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Raisons d'Être

1.1 INTRODUCTION

A scientific laboratory is created with the objective of performing a specific type of research (as defined by the so-called Frascati Manual of 1980; see Appendix 1.1 in this chapter). It can be a multipurpose organization or laboratory aimed at providing several large facilities to a scientific community and establishing a kind of club or forum for this community. It can also be a large, expensive facility, directly used by a scientific community, or a basic instrument requiring further ad hoc installations. The initiator of the idea can be a person, a group of persons, a community, a nation, or a group of nations. The project can be developed from scratch or built within an existing setup.

Such a large undertaking may require a significant fraction of the wealth of its sponsors. Therefore, economic problems will need to be confronted. Above a certain level of resources, in particular under difficult economic conditions, political aspects may become crucial. Also, large installations, wherever they are built, create social spin-offs. Recent experiences show that the managerial, economic, political, and social aspects of such an undertaking can outweigh the scientific and technical aspects. The common criteria for assessing the benefits of a research laboratory as defined in 1975 by the Science and Engineering Research Council of the United Kingdom (SERC) provide a good example of this trend (see Appendix 1.2). Therefore, management tools must be created or developed.

When a scientific laboratory is established, it becomes an asset to the nation or group of nations that have invested in its creation and operation and to the region housing it. Again, any decision on maintaining, extending, or killing it may depend on many factors besides scientific justification.

Examples will be taken from large laboratories funded mainly by public money and devoted essentially to basic research. This may appear restrictive, but because profit or return on investment cannot be used as a yardstick to measure the cost/efficiency, the problems of justification, objectives, and strategy are far more complex and should therefore cover most situations, in particular those relating to smaller units or to other (nonprofit) performance/knowledge-oriented organizations.

1.2 CREATION OF A SCIENTIFIC LABORATORY

A scientific laboratory, when it is the first to be built in its field, is normally created when the public and politicians face a major problem for which no solution is offered by existing knowledge. Examples of major problems are:

- Medical (fear of a disease or an epidemic)
- Environmental (earthquake, storm, flood, pollution)
- Social (unemployment, brain-drain)
- Military (war or preparation for war, energy independence)

Once initiated in a field, the process becomes smoother: The next laboratory does not need a catastrophe to bring it to life, because the argument of competition at the national or international level or the argument of complementarity takes over.

In few, if any, fields (except for astronomy, which conjures up romantic dreams for most people) have large scientific laboratories been created just because scientists felt they were worth exploring:

- The large high-energy physics (HEP) laboratories ultimately derive from the Manhattan Project and Los Alamos Laboratory, created to make the atomic bomb during World War II.
- The Conseil Européan pour la Recherche Nucléaire (CERN) was built to stop the brain-drain of European physicists in the 1950s and as a symbol of the new relations between European nations.

Of course, these laboratories were created also because the related scientific community had its plans ready to proceed at the "politically correct" time. Therefore, the questions to be answered when embarking on the creation of a new laboratory are as follows:

- Is there an organized community ready to propose, build, and support the laboratory?
- Is the general economic situation in good shape?
- What are the possible supporting lobbies?

- Is there a strong nonscientific motivation behind the idea?
- Is the timing politically correct?

If the new laboratory is number *n* in an existing field, it should be possible to answer these questions faster, but this does not mean that its creation is easier.

1.3 DEVELOPMENT OF A SCIENTIFIC LABORATORY

As stated above, once initiated in a field, the process becomes smoother. Governments and the public have accepted the existence of the facilities required by the scientific community. Because an existing scientific laboratory is an asset to the nation or group of nations that have invested in its creation and operation and to the region housing it, any decision on extending its life (e.g., by building a project on its premises) may depend on more than scientific justification.

At the beginning, governments and the public accept or even encourage extension justified either by developments in the field or by competition at the national or international level or by the argument of complementarity. After a certain period, in a successful and popular field, a laboratory faces two problems: size and aging. These affect the management of the laboratory. In general, laboratories start small, managed almost as a family company. All the staff members know each other, their daily contacts encourage team spirit and motivation, and bureaucracy is minimal. The facilities are also small, essentially consisting of prototypes. Then the facilities become bigger and more sophisticated, more of an industrial type. The staff profile is changed by the arrival of people with new skills, different from the pioneers. The age distribution of the staff may change dramatically. Budgets grow accordingly and the laboratory becomes more visible to the world outside it.

After a time, management problems become more acute, and the questions to be answered when embarking on further development of the laboratory are as follows:

- Is the community served still strong enough and motivated to support the development?
- Is the general economic situation still in good shape?
- Are there still supporting lobbies?
- Are the laboratory resources and infrastructure appropriate for absorbing the development?
- Is the timing politically correct?

1.4 RECONVERSION OF A SCIENTIFIC LABORATORY

While a laboratory benefits from rapid growth, it runs the risk of passing a threshold that makes it too visible, if its field loses some momentum in the areas of political power or public opinion. Some laboratories devoted to fundamental research anticipated such an evolution and introduced new fields of research into their programs. These include Brookhaven and Argonne National Laboratories in the United States and the Rutherford Laboratory in the United Kingdom. Some attempts were made at CERN (astrophysics, synchrotron radiation, or fusion), but they did not materialize. One must understand that the scientific community served by the laboratory hates the idea of such diversification and will strongly fight its proponents.

Things may be worse for small laboratories in the sense that the closure of a small unit may have less political and social impact: Small numbers of persons are easier to redeploy elsewhere, and regional lobbies may be less aggressive in defending a small unit. Therefore the motivation to reconvert a small laboratory may not be very strong.

1.5 LIFE AND END-OF-LIFE OF A SCIENTIFIC LABORATORY

Several possible fates for a scientific laboratory can be identified. Some can be clearly defined, while others can be rather controversial.

Success

The real meaning of success may lead to heated debates. In physics, success might be defined by (a) the experimenter, as discovering something totally unexpected, (b) the machine builders, as delivering equipment performing above specifications and on schedule, and (c) the administrator, as keeping within the authorized budget.

