do not reveal the full story about the relative contributions made by the Fermilab and SPS machines. In particular, it should be noted that most of the highly cited papers from Fermilab were published in 1977 (seven out of the ten papers cited more than 30 times, for example), and that a number of them (three out of the five cited more than 50 times) reported results (on trimuons) that subsequently came under severe doubt (see Paper I [51]). Hence, on balance, the SPS was probably not as far behind the Fermilab accelerator in terms of major advances as the figures in table 5 might first suggest, in particular after 1978.

What cannot be doubted, however, is that, as in previous periods, crucial discoveries continued to elude the CERN machines. There were two such discoveries made during these four years, the upsilon at Fermilab, and parity violation in inelastic electron-scattering at the Stanford Linear Accelerator Center – the discovery which, in the eyes of many physicists, finally removed any lingering doubts about the Weinberg–Salam theory being *the* unified theory of electro-weak interactions. [26] At one stage, it did in fact seem as though the SPS had made a discovery of equally crucial importance – a meson containing the bottom quark. This result, if it had subsequently been confirmed, would have constituted a discovery of similar magnitude to that of the charmed D mesons found a few years earlier at Stanford: at the time, it was heralded as providing "seemingly conclusive evidence for the fifth quark" (Robinson [57, p. 777]). Unfortunately, the "particle" later disappeared under a mass of new data (cf. [31, p. 241], [27] thus denying the SPS one of its best chances of a significant discovery.

As in the case of other discoveries, physicists have speculated why the upsilon was discovered at Fermilab and not at CERN. Probably the SPS came into operation somewhat too late, and certainly at the time of the Fermilab announcement it was not in a position to mount an appropriate experiment. [28] In contrast, the upsilon *could* almost certainly have been discovered on the ISR, and, as with the J/psi three years earlier, ISR users came very close. During a preliminary experimental run on the ISR in 1976 (a year before the Fermilab discovery), a few events were seen at an energy of 9.5 GeV, which is almost exactly the mass that the upsilon was subsequently found to have. However, when one of the experimenters involved reported the result,

> he was greeted with great scepticism, so he went away, and eventually came back with new data without this signal. The collaboration only 'found' the signal again after the Fermilab discovery. So, as with the J/psi, the upsilon was in fact seen here first, but it was not interpreted (Interview, 1981).

It is worth noting that, if these few early events had been observed, say, at Fermilab, they would in all probability have been publicly announced, just as in early 1976 a Fermilab collaboration reported on the basis of just 11 events, the discovery of a new particle at 6 GeV (which they named the "upsilon" – see [22, p. 83]), another particle which then "disappeared" in subsequent months. At CERN, in contrast, the tradition has generally been to adopt a more cautious approach to announcing potentially interesting research findings. While this means that CERN has, at least since the "split A_2," largely avoided "mistaken" results of the type that initially afflicted Fermilab (one exception being the candidate "bottom meson" referred to above), it may also have resulted in the laboratory yielding "priority" in a few cases to experiments carried out elsewhere. This question of the difference in research style between Europe and the United States, with high-energy physicists in the latter tending to adopt a more bold and speculative, but at the same time more risky, approach to their experiments, is one to which we

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[26] Although neutral currents had been discovered in 1973, this Stanford experiment was the first to demonstrate that they had just the properties predicted by the Weinberg–Salam theory.

[27] More recently, in 1981, another CERN experiment, this time on the ISR, claimed to find a "bottom particle." However, this finding was disputed, in particular by another ISR team (see [55, p. 478] for details), and most high-energy physicists seem to have reserved their judgement as to whether to believe the result.

[28] "At CERN, the energies available from the SPS for the counter experiments which have appropriate detector configurations in the West Area are too low for fruitful upsilon hunting." [25, p. 320].

Table 6
Experimental high-energy physics, post-1980 [a]

	Numbers of experimental papers published			Citations [b] to work of past four years			Average citations per paper			Highly cited papers (Number cited n times)	
	1980	1981	1982	1980	1981	1982	1980	1981	1982	$n \geqslant 15$	$n \geqslant 30$
CERN $p\bar{p}$ (270 + 270 GeV)	–	4 (1.0%) [c]	8 (2.0%)	–	–	110 (2.0%)	–	–	9.3	3 (20.0%)	3 (75.0%)
CERN SPS (400 GeV)	42 (9.5%)	72 (16.5%)	58 (15.0%)	520 (8.5%)	610 (12.0%)	800 (16.0%)	5.0	3.6	3.8	2 (13.5%)	0
CERN ISR (31 + 31 GeV)	19 (4.5%)	26 (6.0%)	36 (9.5%)	440 (7.5%)	420 (8.0%)	530 (10.5%)	4.4	3.8	4.7	1 (6.5%)	0
CERN PS (28 GeV)	45 (10.5%)	41 (9.5%)	26 (7.0%)	770 (12.5%)	430 (8.5%)	250 (5.0%)	2.2	1.6	1.4	0	0
DESY PETRA (20 + 20 GeV)	25 (5.5%)	23 (5.5%)	31 (8.0%)	470 (7.5%)	580 (11.0%)	880 (17.5%)	11.7	9.2	9.3	6 (40.0%)	0
DESY DORIS (5 + 5 GeV)	6 (1.5%)	7 (1.5%)	3 (1.0%)	460 (7.5%)	280 (5.5%)	120 (2.5%)	10.2	7.3	4.4	0	0
Other W. Europe	23 (5.5%)	17 (4.0%)	11 (3.0%)	250 (4.0%)	160 (3.0%)	130 (2.5%)	1.8	1.4	1.5	0	0
Total W. Europe	160 (36.5%)	190 (44.0%)	173 (44.5%)	2920 (48.0%)	2480 (48.0%)	2820 (55.5%)	3.7	3.3	3.9	12 (80.0%)	3 (75.0%)
Brookhaven AGS (33 GeV)	32 (7.5%)	20 (4.5%)	11 (3.0%)	170 (3.0%)	170 (3.0%)	120 (2.5%)	1.6	1.7	1.4	0	0
Cornell CESR (8 + 8 GeV)	4 (1.0%)	5 (1.0%)	12 (3.0%)	50 (1.0%)	160 (3.0%)	200 (4.0%)	–	18.2	9.3	2 (13.5%)	0

Fermilab (400 GeV)	72 (16.5%)	66 (15.5%)	50 (13.0%)	1320 (21.5%)	1060 (20.5%)	800 (16.0%)	3.6	3.1	2.7	0	0
SLAC PEP (18 + 18 GeV)	–	–	10 (2.5%)	–	–	20 (0.5%)	–	–	2.4	0	0
SLAC SPEAR (4 + 4 GeV)	12 (2.5%)	9 (2.0%)	16 (4.0%)	360 (6.0%)	270 (5.0%)	300 (6.0%)	6.4	5.5	6.6	1 (6.5%)	1 (25.0%)
SLAC linear accelerator (32 GeV)	13 (3.0%)	7 (1.5%)	8 (2.0%)	330 (5.5%)	240 (4.5%)	130 (2.5%)	3.2	3.0	2.7	0	0
Other US	19 (4.5%)	17 (4.0%)	10 (2.5%)	390 (6.5%)	260 (5.0%)	140 (2.5%)	3.0	2.8	2.0	0	0
Total US	152 (34.5%)	124 (29.0%)	117 (30.0%)	2620 (43.0%)	2160 (41.5%)	1710 (33.5%)	3.4	3.2	3.0	3 (20.0%)	1 25.0%)
Serpukhov (70 GeV)	64 (14.5%)	65 (15.0%)	45 (11.5%)	310 (5.0%)	350 (6.5%)	300 (6.0%)	1.2	1.4	1.3	0	0
Other – rest of world	62 (14.0%)	52 (12.0%)	54 (14.0%)	240 (4.0%)	210 (4.0%)	250 (5.0%)	1.0	0.9	1.1	0	0
Total – rest of world	126 (29.0%)	117 (27.0%)	99 (25.5%)	550 9.0%)	560 (10.5%)	550 (11.0%)	1.1	1.1	1.2	0	0
World total	438 (100%)	431 (100%)	389 (100%)	6090 (100%)	5190 (100%)	5070 (100%)	3.0	2.7	2.9	15 (100%)	4 (100%)

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[a] The notes to tables 7 and 9 of Martin and Irvine [51] also apply.

