This paper describes the scientific, technical and political genesis of the Large Hadron Collider (LHC). It begins with an outline of the early history of the LHC, from first thoughts and accelerator and detector developments that underwrote the project, through the first studies of the LHC and its scientific potential and the genesis of the experimental programme, to the presentation of the proposal to build the LHC to the CERN Council in December 1993. The events that led to the proposal to build the LHC in two stages, which was approved in December 1994, are then described. Next, the role of non-Member State contributions and of the agreement that CERN could take loans, which allowed single stage construction to be approved in December 1996, despite a cut in the Members’ contributions, are explained. The paper concludes by identifying points of potential relevance for the approval of possible future large particle physics projects.

1. Beginnings and background

(a) First thoughts

The Large Hadron Collider (LHC) story began in 1976 when the European particle physics community began to discuss building a Large Electron Positron (LEP) collider at CERN. LEP was, of course, eventually built and installed in a 27 km tunnel, which today houses the LHC.

At that time, CERN had two Directors General—Leon van Hove, the Scientific Director, and John Adams, the Technical Director. John thought LEP was the wrong choice for CERN’s next major project, but in 1977 conceded that he had lost the argument and wrote a note proposing that the LEP tunnel should be made large enough to accommodate a ring of superconducting magnets to enable the acceleration of protons to at least 3 TeV [1]. This suggestion was widely known by the time of the LEP Summer Study in September 1978.
Figure 1. Major particle physics facilities in Europe and the USA from the mid-1950s to the late 1990s. Construction and operational phases are indicated by dotted and solid lines, respectively. (Online version in colour.)

(b) Historical background

Following the start-up of CERN’s Proton Synchrotron (PS) in 1959, it was generally assumed that CERN’s next major project should be a larger Super Proton Synchrotron (SPS) located at a new laboratory. Up to that time, all accelerators had been built on the surface. There was clearly not enough space near CERN to accommodate the SPS on the surface, and in any case it was thought desirable to start a second European particle physics laboratory at a new site.

The proposal to build the SPS stalled when the CERN Members were unable to agree on a site, and the UK proved unwilling to participate in what they considered an unacceptably expensive project. A way forward was found in 1970 by John Adams, who had left CERN after leading construction of the PS but had been invited back as SPS project leader, although the project was not approved. Adams pointed out that advances in tunnelling made it possible to build the SPS underground at CERN, at much the same cost as on the surface, while the total cost of the project would be reduced because the SPS could use existing infrastructure, including the PS which could serve as the injector, and the cost of setting up a new laboratory would be avoided [2]. The upshot was that the SPS was built at CERN and subsequently Europe put all its high-energy physics eggs in one and a half baskets, rather than three or four as in the USA, as illustrated in figure 1 (the half basket being DESY which never competed directly with CERN for funding and where, before HERA, all construction projects had been entirely funded by Germany). This underwrote Europe’s rise to pre-eminence in experimental high-energy physics and enabled the construction of the LHC with an annual CERN budget lower in real terms than in the early 1970s.

(c) Developments from 1978 through 1983

Key factors in the pre-history of the LHC were the approval of LEP in 1981, the 1978 decision to adapt the SPS to collide protons with anti-protons, and the successful operation of the SppS
collider in 1982, leading to the discovery of the W and Z bosons in 1983. Meanwhile in the USA, construction of the 400 × 400 GeV proton–proton collider Isabelle at Brookhaven had run into trouble as (in words taken from the Brookhaven website) ‘problems with the magnets, with double-digit inflation and political factors caused the US physics community to lose enthusiasm’ for the project. The discovery of the W and Z at CERN, which led to a headline ‘Europe W, USA not even Z0’ in the New York Times, proved to be the last straw (although by then the problems with the novel 2-in-1 Isabelle magnets had been resolved). In early July 1983, a committee appointed by the US Department of Energy’s High Energy Physics Advisory Panel (HEPAP) recommended that Isabelle should be abandoned and the USA should push for a 20 × 20 TeV Superconducting Super Collider (SSC).

The advocates of the SSC asserted that 20 × 20 TeV was necessary in order to explore constituent collisions at centre of mass energies up to 1 TeV, where it was expected that new phenomena would be found. European particle physicists recognized that the SSC would enable a major step forward. However, many wondered whether reaching 1 TeV required 20 × 20 TeV and suspected that this energy had been chosen partly because it was at least a factor two larger than that of a possible LHC in the LEP tunnel. In any case, the US community quickly rallied behind the SSC, encouraged by George Keyworth, President Reagan’s science advisor, who urged them to ‘think big’ and go for a project that would allow the USA to ‘regain leadership’ in high-energy physics.