In the light of one or more of these criteria, most laboratories, or projects achieved by them, can be identified as successes. The latest large CERN projects can serve as examples. Analyses of the intersecting storage rings (ISR), super proton synchrotron (SPS), and large electron–positron collider (LEP) projects show that they essentially fulfilled most criteria (see Appendix 1.3), except that the cost of LEP went, after due authorization, slightly above budget, which, however, included no contingency (the first CERN project that did not).

The absence of contingency, now recurring in many new projects, raises some concern. In any large scientific project, contingency is an essential element for the cost/efficiency performance: Some unforeseen developments may be expected. For example, the SPS contingency paid for the proton–antiproton program (with the a posteriori blessing of the member states), and the overshoot of the LEP project was essentially due to civil engineering that was overdesigned in view of a future large hadron collider (LHC) project in its tunnel. This flexibility, which could allow major advances for a marginal cost, is now disappearing; it obviously violated all bureaucratic regulations, but it showed itself to be rewarding.

Another important finding is that the success of a project is highly correlated to the talent of its initiator. An initiator is a prominent scientist with long-term vision, an outstanding talent for attracting support from the public and from politicians, and the needed managerial skills to select and put people to work and monitor costs and schedules. He or she should also be able to delegate authority to the right persons.

Stoppage

Fortunately, not many scientific laboratories or projects have suffered a fatal outcome. Several projects have been blocked at the level of technical specifications (e.g., the European Spallation Source, the Tau-charm Factory in Spain, the Kaon Factory in Canada), but never after ground breaking, in Europe. In the United States, however, ISABELLE (at Brookhaven) and the Superconducting Super Collider (SSC, in Texas) were killed after years of work.

The reasons for the demise of the SSC are analyzed in Appendix 1.4. Because the analysis is personal and was done soon after the event, some of the conclusions may be controversial—in particular, the finding that no partner was innocent.

Failure

In view of the various definitions of success quoted above, it is risky to outline failures of scientific undertakings. In fact, the Forum on Megascience set up by the Organization for Economic Cooperation and Development (OECD) decided not to draw up such a list.

To be provocative, we could mention Superphénix (the major French fast breeder reactor), which essentially met none of the criteria for success. The late European Launchers Development Organization (ELDO) had several disappointments when building satellite launchers, until its activities were taken over by the European Space Agency (ESA). A number of national or European attempts at advanced computing can also be listed as failures, as can some attempts relating to fusion on both sides of the Atlantic. All these examples, however, are outside basic research: Technological failures are obviously easier to detect.

Transmutation

A scientific laboratory may be changed into an institution whose real objective is no longer to compete in becoming or remaining the leader in a field of research but to protect an acquired right to exist and to be respected. Its position is then justified by its name and its past achievements and no longer by its potential ability to contribute to the advancement of science. The institution may even become an obstacle to new ideas and developments in its field. As a matter of fact, an attempt to create a monopoly in some field may be a significant symptom of such an evolution.

Such a "transmutation" of a genuine research center into a respectable institution can happen with an aging scientific community and a "pork barrel"-oriented environment.

Further Considerations

Basic research aimed at the development of human knowledge requires supporting instruments. As an example, we can compare some leading fields:

Field	Instrument	Cost of Field
Astronomy	Telescope	1
High-energy physics	Accelerator	10
Space research	Satellite launcher	>20

Funding of telescopes is not too controversial: It is probably below a "psychofinancial" threshold. Funding of satellite launchers implies an extended partnership with industry: It may break even for companies. Accelerators for HEP probably fall in between: above threshold, below break-even.

In parallel with the above developments, applications of accelerators to areas other than high-energy or nuclear physics (medicine, biology, chemistry, material sciences, energy production) have been growing significantly, in particular during the last decade. These applications are based on small or medium-sized machines and are much closer to the immediate needs of the general public and to the interests of industry. They are, therefore, obviously closer to the interests of politicians and governments.

Evolution of Relations with Institutions

Public support is essential for obtaining the resources needed for forefront research, even if the cost is marginal relative to overall public spending. The public perception of research laboratories has evolved over the last decades. Until recently, support for research was considered almost a "must." But a significant change has occurred: The public now demands more justification for a given field of research, and resistance is developing against the "big" science considered to be costly and esoteric. The evolution of the relationship between the people and accelerator-driven research is analyzed below as a typical case study, applicable also to other fields of research.

One of the main problems encountered by the accelerator community is that the public now considers accelerators to be one of the leading tools of "big science." For many decades, accelerators were developed essentially to serve what is sometimes called "heavy" research (civilian or military). Governments and politicians were strong supporters of high-energy and nuclear physics and accelerators, especially when it was thought that they could have some future military application. This was very beneficial to the field: From 1945 to 1990, military budgets were more than protected. In addition, public opinion was in favor of "big science"—large, impressive, and adventurous technical projects. There was a strong constituency in the scientific community, because of interest in the field and also because, with this political support, resources came from "special" funding. During this "golden age," industry was involved as a supplier, not really as a partner: The argument was that the things needed were so specific that only "our" scientists could design and develop them. In recent years, governments have been cutting civilian and military research budgets: The people in high-energy physics have to fight for funds as do those in biology, material sciences, social sciences, and so on. In general, these resources come from the same global budget item.

The field of large accelerators has come to a turning point (see Appendix 1.5). The sad saga of the SSC illustrates that. Everyone knows about the press campaigns, the numerous reports, and the debates in Congress: They had an international impact. For many people the SSC, called a "mega-project" even by members of the accelerator community, was a symbol of arrogant "big science." Proponents of "small is beautiful" now take the lead, even in science. (Some scientists seem to be unable to understand that money taken from a given scientific field does not often go to their own field or even to some other scientific field: Where has the SSC money gone?). Industry gave some support to the SSC, though probably too late, and it is not clear that the parties really understood each other's interests and potential. Messages such as "We work with a constant or reduced fraction of the GNP," "Our lab's energy consumption is less than that of a jumbo jet," and "Our machine costs less than an aircraft carrier" do not work anymore: "The SSC is a behemoth," and "It is not worth investing 12 billion dollars in the SSC or 3 billion Swiss Francs in the LHC to find the Higgs boson" are typical answers given now.