[b] All citation figures have been rounded to the nearest 10.

[c] All percentage figures have been rounded to the nearest 0.5%.

will return in our discussion below on the factors affecting the scientific performance of each of the CERN accelerators. It will suffice to say here that CERN has in recent years become more aggressive in promoting the results of its research, and this, together with the concerted action taken to develop a unique range of sophisticated detectors and specialized particle beams, has played a large part in enabling the users of the laboratory to match and eventually surpass the performance of American laboratories in recent years, as we shall see in the next section.

7. Experimental high-energy physics, post-1980: The European renaissance

In June 1980, the CERN SPS was turned off for 11 months to make possible the construction of the new proton-antiproton collider. As one commentator remarked at the time,

> such a major sacrifice of prime research time has never been seen before and is as sure a pointer as any to the importance of the extension in CERN's research potential which the proton–antiproton collider will bring [30, p. 143].

In the event, this major sacrifice very quickly began to pay dividends. The first proton–antiproton collisions were observed in July 1981, but during the remainder of 1981 the luminosity remained low – a peak of 5×10^{27} $cm^{-2}s^{-1}$ was achieved, a factor of 200 below the design figure. By the end of the following year, however, the luminosity had been increased by an order of magnitude and this, combined with an almost continuous period of operation of the collider lasting nearly two months, yielded a vastly improved integrated luminosity (of some 25 inverse nanobarns). At the end of this run, it was noted that,

> According to the theory, this should be sufficient for the production of some intermediate bosons of the charged kind [33, p. 7].

It therefore came as little of a surprise when, a few weeks later in January 1983, it was announced that the first signs of the charged W intermediate vector boson had been seen. This was followed in May 1983 by the announcement of the discovery of the neutral Z_0 boson.

Besides the commissioning of the proton–antiproton collider, CERN users also benefited from several other improvements to the research facilities. For example, while the SPS was shut down, the WA experimental area for the SPS was upgraded, while the new European Hybrid Spectrometer was completed in 1981, giving SPS users a wide range of powerful detectors, including the EMC, CHARM, BEBC, and Omega prime facilities, with which to exploit the accelerator. In the case of the ISR, improvements continued to be made to the luminosity, and the first experiments involving collisions between alpha particles and between protons and antiprotons were carried out in 1980 and 1981 respectively.

The effects of these various additions and improvements to the experimental facilities at CERN are readily apparent in the figures in table 6. To take first the output of experimental publications, although the number yielded by the SPS dropped slightly in 1982 (reflecting the shut-down a year earlier), this was more than compensated by the emergence of the first results from the proton–antiproton ($p\bar{p}$) collider, and by an appreciable increase in ISR papers as users of the machine rushed to complete as many experiments as possible before it closed at the end of 1983 (see Paper III [52]). Consequently, whereas papers from the CERN accelerators accounted for 24.5 percent of the world total in 1980, the corresponding figure in 1981 and 1982 was some 33 percent. Similarly, in terms of citations, CERN users increased their fraction of the world total from 28.5 percent in 1980 to 33.5 percent two years later. There were three reasons for this: first, the coming into operation of the collider; second, the growing impact of work on the SPS – SPS papers virtually doubled their share of world citations from 8.5 percent in 1980 to 16 percent in 1982, finally catching up Fermilab (although in the meantime the SPS had been overtaken by PETRA at DESY); and, third, a significant increase in the overall impact of ISR papers from 7.5 percent of the world total of citations in 1980 to 10.5 percent in 1982.

The figures on citations per paper suggest that, in 1981 and 1982, CESR at Cornell and PETRA at DESY were the machines with the highest average impact per paper, both recording a level of 9.3 in 1982. For the CERN SPS, the corresponding figure fell from 5.0 in 1980 to 3.6 in 1981 (although it did rise again very slightly in 1982), comparatively low for an accelerator that had only been operating for five years. This somewhat rapid obsolescence can perhaps best be explained by the fact that, by 1980, the Fermilab accelerator – which had been operational since 1972 – and the SPS had between them carried out most of the important work in the 400 GeV energy-range. If so, this illustrates the dangers of building a machine very similar in energy to one that has already been operating for several years, even if it does represent a significant technical improvement on that earlier accelerator. The figures on citations per paper in the case of the ISR are particularly illuminating in this respect since they show an increase from 3.8 in 1981 to 4.7 in 1982. The latter is remarkably high for a machine that has been operating for twelve years (far higher, for example, than the very successful Brookhaven AGS in its twelfth year – see table 3), and must surely be linked to the fact that the ISR was until its closure in 1983 always a unique facility. Given that no larger proton–proton collider is likely to be completed in the near future, the decision to close the ISR in order to free resources for the construction of LEP clearly represented a major sacrifice for the facility's users. In contrast, publications from the CERN PS earned only 1.4 citations per paper – very similar to the figures for the Brookhaven AGS and Serpukhov – suggesting that it no longer constituted a frontier research facility in high-energy physics.

The last two columns of table 6 give the numbers of papers published in 1981 and 1982 that had been highly cited by the end of 1982. These figures suggest that, in terms of "advances" (papers cited 15 or more times in a year), PETRA has proved the most successful over this period with six such papers, followed by the CERN proton–antiproton collider with three, the SPS and CESR with two each, and the ISR with one (Fermilab had no such papers). If the figures for CERN and DESY are combined, one sees that Western Europe produced 80 percent of the papers cited 15 or more times, while the United States managed only 20 percent. A similar picture emerges in the case of "major advances" (papers cited 30 or more times in a year) – the CERN collider yielded three such papers, while the United States had just one (from SPEAR), this being the first time ever that a CERN machine has led the world in terms of the most highly cited papers. Overall, the figures in table 6 for the various machines at CERN and DESY give good grounds for claiming that the onset of the 1980s had witnessed a European renaissance in experimental high-energy physics, even before the discoveries of the W and Z particles were announced in 1983.

Table 7
Comparison of the scientific outputs of the CERN PS and Brookhaven AGS accelerators, 1961–1980

		1961–1964		1965–1968		1969–1972		1973–1976		1977–1980	
		PS	AGS	PS	AGS	PS	AGS	PS	AGS	PS	AGS
Experimental papers[a]		21.5%	8.0%	28.5%	19.5%	25.0%	16.5%	22.0%	10.5%	17.5%	5.5%
Citations to recent[b] work[a]		14.5%	23.0%	27.5%	27.5%	22.0%	20.5%	17.0%	10.5%	13.5%	4.0%
Average citations per paper		1.9	8.0	3.3	5.5	2.5	3.2	2.5	2.8	2.2	2.2
Number of papers cited n times[a] No. of times achieved	$n \geqslant 15$	10.0%	27.0%	30.5%	28.0%	15.5%	18.0%	9.0%	7.5%	8.0%	0.0%
		(8.5%)[c]	(39.0%)	(27.5%)	(33.0%)	(13.0%)	(17.5%)	(13.0%)	(7.5%)	(7.0%)	(0.0%)
	$n \geqslant 30$	6.0%	29.5%	5.5%	66.5%	5.0%	5.0%	6.0%	7.5%	4.0%	0.0%
		(7.0%)	(55.0%)	(13.0%)	(66.5%)	(4.5%)	(2.5%)	(7.0%)	(11.5%)	(4.0%)	(0.0%)
	$n \geqslant 50$	0.0%	25.0%	0.0%	100%)	0.0%	0.0%	7.5%	7.5%	7.0%	0.0%
		(0.0%)	(57.0%)	(0.0%)	(100%)	(0.0%)	(0.0%)	(9.0%)	(11.0%)	(4.5%)	(0.0%)
	$n \geqslant 100$	0.0%	100%)	–	–	0.0%	0.0%	0.0%	12.5%	0.0%	0.0%
		(0.0%)	(100%)			(0.0%)	(0.0%)	(0.0%)	(19.0%)	(0.0%)	(0.0%)

[a] All figures are expressed as a % of the relevant world total, and each has been rounded to the nearest 0.5%.