The scientific community needs to think ahead and try to find ways to reverse these trends. Industries have developed considerable ability in designing and developing high-tech projects. They are qualified and reliable partners. Accelerators have a good record of "useful" applications and a strong potential for more and better ones, and the laboratories can transfer to industry technology that will be productive in the long term rather than immediately. If the accelerator community seriously considers these developments, loosens its ties to pure research, puts more emphasis on applications of its machines, and strengthens its links to industry, it should improve its image in the eyes of the public, which should then, through the democratic process, favorably influence governments. At present, especially in the United States, there is some hesitation about what strategy to choose. In 1994, the Galvin report recommended that national laboratories should concentrate on the mission of doing fundamental research, applied research being the job of industry. In 1996, a composite subpanel of the High-Energy Physics Advisory Panel (HEPAP) was asked by the U.S. Department of Energy (DOE) to assess the status of accelerator physics and technology. Its proposals, detailed in Appendix 1.6, step back slightly from the Galvin report.

Further reflection is needed on the problem of relations between industry and scientific laboratories.

1.6 THE LABORATORY

When the decision to build or to develop a laboratory has been taken, other tasks are given to the managers. Assuming that the political and social problems discussed above are under control and that the scientific and technical cases are set, the following managerial functions will be required:

- To generate corporate planning
- To draw an organizational chart
- To create internal and external communication channels
- To organize the management and administration of human resources
- To organize the management and administration of financial resources
- To implement purchasing and supply services
- To organize the logistics support
- To organize the management of the site and its buildings
- To set up all the other general services

The establishment of these functions is progressive and iterative. However, it is best to set them up or at least carefully analyze them from the start. Flexibility for further adaptation is mandatory, but a managerial problem will generate far more difficulties if it is ignored at the creation of a laboratory than if it is, at least partially, solved.

1.7 CONCLUSIONS

The creation of a new laboratory occurs when scientific ambitions and political and economic interests converge. It represents an important investment to be made by a national or international community. Therefore, the decision to use the taxpayer's money is never taken on scientific grounds alone: The managerial, economic, political and social aspects are important to the project's life and death.

In addition, there is no example of the construction of a scientific laboratory or of a large project without an outstanding personality as its initiator (either officially or behind the scene). Scientific communities, groups, and politicians may be smart followers (though sometimes obstacles) but not genuine initiators. However, even outstanding personalities may be wrong on specific issues. To add a touch of humor, Appendix 1.7 illustrates these statements.

By the time management is in place, the scientific case has already been made. Therefore, a primary role of the laboratory management, besides setting up the internal managerial system, is to concentrate on a strategy for dealing with the external world: political, industrial, and regional. Management has to make its case through an impact study detailing basic requirements and consequences with regard to the scientific community, the level of resources, the environment, industry, and so on, in order to anticipate future difficulties. Management will have to compete with other candidates for money. It should never underestimate its competitors or opponents.

Finally, the utmost importance of promoting science must be emphasized. New laboratories will be created and old ones reconverted if, and only if, healthy science programs are undertaken throughout the world. After years of down-sizing, some light may be seen at the end of the tunnel, at least in Japan and maybe in the United

States. In Europe, the prospects remain worrysome. Appendix 1.8 outlines the present situation.

APPENDIX 1.1 DEFINITION OF RESEARCH

The 1980 Frascati Manual (OECD) proposes the following definitions:

Basic research is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundations of phenomena and observable facts, without any particular application or use in view.

Basic research analyzes properties, structures, and relationships with a view to formulating and testing hypotheses, theories, and laws. The results of basic research are not generally sold but are usually published in scientific journals or circulated to interested colleagues. Occasionally, basic research may be "classified" for security reasons.

Basic research is usually undertaken by scientists who may set their own goals and to a large extent organize their own work. However, in some instances, basic research may be oriented toward some broad fields of interest. Such research is sometimes called "oriented basic research."

Applied research is also original investigation undertaken in order to acquire new knowledge. It is, however, directed primarily toward a specific practical aim or objective.

Applied research is undertaken either to determine possible uses for the findings of basic research or to determine new methods or ways of achieving some specific and predetermined objectives. It involves the consideration of the available knowledge and its extension in order to solve particular problems. In the Business Enterprise sector the distinction between basic and applied research will often be marked by the creation of a new project to explore any promising results of a basic research program.

The results of applied research are intended primarily to be valid for a single or limited number of products, operations, methods, or systems. Applied research develops ideas into operational form. The knowledge or information derived from it is often patented but may also be kept secret.

Experimental development is systematic work, drawing on existing knowledge gained from research and practical experience, that is directed toward (a) producing new materials, products, and devices, (b) installing new processes, systems, and services, and (c) improving substantially those already produced or installed.

In the social sciences, experimental development may be defined as the process of translating knowledge gained through research into operational programs, including demonstration projects undertaken for testing and evaluation purposes. The category has little or no meaning for the humanities.

APPENDIX 1.2 COMMON CRITERIA TO ASSESS BENEFITS OF A RESEARCH LABORATORY

In 1975 the Science and Engineering Research Council of the United Kingdom (SERC) was given the following instructions:

Councils and their Boards/Committees/Groups are invited to use the criteria listed here to discuss and compare relative benefits.

Whenever practicable, reference should be made to objective data in support of the assessment (e.g., demographic data, social costs, relevant government expenditure, etc.) in relation to the cost of the research.

Scientific Policy Criteria

1. *Excellence of Study Field.* Where benefits are attributable to a high proportion of the research being intrinsically of high intellectual value.
2. *Excellence of the Research Workers.* Where benefits are attributable to the exceptional quality of the individuals or teams to be employed in the activity.
3. *Pervasiveness of the Activity.* Where benefits include the impetus to advances in other and related fields of science in addition to the primary field.
4. *Social and/or Economic Importance.* Where expected benefits arise from the work being directed to supporting social or economic aims.
5. *Significance for the Training of Scientific Manpower.* Where benefits will include training and experience for scientific research workers.
6. *Educational Importance.* Where benefits will include a contribution to education.
7. *Significance in Maintaining National Scientific Prestige.* Where benefits will contribute to national reputation.

Management Criteria

A set of selected management criteria are also offered. These apply to the consideration, from a management point of view, of alternatives which have already been assessed on the scientific policy criteria.

A. *Efficiency of Operation.* Where improvements in organization and/or plant would lead to a general increase in efficiency.
B. *Obsolescence.* Where the maintenance of a capability (at whatever level of activity) requires replacement within the Forward Look period of a major item of obsolescent plant or equipment.
C. *Timing.* Where a start on a new or increased activity within the Forward Look period is critical if the expected benefits are not to be lost or much reduced.
D. *Dependence on Science Budget Support.* Where there is likely to be limited support, national or foreign, available for work related to the activity except the Science Budget.

E. *Availability of Scientific Manpower.* Where an activity attracts priority by virtue of greater availability of scientific manpower for it (or its execution is constrained by lack of it).

F. *Scope and Limits of Redeployment.* Where the priority accorded to an activity is conditioned by difficulties or opportunities of redeployment.

APPENDIX 1.3 CERN LARGE PROJECTS, PERFORMANCES IN COST AND SCHEDULE

One often reads that CERN has a great record in completing its projects on time and within budget. It is worth quantifying this statement, which in most cases is qualitative. Therefore, a survey of the three largest CERN projects, approved and managed as supplementary programs for ISR and 300 GeV, as part of the basic program for LEP, was undertaken to measure, beyond their technical performances, how their cost and schedule have been managed.

The ISR Program

The ISR Supplementary Program, as approved by the CERN Council in June 1965, provided for the construction of (a) two intersecting storage rings, connected to the proton synchrotron, for the purpose of storing protons up to about 28-GeV, with their associated infrastructure and equipment, and (b) two colliding-beam halls and one 25-GeV hall. It extended over a period of six years.

The actual construction work started in July 1966, the ISR was commissioned in January 1971, and the program was completed in March 1971.

The planned cost of the ISR Program was 332 million Swiss francs (MCHF) and the actual cost was 326 MCHF (both at 1965 prices). Both costs included personnel expenditure.

The 300-GeV Program

The aim of the 300-GeV Supplementary Program, as approved by the CERN Council in February 1971, was to provide in Europe facilities that would enable particle physics research to be carried out at incident proton energies of at least 300 GeV, the principal facility being a proton synchrotron with an internal beam intensity of at least 10^{12} protons/s. It also provided for the use of the existing PS as injector and the existing West Hall as first experimental area and the construction of a new experimental hall in the so-called North Area. It extended over a period of eight years.

The actual construction work started in September 1971 and, though the overall program was completed in February 1979, the SPS machine itself was commissioned in June 1976.

The planned cost of the 300-GeV Program was 1,150 MCHF and the actual cost was 1,089 MCHF (both at 1970 prices).

The LEP Project

The aim of the LEP Project, as approved by the CERN Council in December 1981, was, in its first phase, to build a machine accelerating and storing electrons and positrons in its main ring to reach an energy of 50 GeV, at a luminosity sufficient for the initial research experiments. The project also included the construction of the two linacs and the accumulator ring as well as the necessary modifications to the existing PS/SPS machine complex to enable it to accelerate electrons and positrons up to 22 GeV energy. It extended over a period of seven years.

The actual construction started in July 1983, and the LEP collider was commissioned in August 1989, completion date of the LEP Project.

No contingency was included in the planned budget (unlike the ISR and 300-GeV budgets, which included about a 12% contingency). The CERN Council had decided that time would be used as the contingency: Accordingly, in 1985 and 1987 it adjusted the costs and the completion dates. Therefore changes in cost, financial profile, and schedule were made under the strict control and with the approval of the Member States.

It is worth noting that the worst surprise concerning LEP costs came from the civil engineering. This was also true for the SPS (but the overshoot was absorbed by the contingencies). On the contrary, ISR had a good surprise, probably due to the difference in volume between the relatively small ISR and the other two large facilities, which required huge civil engineering contracts. This is rather encouraging when considering the LHC project, for which civil engineering represents only a minor fraction of the cost.

The planned cost of the LEP Project was 890 MCHF and the actual cost was 1,053 MCHF (both at 1981 prices). Staff costs were not included; they are estimated at 500 MCHF.

Summary Table

The projects in Table 1.1 cover almost three decades in the history of CERN: ISR in the 1960s, 300 GeV in the 1970s and LEP in the 1980s. The management of these projects and of CERN, the Member States, and their delegates, and also economic

TABLE 1.1 Projects

Schedule and Cost[a]	ISR	300 GeV	LEP
Starting date	July 1965	February 1971	January 1982
Planned completion date	July 1971	February 1979	January 1989
Actual completion date	March 1971	February 1979	August 1989
Change: completion/planned	−6%	0%	+8%
Planned cost (MCHF)	332	1,150	890
Actual cost (MCHF)	326	1,089	1,053
Change: actual/planned	−2%	−5%	+18%

[a] 1965 prices for ISR (personnel included), 1970 prices for 300 GeV (personnel excluded), 1981 prices for LEP (personnel excluded).

conditions and the constraints imposed on CERN, changed significantly over this period. Nevertheless, it appears that, "mutatis mutandis," the CERN tradition of respecting time schedules and budgets has been preserved over these decades.

APPENDIX 1.4 WHY WAS THE SSC PROJECT KILLED?

The SSC project was first proposed by the DOE in 1983. It was a response to the lead taken in high-energy physics by CERN and its UA1 experiment. It could be compared to the response to Sputnik by the Apollo project. The performance and size of the SSC were to be designed so that no competing machine could be built on another continent. The objective, clearly stated, was to give the United States world supremacy in the field.

A technical design was developed, according to the scientific options defined by the potential users' community. The proposal was elaborated by the DOE and submitted to Congress in 1987. A site was chosen in Texas in 1988, and work was started in 1989. After threats from Congress in 1992, the project was pronounced dead in 1993. About 2 billion dollars had been spent and more than 2,000 people lost their jobs.

Possible reasons for this failure are developed below. They are personal interpretations and may be challenged. The reasons are listed in chronological order and without weight or priority because probably their combination (in a period of public concern about federal deficits and big science) led to the abandonment of the SSC.

Lack of Understanding of the International Situation

When asking for approval, the DOE authorities told Congress that an important part of the funding (25% to 30%) would come from nonfederal sources. Some funds came from Texas, but almost none came from abroad.