[b] Work published during the preceding 4 years.

[c] The figures in brackets correspond to those for the number of times papers earned n citations in one year.

8. The overall performance of the CERN accelerators

8.1. The Proton Synchrotron

Having analyzed in some detail the historical development of experimental high-energy physics over the period from 1961 to the end of 1982, we are now in a position to draw some more general conclusions about the overall scientific performance of each of the principal CERN accelerators. Table 7 summarizes the data on the outputs from the first of these accelerators, the Proton Synchrotron, relative to those from the rival AGS machine at Brookhaven. As can be seen, the PS produced appreciably more experimental papers than the AGS. However, this is the only respect in which it consistently outperformed the AGS, since the average impact of those papers was in general considerably less. In terms of citations per paper, the Brookhaven machine until very recently had a superior record to the PS. The AGS was also by far the more successful of the two accelerators in terms of both "crucial discoveries" (papers cited 100 times or more in a year) and "major advances" (those cited 50 or 30 times or more), particularly during the 1960s. Certainly, in the case of "advances" (papers cited 15 or more times), the PS overtook the AGS in the mid-1970s, but it had been well behind in the early years. Hence, when taken together, these indicators strongly suggest that, overall, the AGS was appreciably more successful than the PS over the twenty-year period under consideration. As noted at the time of its twentieth anniversary, the AGS

> has been one of the most productive of accelerators for physics, counting amongst its discoveries the muon neutrino, charge-parity violation, the omega minus, the J/psi,

Table 8
Assessments (on a 10-point scale [a]) of main proton accelerators in terms of (1) discoveries, (2) providing more precise measurements

	Self-rankings	Peer-rankings	Overall rankings
(1) Discoveries			
Brookhaven AGS	9.5(±0.1)	9.0(±0.1)	9.2(±0.1)
	(n = 48) [b]	(n = 121)	(n = 169)
CERN PS	7.1(±0.2)	6.7(±0.2)	6.9(±0.1)
	(n = 86)	(n = 83)	(n = 169)
CERN ISR	6.8(±0.3)	5.9(±0.2)	6.1(±0.2)
	(n = 36)	(n = 133)	(n = 169)
CERN SPS	5.9(±0.3)	5.6(±0.2)	5.7(±0.1)
	(n = 68)	(n = 101)	(n = 169)
Fermilab 400 GeV	7.4(±0.3)	7.1(±0.1)	7.2(±0.1)
	(n = 46)	(n = 123)	(n = 169)
Serpukhov	3.8(±0.5)	2.6(±0.1)	2.7(±0.1)
	(n = 20)	(n = 149)	(n = 169)
(2) More precise measurements			
Brookhaven AGS	7.1(±0.2)	7.2(±0.2)	7.2(±0.1)
	(n = 47)	(n = 120)	(n = 167)
CERN PS	8.5(±0.1)	8.5(±0.1)	8.5(±0.1)
	(n = 86)	(n = 81)	(n = 167)
CERN ISR	7.3(±0.3)	6.9(±0.2)	7.0(±0.1)
	(n = 35)	(n = 132)	(n = 167)
CERN SPS	8.2(±0.2)	8.2(±0.2)	8.2(±0.1)
	(n = 67)	(n = 100)	(n = 167)
Fermilab 400 GeV	6.3(±0.2)	6.0(±0.2)	6.1(±0.1)
	(n = 46)	(n = 121)	(n = 167)
Serpukhov	4.3(±0.5)	3.5(±0.2)	3.6(±0.2)
	(n = 20)	(n = 147)	(n = 167)

[a] 10 = top, and 1 = bottom. The rankings are based on the relative outputs from the accelerators over their entire careers up to the time of the interviews with the high-energy physicists in late 1981/early 1982. The figures in brackets indicate the root-mean-square variations, giving some approximate idea of the divergence of opinion within each group. It should be noted that these rankings were normally made by interviewees in written form.

[b] The sample size of each group is denoted by n.

the charmed baryon, and numerous other particles and resonances [32, p. 242].

Indeed, the AGS can boast a record of major discoveries that only the Stanford Positron–Electron Accelerator Ring, SPEAR, can match. The PS, in contrast, could manage only one result of similar importance – the discovery of neutral currents. This difference between the two accelerators suggested by the indicators listed in table 7 is also well supported by the results obtained in interviews with 182 high-energy physicists (see Paper I [51] for details). Interviewees were invited to assess the overall scientific performance of the world's six principal proton accelerators on a 10-point scale (10 = top, 1 = bottom) in terms of two different criteria: (1) crucial experiments and discoveries; and (2) experiments involving more precise measurements of known particles and their properties but without discovering anything new. The results are given in table 8. On this 10-point scale of achievement, the AGS was judged considerably ahead of the PS in the case of the first criterion (9.2 compared with 6.9), a view, it should be stressed, held by users of both accelerators.

A rather different conclusion, however, emerged when the second criterion of "precise measurements" was considered – the PS was rated 8.5 on the 10-point scale, somewhat ahead of the AGS on 7.2 (with the figures based on self-evaluation and peer-evaluation again showing little variance). Two of the bibliometric indicators are of relevance to this result – the total number of publications, and the average citations per paper. As can be seen from table 7 above, the AGS had a rather better record in terms of the latter. However, this advantage appears to have been outweighed by the far greater publication output of the PS, particu-

Table 9
Factors explaining the relative scientific performance of the CERN PS and Brookhaven AGS (% of interviewees believing this factor to have been important [a])

		Users of AGS (%) ($n = 42$)	Users of PS (%) ($n = 69$)	Users of AGS and PS (%) ($n = 17$)	All interviewees (%) ($n = 125$)
1.	More bold, speculative ethos of US physicists – Europeans more conservative, less risky approach	55	54	65	51
2.	Greater experience of US physicists – Europeans had to learn to use a large accelerator	31	42	47	36
3.	US had better experimentalists in 1960s	29	29	29	30
4.	Higher work ethic and more competitive attitude of US physicists	29	20	35	20
5.	Europe's "missing generation" of physicists – wartime emigration to US of many of the best	19	14	29	11
6.	Luck of Brookhaven in choosing right experiments	21	13	24	17
7.	Better scientific management at Brookhaven – e.g. quick to respond to the unexpected	21	14	24	16
8.	CERN's tendency to "over-engineer" – e.g. detectors built bigger but later	10	14	12	10
9.	Problems of CERN being multinational – e.g. slow committees, over-conservative choice of experiments	62	46	59	51
10.	Social structure of European groups more hierarchical – non-scientific ("political") factors introduced	17	4	6	10

[a] These figures represent minimum values only since they are based on a content analysis of answers to a general question concerning the factors structuring the relative scientific performance of the two accelerators. The sample size of each group of interviewees is denoted by n.

larly in the 1970s when the "citations per paper" figure was not very different for the two accelerators. As a result, whereas between 1961 and 1964 AGS experiments appear to have had a significantly greater impact (at least in terms of the total numbers of citations), the two machines had a very similar record in the period from 1965 to 1968, and then the PS edged ahead. The gap widened considerably during the 1970s as the output from the AGS declined, citations to work published over the preceding four years falling from 20 percent of the world total in 1969–72 to 4 percent in 1977–80. Thus, over their entire lifetimes, the record of the PS was probably slightly superior in terms of this indicator, a result consistent with the rankings given in table 8 for experiments involving more precise measurements.