An initial error was to design the project with very limited participation by non-U.S. scientists (who in any case could not commit their funding agencies). Even if the scientific case was excellent, many non-U.S. physicists felt some resentment for what they considered a parochial approach.

A second error was to overestimate the power of diplomatic pressure from the DOE and the State Department to obtain contributions from European countries while underestimating the attachment of these countries to CERN. No European country wished to jeopardize CERN's position, even if they saw merit in sending groups of physicists to participate in the SSC experiments.

A third error was to try to impose U.S. procedures and contractual commitments on non-U.S. funding agencies. Procedures were quite different on other continents, and formal commitments were almost impossible to obtain: For example, the participation of national institutions in the LEP experiments always implied a significant margin of flexibility; had formal commitments been compulsory, these experiments might never have been built.

A fourth error was to leave uncertainty about down-to-earth problems such as visas, education facilities, and work permits for the families of foreign experimenters.

Wrong Sale Messages Sent to the Public

The public relations of the project were organized by the DOE. They were done very professionally, with the help of members of the scientific community, but with a highly commercial approach somehow incompatible with the seriousness required for scientific undertakings. Expected technical transfers were overemphasized, applications to medicine and industry were almost taken for granted (which, in fact, may or may not have been realized), and economic benefits and the potential creation of thousands of jobs were overestimated.

The public was not so sure that the results would match the expectations; it blamed the scientific community, not the public relations "experts."

Another approach that had a wrong impact was the excessive use of words such as giant, super, and so on, and the anticipated growth of the community of SSC users; this probably worried both the political authorities and the people in other fields of research. They feared that the growth of this field could result in an inflation of instrument needs—that is, a progression from "big" science to "mega" science.

Wrong Budget Tactics

- In May 1984, the DOE issued the first official estimate of the cost of the project: between 2.7 and 3.1 billion dollars (1984 costs).
- In 1988, for the preparation of the federal budget for FY 1989 the DOE quoted figures between 3.9 and 4.8 billion dollars (1988 costs).
- In October 1988, the Congressional Budget Office (CBO) reviewed the project and estimated its costs between 4.5 and 6.4 billion dollars (1988 costs).
- In January 1989 (after the Texas site was chosen), the DOE rejected the CBO assumptions and insisted that the correct figure was 4.4 billion dollars (1988 costs).
- In January 1991, the DOE published a detailed cost estimate giving a final cost between 7.8 and 8.2 billion dollars (1990 costs).

Later, the DOE did not change this figure (at least officially: their real latest estimate was probably around 10 billion dollars) and said that, in spite of the dates of their publication, the previous figures were not based on the Texas site. However, both Congress and the public had lost confidence in the DOE estimates: All kinds of uncontrolled figures started to float around, and people were inclined to believe the highest.

Top-Heavy Organization

A very sophisticated multireporting system, implying the line hierarchy of the DOE but with a bypass to the Secretary of Energy, was combined with an unclear definition of the roles of the Laboratory director and of the DOE field office.

This system, added to numerous and frequent requests for information from the many task forces, panels, committees, and so on, made the life of the laboratory impossible. It also generated a highly developed internal structure: The SSC management, even with 15 associate directors, hardly had enough persons to prepare for the hearings requested by the above-mentioned task forces, panels, and committees, and most of its work was devoted to bureaucratic obligations rather than to scientific, technical, or even strategic tasks.

In conclusion, it can be seen from the above that, even with a good scientific basis and a staff of highly professional experts, a project can fail if the strategic options and the administrative process go in the wrong direction and if some actions are overdone.

APPENDIX 1.5 THE EVOLUTION OF ACCELERATOR LABORATORIES

The Livingston plot shown in Figure 1.1, and a similar plot for colliders in Figure 1.2, show the evolution of the "performance" of accelerators.

During the last 40 years, the "glorious period" of accelerators, "performance" has risen by more than two orders of magnitude. Costs, however, have increased only by a factor of five, as seen in Figure 1.3, which shows the material costs of the machines built at CERN since 1955.

Nevertheless, this kind of argument, used successfully until the mid-1980s, is no longer a decisive one to justify new projects. Above a certain threshold in cost, increased efficiency is not enough, as illustrated in Figure 1.4.

Figure 1.4 shows the smoothed evolution, over four decades, of the fraction of the gross national product (GNP) devoted to high-energy physics (all costs of all laboratories, including personnel, costs incurred at universities, overhead, etc.) in the United States and Europe and an approximation for Japan. The share of national wealth given to high-energy physics is seen to drop significantly for the United States and Europe. This share has been almost halved in the United States. In Europe, even with the LHC, it has dropped to about three-quarters of its 1966 value and will continue to decline after the 1996 CERN Council decisions. It remains fairly stable in Japan, but at a lower level than the others.

APPENDIX 1.6 EXTRACTS FROM THE HEPAP COMPOSITE SUBPANEL REPORT

The Director of the DOE Office of Energy Research has charged a composite subpanel of HEPAP with the task of assessing the status of accelerator physics and