Besides identifying differences between the scientific performance of the PS and AGS, high-energy physicists were also asked to point to reasons for those differences. Even though this question was unstructured, in the sense that interviewees were not prompted as to all possible reasons for the divergence in performance, the responses tended to conform to certain patterns, and it was therefore relatively easy to classify them into a number of separate categories (although the factors are obviously related to a certain extent). The results are summarized in table 9, which lists all factors cited by 10 percent or more of the interviewees. The first five of these ten factors relate to differing characteristics of the respective user-communities of the AGS and PS. Particularly important here was what many interviewees (in both Europe and North America) identified as a more bold, speculative ethos of physicists in the United States, where the research system was regarded as normally allowing more scope for individual initiative. In contrast, physicists in Europe were seen as adopting a more solid, safe, and conservative approach to their experiments. As can be seen from the table, this factor was specified by approximately half those questioned on the issue, and by rather more (65 percent) of those who had direct experience of using both the AGS and PS accelerators.

Another extremely important factor appears to have been the generally higher level of experimental experience among the AGS user-community, particularly in the 1960s. Many Brookhaven users had previously carried out experiments on the laboratory's 3 GeV Cosmotron or the 6 GeV Berkeley Bevatron, while the experience of most European experimentalists was limited to accelerators of 1 GeV energy or less. The jump involved in moving from experiments on the 0.6 GeV CERN synchro-cyclotron (or small national accelerators) to the 28 GeV PS was obviously much more difficult than for experimentalists progressing from the relatively high-energy 6 GeV Bevatron to the 33 GeV AGS. Consequently, users of the PS inevitably took rather longer to establish how best to exploit the accelerator than their counterparts on the AGS. This factor was again particularly stressed by those who had used both accelerators (47 percent compared with 36 percent of all interviewees).

Other interviewees (30 percent) were rather less specific about the nature of the differences between PS and AGS users, merely mentioning that the US had more competent experimentalists in the 1960s, a fact which some attributed to a better postgraduate educational system. Also cited were the higher work ethic and more competitive attitude of American researchers (20 percent), and Europe's "missing generation" of physicists lost through both the Second World War and emigration to the United States in the 1930s and 1940s (11 percent).

Besides these five user-related explanations, a sixth factor identified was the "luck" of Brookhaven in choosing the right experiments (17 percent). Some of those specifying this factor did, however, go on to relate this "luck" to the greater flexibility and even opportunism of the Brookhaven management in re-orienting the laboratory's experimental programme whenever an unexpected new possibility presented itself (16 percent).[29] Both this and factor 8 in table 9 (CERN's tendency to "over-engineer"[30] – for example, constructing highly

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[29] See also the quotation above in the section on 1961–64 about the absence of committees in the early years at Brookhaven.

[30] This tendency was attributed by some interviewees to the fact that CERN has a relatively large number of "in-house" technical staff who build a higher proportion of experimental equipment than is the case at US accelerators, where universities have a greater role in providing equipment. One consequence of this, it was argued, is that American researchers have generally been more in control of their experiments, and, because they understood the equipment better, were able to undertake rapid modifications when the need arose.

sophisticated detectors that came into operation some time later than less advanced instruments built by their competitors) imply at least some criticism of managerial decision-making at CERN. Indeed, certain high-energy physicists evidently believe that the quality of the Brookhaven management was in no small way responsible for the position of advantage achieved by the AGS over the PS. However, these two factors also need to be seen in the context of factor 9 relating to CERN's multinational character. As a central laboratory catering for a large number of geographically dispersed users, CERN inevitably was under great pressure to provide a service that was, above all, reliable, and this meant that those facilities

> must be designed and constructed *conservatively* and within, if at the limit of, current industrial technology or at least with all innovations studied and tested beforehand (Hine [41, p. 180], emphasis added).

As a previous Director-General has stated, CERN has tried to

> avoid taking risks with the reliability of the machine because then all its users suffer and the worst thing of all is to launch accelerator projects irrespective of whether or not one knows how to overcome the technical problems.[31] That is the surest way of ending up with an expensive machine of doubtful reliability, later than was promised, and a physicist community which is thoroughly dissatisfied. CERN, I am glad to say, has avoided this trap ...[32] (Adams [1, p. 23]).

In general, however, the physicists interviewed pointed to the disadvantages associated with CERN's multinational character, rather than to the advantages. In particular, the need to balance national interests has resulted, it was argued, in a generally greater reliance on a sometimes cumbersome committee structure than at the US national laboratories. Decision-making, therefore, has often been slower, the response to new initiatives sometimes over-cautious, and the choice of experiments too conservative, at least in the opinion of over 50 percent of those interviewed, although many of these did point to the decision by CERN to proceed with the proton–antiproton collider as evidence of a marked improvement in this respect.

Another, wider institutional problem (factor 10 in table 9) centres on the social structure of European experimental collaborations. Not only have these tended to be slightly larger than their American equivalents; they have also apparently been seen by many physicists as more hierarchical and more subject to influence by non-scientific considerations. For example, in Europe, for various historical reasons, an academic culture has evolved in which there may have been a greater unwillingness among young researchers to risk offending senior professors than was typical in the United States. This has sometimes manifested itself in a reluctance to challenge conventional wisdom, and in the adoption of a risk-minimizing approach to experiments. This was contrasted by several interviewees with the more aggressive and risky approach adopted by many young Americans (see factor 1 in table 9), an approach which may fail, but sometimes succeeeds dramatically. One senior experimentalist, who has worked on both sides of the Atlantic, described the situation thus:

> The institutional structures of physics in the US and Europe are different. The people making discoveries at the AGS – they were from univerisites, and their character was formed in a particular social situation. To get a tenured job in a good American university, you have to do well recognized work and to make an individual contribution. In a European country, things are different. The approach is one of career-optimization. It leads to researchers in Europe working on 'precise measurements'-type experiments, rather than trying to challenge existing ideas. Professors are much stronger – you have to fit in with their established programmes of work (Interview, 1981).

To sum up, most of the factors believed by high-energy physicists to account for the differences between the scientific performance of the PS and the AGS extend far beyond the perimeters of the CERN laboratory itself, relating either to its user-community or to the wider institutional context of the centre and of research in Western Europe. Only factors 7 and 8 relate directly to

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[31] The author probably had in mind here the experiences of Brookhaven in the late 1970s with the ISABELLE project. When the project began, the laboratory was by no means certain that the large number of superconducting magnets required could be manufactured. Unfortunately, the gamble did not pay off, with the result that there were considerable delays and escalating costs – see, for example, Broad [6], and Paper III [52]. The project was finally terminated in 1983.

[32] Whether CERN in fact knew how to overcome all the likely technical problems when it embarked on the ISR project is open to dispute. At the time the ISR began operation, its construction was described as "doing something entirely new whose successful operation *could not be guaranteed*" ([14, p. 31], emphasis added). Similar reservations might also apply to the more recent proton–antiproton collider project, where it was by no means certain that the design luminosity could be reached.

CERN policy and management, and these were by no means the most frequently cited factors. This point is worth stressing lest the identification of differences between the performance of the PS and AGS accelerators be seen as attributing blame unduly to CERN itself; the experience with the PS must rather be seen as at least partly reflecting certain broader features of European society during this period.

8.2. The Intersecting Storage Rings and the Super Proton Synchrotron

Assessing the performance of the ISR and SPS is nowhere near as straightforward a task as for the PS, where a direct comparison could be drawn with the AGS because of the similarity in energy and in the starting date of the first experiments. In contrast, the ISR was always a unique facility, while the SPS, although identical in energy to the Fermilab accelerator, only began scheduled operation for experimental research in 1977, more than four years after the American machine. (This was, as we have noted earlier, a rather large lead to concede to Fermilab, given that much of the physics in a new energy region tends to be carried out within the first few years of becoming accessible.) Nevertheless, there are grounds for believing that valid comparisons between these and other machines can still be drawn. For example, we can contrast the CERN ISR with the Serpukhov 70 GeV accelerator in view of the fact that they began producing experimental publications within two years of each other, and both were the highest-energy machines of their type in the world. And, in the case of the SPS, besides comparing its outputs directly with those from Fermilab, it is also possible to examine the scientific performance of Fermilab before and after the SPS started operating to determine what effect this new accelerator had.

Table 10 summarizes for the fourteen-year period from 1969 to 1982 the overall scientific outputs of these four large proton machines and the subsequent impact their work created within the scientific community. For the first four years, Serpukhov publications gained a relatively high rate of citations per paper (CPP), especially for articles in Western journals (for the four years, the figure was 7.7, having been as high as 10.7 in 1970 for the first two years of the accelerator's operation), suggesting these early results had a considerable impact. The same is true of the ISR, with a corresponding figure of 12.9 over the first two years of its experimental life. However, because the Serpukhov accelerator had been operating longer, it achieved more citations overall up to 1972 – 4 percent of the world total in that year compared with 2.5 percent for the ISR. In terms of "advances" and "major advances," the records of the two accelerators were virtually identical. Serpukhov was, however, responsible for the sole "crucial discovery" (cited more than 100 times in a year) of this period – the discovery of rising total cross-sections for hadron–hadron collisions. Finally, it should be noted that the new machine at Fermilab began to make an impact on the statistics for highly cited papers towards the end of 1972.

Over the next four years from 1973 onwards, the picture presented by the bibliometric data in the table is one of clear Fermilab dominance, except in the case of very highly cited papers (gaining 100 or more citations in a year) where the ISR did better (as a result of the publication reporting the discovery of rising total cross-sections for proton–proton collisions). Both Western machines were by then contributing significantly more than the Serpukhov synchrotron, which was increasingly overshadowed by the much higher-energy facility at Fermilab.

In the four years up to 1980, Fermilab continued to produce a large number of experimental publications (17.5 percent of total world output), and achieved by far the greatest number of citations and highly cited papers of the four major proton machines. However, some of the mistaken results from earlier experiments (see Paper I [51]) were beginning to be recognized, and these should be taken into account when comparing its scientific performance with that of the SPS. As we noted earlier, it is significant that the SPS figure for citations per paper fell very rapidly from 12.7 in 1978 to under 4 in 1981 and 1982. Clearly, by the time the SPS began operating in 1977, most if not all the more obvious experiments to be carried out in the energy region had been attempted at Fermilab, and the major discoveries (like dimuons and the upsilon) had already been made. Thus, while the SPS was undoubtedly responsible for a number of major advances (cited 30 or 50 times in a year) stemming from sophisticated second-gener-

Table 10
Comparison of the scientific outputs of the Serpukhov, CERN ISR, Fermilab (FL) and CERN SPS accelerators, 1969–1982

		1969–1972			1973–1976			1977–1980				1981–1982	
		Serpukhov	ISR	FL	Serpukhov	ISR	FL	Serpukhov	ISR	FL	SPS	FL	SPS
Experimental papers[a]		4.5%	1.0%	0.0%[c]	10.0%	4.0%	12.0%	13.0%	5.0%	17.5%	5.0%	14.0%	16.0%
Citations to recent[b] work[a]		4.0%	2.5%	0.0%[c]	7.0%	10.5%	18.5%	4.5%	7.5%	27.5%	6.0%	18.0%	14.0%
Average citations per paper		3.4 (7.7)[d]	12.9	–	2.4 (4.2)[d]	9.9	6.8	1.2 (2.4)[d]	4.9	5.5	6.4	2.9	3.8
Number of papers cited n times[a]	$n \geqslant 15$	9.5%	10.5%	3.5%	3.5%	14.0%	36.0%	0.0%	6.5%	24.5%	11.5%	0.0%	13.5%
		(12.5%)[e]	(11.5%)	(4.0%)	(3.0%)	(15.0%)	(32.0%)	(0.0%)	(7.0%)	(23.0%)	(13.0%)	(0.0%)	(12.0%)
	$n \geqslant 30$	19.0%	19.0%	14.5%)	0.0%	18.0%	39.0%	0.0%	4.0%	21.0%	14.5%	0.0%	0.0%
		(23.0%)	(18.5%)	(14.0%)	(0.0%)	(15.5%)	(34.0%)	(0.0%)	(4.0%)	(24.5%)	(13.0%)	(0.0%)	(0.0%)
	$n \geqslant 50$	22.0%	22.0%	33.5%	0.0%	7.5%	34.5%	0.0%	0.0%	35.5%	21.5%	–	–
		(26.5%)	(13.5%)	(33.5%)	(0.0%)	(11.0%)	(23.5%)	(0.0%)	(0.0%)	(36.5%)	(18.0%)		
	$n \geqslant 100$	100.0%	0.0%	0.0%	0.0%	12.5%	0.0%	0.0%	0.0%	50.0%	0.0%	–	–
		(100.0%)	(0.0%)	(0.0%)	(0.0%)	(6.5%)	(0.0%)	(0.0%)	(0.0%)	(66.5%)	(0.0%)		

[a] All figures are expressed as a % of the relevant world total, and each has been rounded to the nearest 0.5%.

[b] Work published during the preceding 4 years.

[c] The very first experimental papers from Fermilab were published in the latter part of 1972, but the statistics are too small to be significant, except those for highly cited papers.

[d] This is the figure for papers in Western journals only.

[e] The figures in brackets correspond to those for the number of times papers earned n citations in one year.

ation experiments, its record up to 1982 was probably not quite as distinguished as that of the ISR in its early years. In particular, it should be noted that, while the ISR was responsible for one crucial discovery (cited more than 100 times in a year), the SPS did not come close to equalling this feat in the period reviewed.

In summary, then, if these four accelerators are compared in terms of "discoveries," the Fermilab machine must undoubtedly be placed first, followed by the ISR, and with little to choose between the aggregate records of Serpukhov and the SPS. The fact that the SPS achieved in the four years up to 1980 approximately the same number of papers cited more than 15, 30 and 50 times in a year as Serpukhov achieved over its first twelve years (Serpukhov has contributed very little since 1972), would probably tip the balance in favour of the SPS being ranked third, slightly ahead of Serpukhov. Referring back to the peer-evaluation results given earlier in table 8, one sees that these rankings are in close agreement with those made by high-energy physicists: Fermilab was given a score of 7.2 on the 10-point scale for discoveries (though, it should be noted, some way behind the Brookhaven AGS), while the ISR was scored 6.1, the SPS 5.7, and Serpukhov 2.7. [33]

As for the relative positions of the four machines in terms of "precise measurements," the results are rather more difficult to interpret, largely because of the differing periods of time over which the accelerators have been operating. However, we have seen from tables 5 and 6 that since 1980 the SPS has yielded more publications than the three other machines, and that from 1978 SPS publications have earned more citations per paper (except in the case of ISR publications in 1981 and 1982). It is therefore perhaps not too surprising that, for "precise measurement" work, high-energy physicists ranked the SPS first of these four accelerators (although it was placed a little behind the PS) with a score of 8.2, while the ISR was also rated quite highly at 7.0 in line with its consistently high

Table 11
Factors explaining the relative scientific performance of the CERN ISR (% of interviewees believing this factor to have been important [a])

		Users of ISR (%) (*n* = 29)	Others (%) (*n* = 84)	All interviewees (%) (*n* = 113)
1.	Poor/wrong choice of detectors initially (e.g. no 4π solid-angle detectors)	72	49	55
2.	ISR given too low priority by CERN (because of SPS) – some experiments/detectors too late	34	19	23
3.	Poor scientific management – failure to respond promptly when need for new detectors apparent	28	19	21
4.	Wrong choice of machine – colliding beams less fruitful for HEP than fixed target machines	3	23	18
5.	Research programme too fragmented – no overall strategy (e.g. too many small experiments initially)	28	13	17
6.	Early luminosity too low for successful experiments	34	17	21
7.	Problems with the main detector (Split Field Magnet)	7	11	10
8.	Had to learn from scratch how to use machine – proton–proton collisions inherently very complex	48	40	42
9.	Bad luck – turned out not to be a very exciting energy region	17	31	27

[a] Minimum estimates only (see note a to table 9). The sample size of each group of interviewees is denoted by *n*.