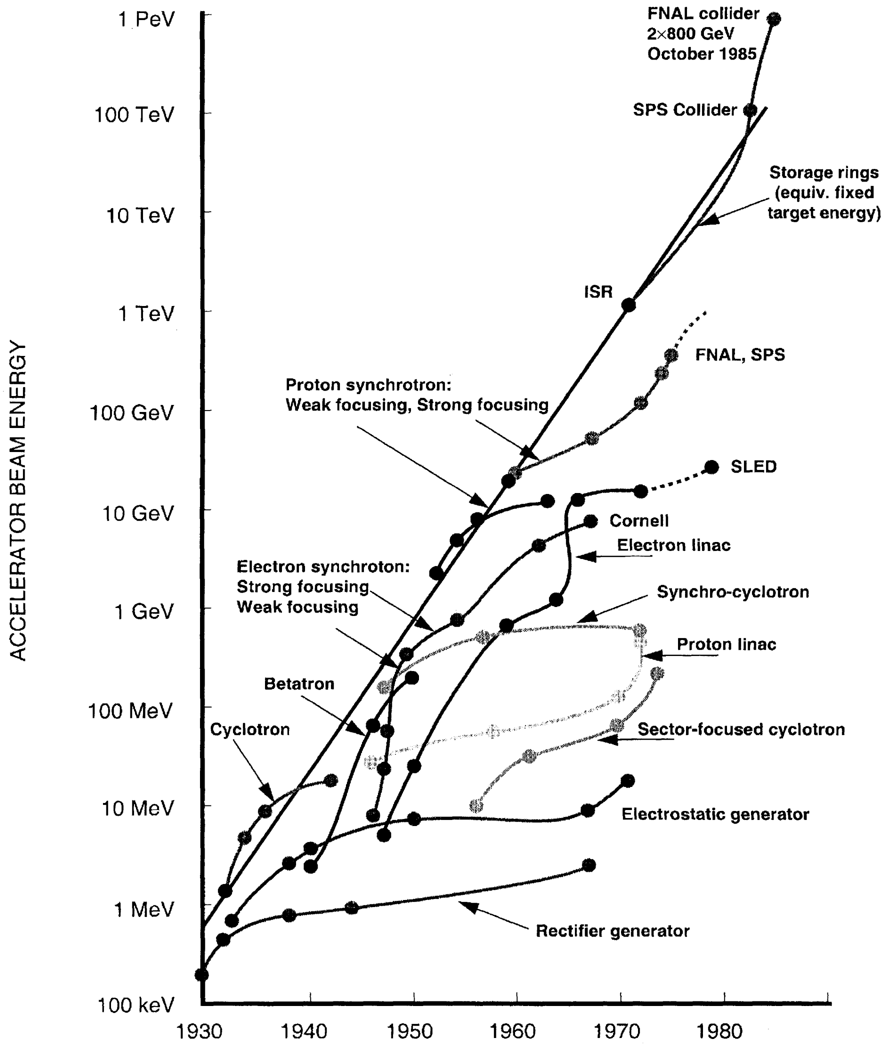

Figure 1.1 Livingston plot.

technology. One aspect the subpanel has examined is interaction with the private sector, in light of the SBIR (support to small business innovation research) and CRADA (negotiated agreement between a laboratory and a commercial entity) programs. The report of the composite subpanel states the following:

> "Generally, collaboration between laboratories and the private sector for technology transfer has had mixed success because: (1) rigid procedural requirements, including intellectual property issues, make cooperative research between the labs and the private sector difficult; and (2) organizational cultures differ in perspectives and value systems,

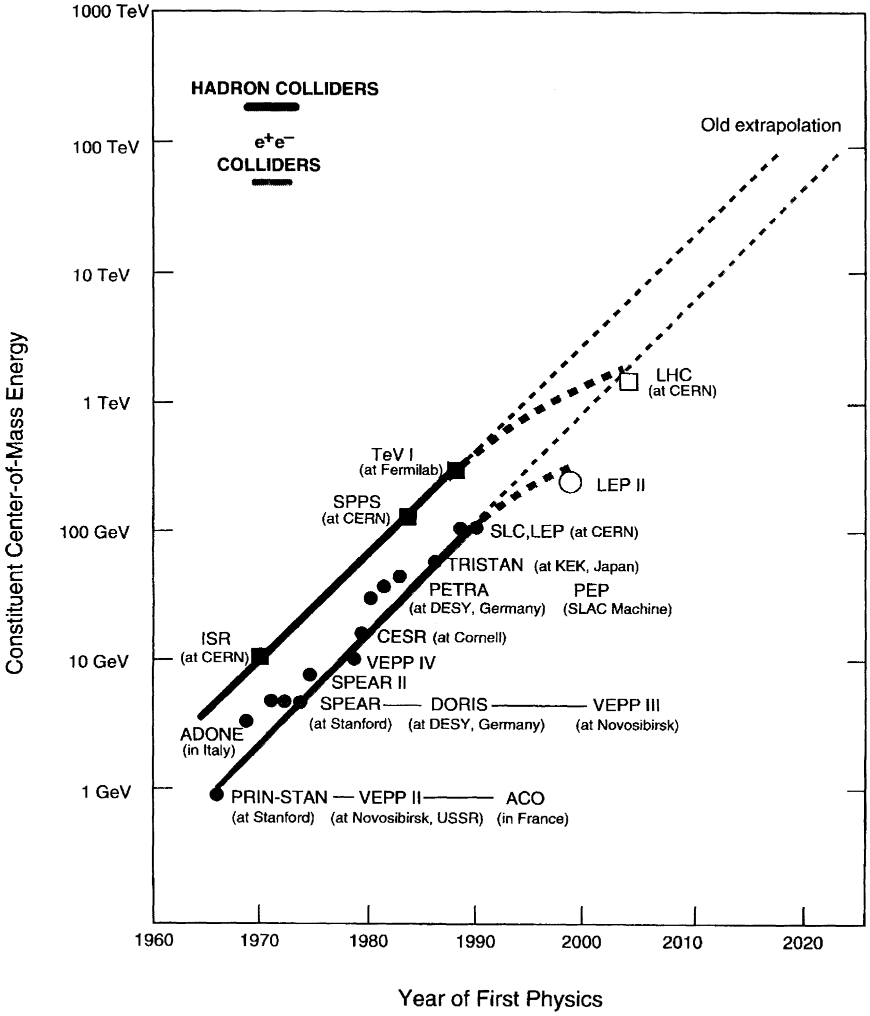

Figure 1.2 Adaptation of the Livingston plot for colliders (as plotted at CERN in 1990), updated by J. P. Revol and C. Roche in 1999).

especially with regard to views on cost, schedules, the concept of deliverables, and optimization strategies for generic technology development.