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[33] These results in all likelihood exaggerate the difference between the contributions made by the Soviet accelerator and the other three. Part of the difference may be attributable to an ignorance on the part of many Western scientists of all the results from the Soviet machine – see Irvine and Martin [43].

number of citations per paper. Fermilab, despite its large number of papers, was deliberately marked down by many interviewees because of several earlier mistaken results, scoring 6.1, with Serpukhov again ranked last on 3.6.

Having examined the relative outputs from the CERN ISR and SPS, we can now move on to analyse the factors that have determined their scientific performance. Taking the ISR first, there can be no doubt that, from a technical point of view, the machine has been a tremendous success. In particular, its luminosity has been improved by several orders of magnitude over its lifetime, far surpassing the original design specifications. There is thus some justification to the claim that

> The ISR is widely regarded as the most perfect example to date of the accelerator builder's art [27, p. 231].

However, there is, as has been seen, a widespread feeling among high-energy physicists that, as a research facility, the ISR was for some years not exploited as successfully as it might have been, somehow missing the opportunity of making such crucial discoveries as the J/psi and the upsilon. This sense of disappointment that the potential of the machine was not fully realized (which is not to say that it was unsuccessful) is reflected in table 11. This summarizes the results obtained from asking high-energy physicists to identify the main factors determining the ISR's relative scientific performance. Factors 1 to 5 relate to problems associated with the management of this facility and to decisions taken at CERN, these clearly being seen as central by many of those interviewed. In particular, the decisions to concentrate initially on small-angle collisions came in for much criticism, especially from ISR users (with 72 percent citing this as a major problem). One of the CERN senior staff closely connected with the ISR recalled the events surrounding this decision in the following terms:

> The Split Field Magnet is the main detector on the ISR. The design of the device as originally instrumented emphasized forward and backward angles, not large angles. So it missed the J/psi exactly. At the time, CERN management was convinced that no particles would be found at large angles. This was based on Argonne experiments and cosmic rays. There was some competition between different possible spectrometers, but they chose the wrong one. At one stage, they considered doing two detectors, one for looking at large angles. But they decided to phase these, and leave the large-angle one till later. They thought the ISR had the energy region to itself. They forgot about the competitive aspect, and Ting [the co-discoverer of the J/psi] at Brookhaven. (Interview, 1981)

The decision to postpone construction of a large-angle detector was also probably related to factor 2 in the table, the judgement by some high-energy physicists (23 percent of these interviewed) that the ISR and its experimental programme were not given sufficient priority by CERN management. The most critical of those physicists believed that the ISR was regarded by some senior CERN officials as little more than a stop-gap measure to preserve the laboratory's accelerator-building capacity (and capital-equipment budget) until agreement could be secured to build the more prestigious SPS accelerator. It was pointed out that, in order to facilitate approval for the SPS project in 1971, CERN had to promise to reduce the budget allocated to existing facilities (in Laboratory I, as it was then called) by just over 200 million Swiss francs between 1971 and 1978 (see Paper I [51]). In 1973, the Director-General reported that preparations for the SPS were

> beginning to dominate all our planning at CERN since it must be pushed hard during the next three years in order to start the experiments as early as possible. This makes considerable manpower and financial demands on Laboratory I ... (Jentschke [48, p. 23]).

While most of this heavy burden fell on the PS experimental programme, the ISR did not escape entirely.

Some of the high-energy physicists interviewed (21 percent) were not so specific in their criticisms, citing the scientific management of the ISR in general as a major problem (factor 3). Others (17 percent) argued that, at least in the early years, the ISR experimental programme had been too fragmented, there being no overall strategy for exploiting the accelerator and addressing the outstanding theoretical questions of the day (factor 5). As one physicist commented,

> They tried to use the ISR in the same way as the PS, with lots of small experiments. (Interview, 1981)

Initially, there was, for example, no large general-purpose detector of the type that is nowadays regarded as essential to exploit colliders. In addition, the early experiments were rather too crowded together – there were at one stage four experiments at just one of the intersection regions. Such problems were perhaps compounded by the division of opinion that existed (and still exists) within

Table 12
Factors explaining the relative scientific performance of Fermilab and the CERN SPS (% of interviewees believing this factor to have been important[a])

	Users of Fermilab (%) ($n = 39$)	Users of SPS (%) ($n = 56$)	Users of Fermilab and SPS (%) ($n = 7$)[b]	All interviewees (%) ($n = 136$)
1. Fermilab had 4-year lead – SPS too late	56	54	29	58
2. Fermilab more bold, speculative experiments – CERN solid, precise 2nd-generation experiments	49	36	43	39
3. Problems of CERN being multinational – e.g. slow committees, over-conservative choice of experiments	10	9	14	11
4. More resources and technical support for CERN experiments – sometimes inadequate at Fermilab	49	55	43	50
5. SPS a much better accelerator – e.g. better beams	15	32	29	26
6. Fermilab cut too many corners building accelerator and detectors – unreliable, "under-designed"	31	32	29	31
7. Fermilab philosophy wrong – too much of running costs channelled into new accelerator projects	15	13	29	14
8. Fermilab spread themselves too thinly on the ground with experiments, given the resources	21	14	14	13
9. Energy range of both accelerators turned out to be relatively unexciting	13	5	0	10

[a] Minimum estimate only (see note a to table 9). The sample size of each group of interviewees is denoted by n.
[b] This is a very small sample size, and the % figures may not therefore be statistically very significant.

the laboratory over the value of the ISR as an experimental facility. A not insignificant fraction of physicists believed the ISR to have been the wrong choice of machine (factor 4), arguing that colliding protons is not as fruitful a research technique as the "fixed target" approach with synchrotrons, and that the ISR diverted CERN's efforts away from the much more important SPS project.

Problems with the management of the ISR were by no means the only factors cited. Many physicists also believed that technical problems were central in limiting the accelerator's scientific performance, particularly in the early years: 21 percent of interviewees pointed out that the luminosity of the ISR was initially too low for certain types of experiment (factor 6); and 10 percent specified various technical difficulties with the main detector. In addition, two problems (factors 8 and 9) were identified that were probably completely unavoidable. Nearly half those interviewed argued that, because the ISR was the world's first proton collider, physicists had to start from scratch in establishing how to use it,[34] while the very big jump in centre-of-mass energy that it made possible meant that there was necessarily a long learning-curve in finding how best to exploit the facility. Moreover, proton–proton collisions are inherently difficult to interpret – they involve the interaction of three quarks with three other quarks, generally producing large numbers of secondary particles which make analysis of the results a rather complex process.[35] In addition, a certain number of interviewees (27 percent) put forward the view that the ISR had been the victim of bad luck, in the sense that the energy range which it opened up proved not to be as exciting as had been anticipated. However, this seems difficult to

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[34] In contrast, users of the Stanford electron–positron ring, SPEAR, were able to draw upon the experience gained from earlier, smaller rings at Frascati, Orsay, and Cambridge.

[35] The analysis of the data requires a powerful computing capability, and some of those interviewed pointed out that this was not available until several years after the ISR began operating.

reconcile with the fact that the J/psi and the upsilon – the two most highly cited discoveries of the 1970s, and arguably the two with the greatest impact – were there to be unearthed by the ISR if appropriate experiments had been carried out soon enough.

Let us now move on to consider the factors underlying the scientific performance of the SPS, especially in relation to its Fermilab rival. As we have seen, the Fermilab accelerator had up to 1982 proved somewhat more successful in terms of major advances and discoveries, while the SPS had a better record (according to the peer-evaluation results) for precise measurements. Table 12 summarizes the main reasons given by high-energy physicists to explain these differences in the pattern of performance of the two accelerators. Factors 1 and 2, and perhaps also 3, together largely explain Fermilab's better record from major advances. Well over half of those interviewed (58 percent) felt that the Fermilab lead of some four and a half years proved too big a handicap to the SPS (factor 1). To many, the subsequent difficulties for the SPS could have been predicted; as early as 1973, it had been pointed out that such a lead represents

> nearly half the 'interesting' lifetime of most high energy physics installations – which raises the question of whether there will be significant investigations left for it to pursue. (Hammond [40, p. 1120])

Indeed, it was the recognition by CERN in 1976 of the significance of Fermilab's lead that led them to consider, even at this early stage, other possibilities for exploiting the SPS. It was as a response to this that the proton–antiproton collider proposal was put forward and approved relatively quickly (cf. Van Hove [66, p. 31]).

In addition, this extensive lead had implications for the type of experimental programme that could profitably be mounted on the SPS accelerator. Many of the physicists interviewed pointed to a difference in approach between Fermilab and CERN, with the former undertaking more bold, speculative experiments (see factor 2). This is, in turn, partly a function of the different styles of American and European researchers (a factor discussed earlier in connection with the AGS and PS accelerators), and partly the result of deliberate policy choices made by the two laboratories. At Fermilab, they decided to take full advantage of the lead over the SPS by carrying out a broad, but sometimes rather shallow, experimental programme, trying a little of everything in order to "cream off" any major discoveries that could be made in the energy range. The discovery of dimuons and the upsilon were the rewards of this approach, while conversely the production of several papers containing "mistaken" results was a negative consequence of the strategy. At CERN, in contrast, it was recognized early on that the SPS would stand the best chance of making a significant contribution to scientific progress by performing detailed, high-precision, second-generation experiments. Though this often meant repeating previous Fermilab work, these experiments have tended to provide definitive sets of statistics, as well a clearing up the mistakes made by the American accelerator. Factor 3 – the problems associated with the multinational character of CERN – has already been considered in connection with the PS, and will not be discussed further. It will suffice to record that 11 percent of interviewees regarded this as a limiting factor on the performance of the SPS.

Factors 4 to 8 shed further light on the reasons why the SPS has done rather better at "precise measurement" experiments (a subject already touched upon in our treatment of factor 2, above). Undoubtedly the main reason for this is the greater resources and technical support available at CERN, this factor (4) being mentioned by about half the researchers interviewed.[36] Many of the research staff at Fermilab openly expressed envy of the large, sophisticated detectors then available at CERN; there was no way, they argued, that Fermilab, with its limited operating budget (which averaged just under half that of CERN[37]) could afford to build such technically advanced equipment. (In the early 1980s, Fermilab experienced difficulty even in meeting the power costs of running its experimental programme, and was forced to shut down the accelerator for extended periods.)

Also of great importance has been the high technical quality of the CERN SPS: with the Fermilab machine so far ahead, there was little

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[36] In addition, 84 percent of the physicists completing the attitude survey (described in the final section of this paper) agreed with the statement, "CERN provides European high-energy physicists with a better level of technical support and facilities than that enjoyed by Americans at their national laboratories". A mere 2 percent disagreed.

[37] See table 5 in Paper I [51].

point in rushing to complete the construction programme of the facility. The SPS was therefore built very carefully, and is, as a result, a significantly better accelerator. Thirty-two percent of SPS users and 15 percent of Fermilab users identified this as a factor explaining the SPS's advantage in producing precise results. The obverse of this is that the Fermilab accelerator has proved somewhat unreliable, over 30 percent of physicists at both CERN and Fermilab pointing to this problem (factor 6). The magnets in particular have caused numerous problems, initially absorbing moisture from the humid air with the result that over half had to be replaced, some more than once. This generally lower reliability at the American centre can be understood partly as a result of long-term financial pressures on the laboratory, [38] and partly as a consequence of the philosophy of R.R. Wilson, the first Director of Fermilab – a philosophy which involved

> cutting corners whenever possible and generally following a tight design. This approach is given credit for getting the accelerator built quickly, and within a stringent budget .... But some physicists now question whether a more conservative approach – such as that being followed [with the SPS at CERN] ... would really have required any more time or money (Hammond [40, p. 1117]). [39]

Another aspect of Wilson's philosophy was his belief that a significant proportion of Fermilab funds should be invested in accelerator development to ensure that, once the SPS came into operation, the Fermilab facility could be rapidly upgraded to give it a renewed advantage over CERN. Some of those interviewed (about 14 percent – see factor 7) clearly felt that too high a proportion of Fermilab's operating budget had been channelled into the new Energy Doubler/Saver project, [40] thus impoverishing what was already a rather inadequately supported experimental programme. A not insignificant number of interviewees (13 percent, and 21 percent at Fermilab) also felt that, in its effort to "skim the cream," Fermilab had tried to do too many experiments too quickly. [41] By the end of the first four years of operation, 152 experiments had been completed, well over twice as many as at the SPS (66 experiments) over the equivalent period of time. This clearly seems to have had implications for the experimental results obtained by Fermilab users. As one commentator concluded,

> The laboratory had started life with a shotgun approach to particle research, mounting many small experiments, and critics think Wilson waited too long to consolidate the experimental program and to build fewer large selected experiments with greater resolving power (Metz [53, pp. 196–97]).

Finally, we should note that 10 percent of interviewees expressed a feeling that, in retrospect, neither accelerator had made a very major contribution to high-energy physics because the energy range that they covered turned out to be relatively unexciting. This could not, of course, have been predicted in advance, but it does highlight the negative consequence, identified by certain physicists, of both Europe and the United States making their largest investment of the 1970s in essentially identical accelerators.

This concludes our analysis of the factors determining the relative scientific performance of the three main CERN accelerators. The only remaining task of this paper is to synthesize the principal points arising from our assessment of CERN over the past twenty years, in particular drawing out any lessons of relevance to the subject of Paper III [52], the future prospects for CERN.

9. CERN's past performance – an overall assessment

Before summarizing the main conclusions that can be drawn from our assessment of CERN's past scientific performance, it perhaps needs reemphasizing that certain aspects of CERN's activities have *not* been evaluated here. We have not, for example, commented upon the very substantial

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[38] Cf. [28, p.111].

[39] Nearly twice as many of those interviewed agreed as disagreed (61 percent compared with 33 percent) with the statement, "The early Fermilab philosophy of cutting all possible corners to save time and money has not paid off in the long run."

[40] Cf. Metz [53, p. 196]. The Doubler/Saver is a major project to increase the energy of the Fermilab accelerator from 400 GeV to between 500 and 1000 GeV. Paper III [52] gives further details.

[41] In the attitude survey (see final section), 72 percent of those interviewed agreed with the statement, "At Fermilab, there has been a tendency in the past to accept too many experiments, with the result that experiments have often been rushed or prematurely cut short"; only 8 percent disagreed.

technical contributions to high-energy physics for which CERN has been responsible. These include the invention of proportional wire chambers, the development of streamer chambers (following up the pioneering work of G. Chikovani at Tbilisi in the Soviet Union), liquid argon detectors, various bubble-chamber developments (ultrasonic chambers, small high-resolution chambers, holographic chambers, etc.), and the invention of the technique of stochastic cooling. Nor have we evaluated the extensive contributions made by CERN theorists – to the quark model, to Regge and Cabibbo theory, to quantum chromodynamics, and more recently to grand unification theories and supersymmetry. And we have mentioned only in passing the participation of CERN physicists in experiments at Serpukhov, and in particular their role in helping make the crucial discovery there of rising total cross-sections.

Furthermore, it should be noted that CERN has been responsible for several wider contributions besides helping to further our knowledge of high-energy physics. These have been described in the following terms:

> It has been 'a source of European spirit'. It has played a key role in re-establishing the stature of European science. It has a continuing impact on science teaching in universities. It has promoted and helped sustain technical excellence in scientific equipment [20, p. 262].