During the 1990s, three important studies involving the role of federal research in fostering economic vigor and competitiveness have been completed:

- Alternative Futures for the Department of Energy National Laboratories (the so-called Galvin Committee Report), DOE, February 1995
- Energy R&D: Shaping Our Nation's Future in a Competitive World (the so-called Yergin Panel Report), DOE, June 1995

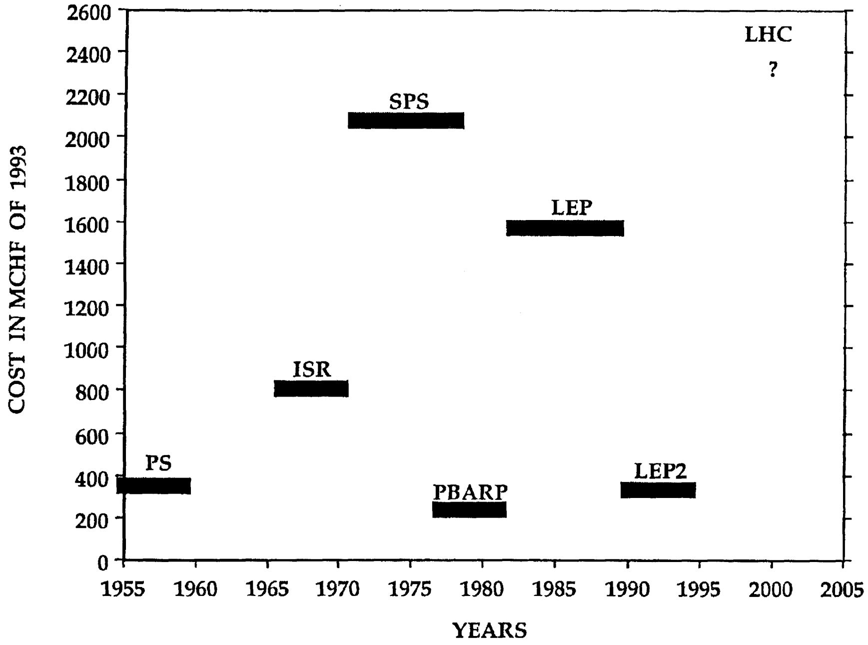

Figure 1.3 Construction costs of CERN machines.

- Allocating Federal Funds for Science and Technology, National Academy of Sciences, December 1995

All three reports recognize that federal science has only a limited role in contributing to industry; however, they also recognize that private sector R&D is concentrating on shorter and shorter time horizons. Thus federal research facilities can make a contribution if the particular technology is closely related to a core mission of the laboratory and if it is not something that industry would fund on its own anyway. The last two reports acknowledge that there are areas in which the national interest is not served by the market alone, such as new enabling or broadly applicable technologies.

In analyzing industry–laboratory interaction, the Subpanel has reached the following conclusions. First, some (perhaps many) national laboratory R&D activities are not relevant to industrial technology commercialization. Second, the highest probability of successful technology transfer occurs when there is user (industry) pull as opposed to technology push from the laboratories, that is, a perceived need should be the focus for identifying a commercialization opportunity. Third, the critical interface necessary for successful transfer and adoption of the technology involved is people-to-people contact. With these findings in mind, we offer the following suggestions for improving the impact of accelerator technology on society.

1. Laboratory technologies should be better publicized to industry. Experience with the Fermilab Industrial Affiliates Association and similar organizations

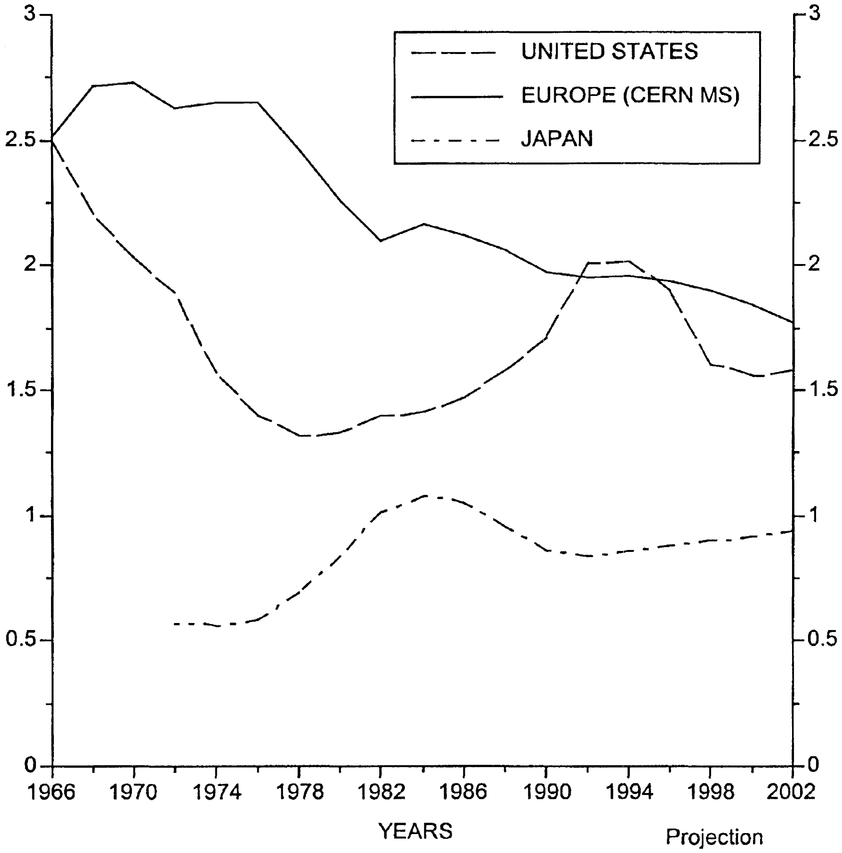

Figure 1.4 Faction of the GNP devoted to HEP.

elsewhere indicates that industry will make an effort to understand the technologies.

2. Protocols for laboratory–industry interaction should be designed to minimize administrative and funding delays in the execution of cooperative projects.
3. Laboratory managers should increase emphasis on the transition of laboratory technologies to the private sector and encourage scientists and engineers in their organizations to assist in transferring technology.

These issues must be addressed if DOE is to become more effective in contributing to U.S. competitiveness. In addition, the Subpanel believes that more attention should be paid within the DOE to providing an environment in which emerging technologies that do not fit into mainstream programs can be nurtured and possibly develop into mainstream programs or into spin-offs."