To take the first of these, there was no doubt in the minds of the high-energy physicists we interviewed that CERN has substantially stimulated international co-operation in science, not only in Europe, but also at a world level, promoting contacts with North America, Eastern Europe, and the Third World. As for the second, a cursory glance at tables 1 and 6 to contrast the respective scientific outputs of CERN in the early 1960s and twenty years later is sufficient to demonstrate the tremendous strides made by West European high-energy physicists in relation to their American counterparts. West European experimental papers for example, earned only 19.5 percent of the world citation total in 1964, a factor of four less than the US figure of 77.5 percent. In 1982, the corresponding figures were 55.5 percent and 33.5 percent, showing that Europe had completely reversed the situation. The third effect, the impact on university education, is harder to gauge; undoubtedly, there has been some impact, but whether this is greater than would have been the case if the resources invested in CERN had been spent on other areas of scientific research is impossible to judge. A similar reservation applies to the fourth type of contribution – technological "spin-off." Again, many instances of spin-off have clearly arisen, particularly to firms supplying equipment to CERN, as Schmied [60] has amply documented. However, the "opportunity costs" have also to be taken fully into consideration – is the level of technological spin-off higher than it would have been if the resources spent on CERN had instead been used to support some other type of research, such as exploration of the ocean bed, for example? [42] To this question, there are as yet no ready answers. [43]

This said, it should nevertheless be stressed that we do not feel our assessment is intrinsically weakened by the fact that it has focused almost exclusively on

> CERN's main purpose [which] is to provide Europe's scientists with excellent facilities for high-energy physics research. *All* justification of the investment that is called for from the twelve Member States begins with a belief in the value of such research ([20, p. 262], emphasis added).

Our aim has been to produce systematic, reliable and reproducible conclusions on the extent to which significant contributions to the advance of scientific knowledge have been made by experimental high-energy physicists using the CERN research facilities.

Three main sets of conclusions can be framed on the basis of the assessment outlined above and in Paper I [51]. The first is that, since about 1970 (and with the possible exception of a few years in the mid-1970s when the Fermilab results were having a major impact), the overall record of the CERN machines taken together has been better than that of the accelerators at any other laboratory in the world when judged in terms of experiments producing precise measurements and results with high statistics. Evidence for this comes not only from the figures on total citations (which give some indication of the overall impact of published

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[42] For further discussion of this question, see Irvine and Martin [44].

[43] It is, however, noteworthy that in the attitude survey (described below), nearly twice as many physicists disagreed with the following statement as agreed with it: "The level of resources spent on high-energy physics can be completely justified by the technological spin-off it generates."

work), but also from the peer-evaluation results presented in Paper I [51]. Prior to 1970, this position was held by Brookhaven, and before that (from the mid-1950s until the early 1960s) by Berkeley (with the Bevatron).

The second major conclusion is that, in terms of scientific productivity – that is, scientific performance evaluated in relation to inputs (number of users, funding, etc.) – the record of each of the three US National Laboratories, and the Stanford Linear Accelerator Center in particular, seems to have been significantly better than that of CERN. Supporting evidence for such a conclusion is provided by the figures in tables 8, 10 and 14 in Paper I [51]. It is also noteworthy that, in an attitude survey [44] conducted among the high-energy physicists interviewed in the study, the great majority (71 percent) agreed on the whole with the statement that, "Overall, the American national laboratories have been more cost-effective than CERN in providing experimental high-energy physics facilities," while only 18 percent disagreed.

The third principal conclusion is that, with the exception of neutral currents, the crucial discoveries in high-energy physics between 1961 and 1982 were all made at laboratories other than CERN. As the Director for International Co-operation at the German Federal Ministry for Research and Technology is reported as saying in 1980,

> CERN has been better at building superb accelerators than at discovering spectacular physics (Dr. G. Lehr, reported in Walgate [69, p. 706]).

This feature of CERN's performance was clearly recognized by high-energy physicists whom we interviewed in 1981 and 1982. Of these, 72 percent agreed with the statement, "The CERN accelerators have been responsible for a relatively small number of major discoveries compared with the other main high-energy physics centres," well over three times the number who disagreed (22 percent). Even at CERN, this relative failure did not go unnoticed. In 1980, the Research Director-General admitted that,

> In the last few years, the most important developments in hadron spectroscopy have been those going beyond SU(3) symmetry and connected with the 4th and 5th quarks. Here the role of CERN has been very modest as compared with the discovery of the J/psi at Brookhaven and at SLAC in 1974, the discovery of the first charmed mesons at SLAC in 1976, and the discovery of the upsilon at Fermilab in 1977 (Van Hove and Jacob [67, p. 33]).

These, it should be stressed, are generally regarded as the three most important discoveries of the 1970s, the papers reporting these findings being the only ones in the decade to earn 150 or more citations in a year. Up to 1982, no CERN publication had come close to equalling these figures or having an equivalent impact on the advance of knowledge. However, the situation may have since changed radically with the publication in 1983 of the papers reporting the first observations of the W and the Z particles – likely to be seen as two of the most important discoveries of the 1980s.

During the 1960s, the comparative failure of the Proton Synchrotron to make major discoveries at a time when several were being made on the Brookhaven AGS accelerator can, as we have seen, be explained largely in terms of factors over which CERN itself had little influence – in particular, the relative lack of experience of PS users, the time inevitably taken by a large multinational organization in evolving efficient managerial structures and procedures, and the traditionally hierarchical structure of European research activity. However, in the case of the Intersecting Storage Rings, which clearly *could* have been first to discover the J/psi and the upsilon, several of the main factors identified by physicists as having been responsible are linked to the scientific management of this facility and to decisions taken at CERN. Happily, it would seem that many of the lessons from the experiences with the ISR have been absorbed at CERN, evidence for such a learning process having taken place coming from the very rapid and hugely successful exploitation of the proton–antiproton collider. As for the relative lack of success of the Super Proton Synchrotron in making major discoveries, the crucial factor was the four-year lead held by Fermilab. This cannot be regarded as entirely the fault of European politicians. If the original design for the accelerator produced by CERN had been less expensive, then the American lead would probably have been far shorter.

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[44] The attitude survey consisted of approximately 30 statements relating to issues previously discussed in the interview. Interviewees had to circle a number on a 7-point scale depending on whether they agreed strongly (1); agreed (2); agreed but with reservations (3); were neutral or had mixed views (4); and so on up to (7), disagreed strongly with the statement.

Perhaps the crucial policy question raised by the above analysis is the extent to which the factors that have limited the scientific performance of the CERN accelerators, in particular those which are internal to the CERN laboratory itself, are intrinsic to any large-scale multinational venture of this sort. Is it inevitable that a laboratory where decisions tend to be made by formal committees rather than individuals or small informal advisory groups, and which must be accountable to a dozen political masters, will take less gambles and risks than a laboratory free from such contstraints? Most high-energy physicists interviewed did seem to feel that, "The system by which the allocation of time on CERN accelerators is decided tends to encourage too much routine research rather than highly innovative but risky experiments" (51 percent agreed with this statement in the attitude survey, appreciably more than the 31 percent who disagreed). The question is whether an international laboratory can devise procedures to overcome this conservative tendency towards safe but routine research.

While we shall not attempt to answer this question here, it is necessary to note two points. First, the decision in 1978 to proceed with construction of the proton–antiproton collider did represent a major gamble by CERN, showing that the conservative tendency can be successfully resisted. Second, it would seem that high-energy physics in the United States is now becoming subject to political pressures similar to those exerted on CERN. In the past, the situation in the large US laboratories was such that individuals like M. Goldhaber at Brookhaven, W.K.H. Panofsky at Stanford, and R.R. Wilson at Fermilab, could stamp their authority on the direction in which their laboratories moved. (Directors-General at CERN were limited by the fact that they were appointed for five years only and, except in one case, were not reappointed.) However, there has been a gradual contraction in the number of US high-energy physics laboratories during the 1970s, so that there are now just three major accelerator centres and a single smaller one (and even this may be more than the US will be able to afford in coming years [45]). This means that an increasingly large number of scientific and institutional interests have to be accommodated (and represented) within the decision-making structure at each centre. Unless the United States proves more adept in avoiding the dangers of what many high-energy physicists termed "committee science" than CERN was in the past, [46] then the previous advantage of US physicists over the rest of the world in terms of making most of the crucial discoveries may disappear permanently. To a certain extent, the balance of power in experimental high-energy physics between North America and Western Europe seems, therefore, to have reached a turning point. A full analysis of the future prospects for CERN, and of whether there are indeed such grounds for European optimism in the longer term forms the subject of Paper III [52].

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