APPENDIX 1.7 SOME INTERESTING QUOTATIONS

"Heavier-than-air flying machines are impossible." *Lord Kelvin, 1895*

"Everything that can be invented has been invented." *Charles Duell, Commissioner, U.S. Office of Patents, 1899*

"Anyone who expects a source of power from the transformation of these atoms is talking moonshine." *Lord Rutherford, 1933*

"I think that there is a world market for maybe five computers." *Thomas Watson, Chairman of IBM, 1943*

"There is no reason for any individuals to have a computer in their home." *Ken Olsen, Chairman of DEC, 1977*

"The peer review system, especially in years of financial restraint and subsequently low award rates, encourages research proposals in the current mainstream of research and is biased against unconventional, innovative or high risk research." *Commission of European Communities, 1991*

APPENDIX 1.8 EVOLUTION OF RESEARCH FUNDING

Research is one of the fundamental tools that ensures the healthy progress and development of a nation or a group of nations. For many decades, research was one of the catalysts of military confrontation between the power blocks. Today, military competition has given way to other forms of confrontation, particularly commercial.

The Situation in Europe

Europe, which played only a supporting role in the military power struggle between 1945 and 1990, made considerable advances in fundamental research during the same period with the creation of CERN, the European Southern Observatory (ESO), the European Molecular Biology Laboratory (EMBL), and ESA. It seems that the continent was far-sighted and intended to make full use of the fruits of its research, not for military purposes but to gain the peace. At the time, the two power blocks, the United States and the Soviet Union, were preoccupied with military matters, the cold war, which had the interesting consequence that all their research budgets were more than healthy. During its reconstruction period, Japan was happy to pick up the results of fundamental research carried out elsewhere.

Since the fall of the Berlin wall, the objectives and rules of the game have changed. Research, stimulated in the United States and Soviet Union by military considerations, has stagnated. One might conclude, therefore, that Europe would have taken full advantage of its foresightedness in maintaining or even developing its research capacity.

Quite the Contrary! During the 1990s, a fierce battle raged among those in charge of European research to see who could make the most drastic cuts. Words like "rigor," "cost cutting," and "re-engineering" are heard, never "ambition," "diversification," or "growth." From 1990 to 1996, the reduction in research funds as a proportion of the total public spending in Europe was similar to that observed in the United States. However, in the latter case, a large component of this is that the United States has reduced its financing of the "star wars" strategy.

Other Continents

In 1996, Japan (where fundamental research funding has been slowly but steadily increasing) was the first country to react to the budget cuts of its European and American partners by increasing public funding on fundamental research by 50%.

The United States at the instigation of Senator Gramm (one of the initiators of the Gramm–Rudman law, designed to reduce the federal deficit), has been examining since early 1997 a draft bill that proposes to double the portion of the federal budget allocated to fundamental research over the next 10 years. The additional funding would be managed by the federal departments involved in fundamental research and allocated on the basis of a peer review system to noncommercial activities (defined as research up to the precompetitive stage).

While Europe is quick to copy its American cousins in certain of their perhaps less-than-desirable aspects, it appears to be reticent when it comes to some of their better ideas. It is to be hoped that this time Europe will react rapidly, realize there is wisdom in recognizing its mistakes, and reinstitute a policy of growth in research funding.

Figure 1.5 shows a possible evolution of the fraction of Gross National Product (GNP) devoted to R&D in Japan and the United States, if the proposed increases are implemented, and in Europe, if the present policy of cutting research funding to balance government deficits continues.

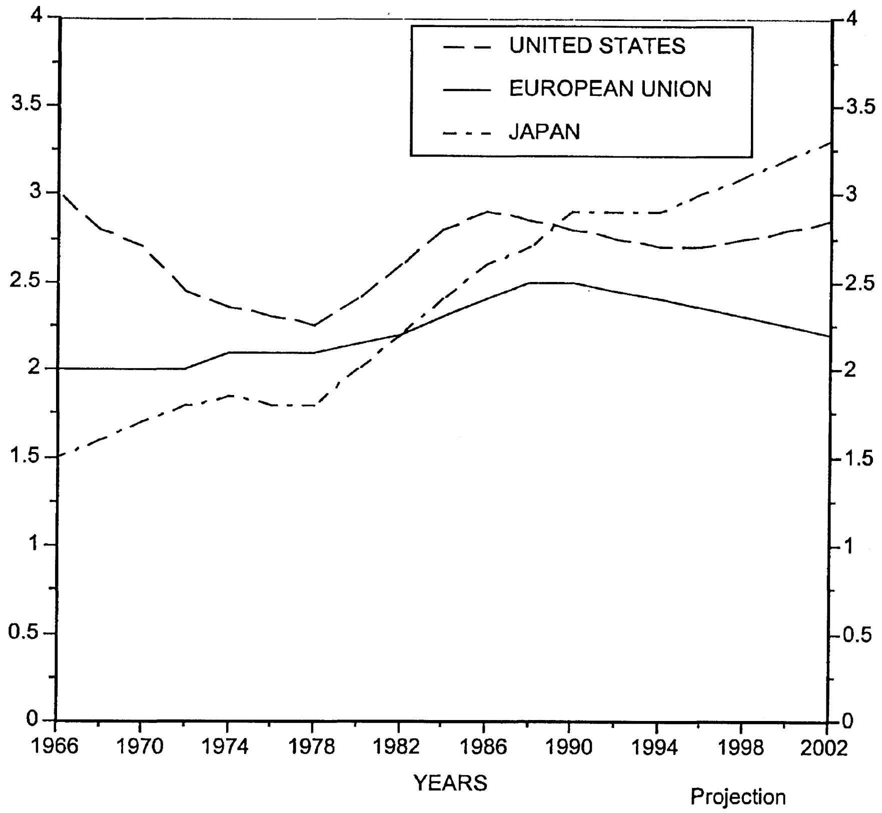

Figure 1.5 National expenditure on R&D as percent of the GNP.

Remark

Some persons, including scientists, say that increases in research funding are possible only in a period of economic growth.

Such a statement can be challenged. The *Wall Street Journal* undertook a survey among 1,500 economists randomly chosen from the faculties of 100 leading U.S. university economics departments and 10 major business schools. The results were published in March 1997.

To the question: "Which policy would have the most positive impact if the federal government wanted to increase long-term economic growth (gross domestic product per capita)," the answers were:

Spend more on education and research and development	43%
Reduce government spending as a percentage of GDP	10%
Replace the income tax with a consumption tax	8%
Spend more on infrastructure	7%

Cut marginal income tax rates	6%
More deregulation	6%
Balance the budget by 2002	5%
Cut the capital gains tax	5%
Other	10%