It is sometimes asked why, in response, CERN’s Director General (Herwig Schopper) did not propose that the Americans should instead join CERN in building the LHC, which would be cheaper as it would reuse the LEP tunnel (as well as the PS, SPS and other existing infrastructure) and be smaller, although the energy would inevitably be lower. The answer is that the CERN Council would not have underwritten such an invitation by committing to the LHC (which was then just an idea) so soon after committing to build LEP, and before excavation of the LEP tunnel had even begun. Schopper did, however, make it clear that if the LHC was ever built, it would be open to international participation, and he launched a first study of the LHC in the winter of 1983–1984 in order that it could be presented at the International Committee for Future Accelerators (ICFA) seminar—a global high-energy physics summit conference—which was due to take place at the KEK laboratory in Japan in May 1984. In preparation for the ICFA seminar, the first LHC workshop, which was followed by a two-day open presentation which filled the CERN auditorium, took place in Lausanne in March 1984.

(d) Enabling technical developments

Construction of the LHC was enabled by the technical success of the proton–proton Intersecting Storage Rings (ISR) and the Sp̅pS, and the accelerator skills that building and operating these machines fostered at CERN. Prior to the construction of the ISR, it had been argued that proton colliders might not work due to the absence of significant synchrotron radiation, which damps oscillations in electron machines. In the case of the Sp̅pS, the concern was that interactions between the beams, which were bunched (in contrast to the situation in the ISR), could lead to instabilities that would undermine its performance. The beam–beam tune shift did limit the performance of the Sp̅pS and the empirical limit that was observed was built into the design parameters of the LHC. However, Lyn Evans (who led construction of the LHC) showed that the limit was not fundamental (and indeed that beam–beam interactions are in principle self-stabilizing) and claimed that a well enough designed and constructed machine could beat the then supposed limit—as the LHC has done.

The second vital enabling factor was the emergence of hermetic detectors, first the Mark 1 detector at SPEAR at SLAC, and especially the UA1 and UA2 detectors at the Sp̅pS collider, which made it possible to measure missing energy/momentum and see rare events against a high background.

(a) The Lausanne workshop (March 1984) and the International Committee for Future Accelerators seminar (May 1984)

As the theoretical convenor of the Lausanne workshop [3], I had the honour of making the first presentation of the overall theoretical case for the LHC (much developed by John Ellis and collaborators at CERN) at the closing meeting at CERN. My talk began: ‘A large hadron collider has always seemed an obvious option to follow LEP and it is clearly becoming time to start R&D on suitable magnets. It is less clear that it is sensible to discuss the physics that might be studied with such a machine without more complete results from the SPS collider, let alone data from LEP, SLC and HERA. All we can do is identify the questions which seem most pressing now and ask how they could be addressed by experiments at an LHC’. The conclusion of the talk was that: ‘A theoretical consensus is emerging that new phenomena will be discovered at or below 1 TeV. There is no consensus about the nature of these phenomena but it is interesting that many of the ideas which have been suggested can be tested in experiments at an LHC. Although many, if not all, of these ideas will doubtless be discarded, disproved or established by the time an LHC is built, this demonstrates the potential virtues of such a machine’.

Very surprisingly, the case for the LHC has in fact remained essentially unchanged for 30 years. The main emphasis of my talk was on the possibility of discovering the Higgs boson, to which one and a half pages were devoted (starting with gluon–gluon fusion via a top quark loop, which was already recognized as probably being the most important production mechanism), followed by two pages on supersymmetry.

The basic layout of the LHC (2-in-1 superconducting magnets installed in the LEP tunnel, with the existing PS booster, PS and SPS forming part of the accelerating chain that supplies protons to the LHC via new transfer tunnels) has not changed since 1984. Most other features were, however, changed when a detailed design was worked out later (eight crossing points reduced to four, magnets and the cryo-line installed on the floor in place of LEP, rather than—as then envisaged—in a combined cryostat above LEP, etc.).

During the ICFA workshop [4], the LHC and the SSC were presented, together with plans for other facilities around the world, and there was a general discussion of future physics goals and the need for R&D on magnets and detectors. Two points remain strongly imprinted in my memory. First, the SSC was presented as a national US project, which greatly irritated some of the Japanese who were present, and not surprisingly coloured their reactions when a few years later, after the project had been approved and the parameters chosen, the USA asked Japan to contribute $2 billion.

The second was the discussion of luminosity. We theorists knew that in terms of event rates for constituent collisions at a particular energy, higher luminosity could compensate for the lower beam energy of the LHC compared with the SSC: indeed gluon–gluon collisions at the LHC at energies up to 1 TeV will be more copious that they would have been at the SSC (where the design luminosity of $10^{33}$ cm$^{-2}$ s$^{-1}$ could only have been increased by increasing the aperture, and hence the cost, of the magnets in order to accommodate a beam liner—as at the LHC—to absorb the synchrotron radiation at a higher temperature than that of the magnets). Our experimental colleagues, however, were not impressed by this argument. Carlo Rubbia ‘expressed doubts with regards to the usefulness of luminosities as high as $10^{33}$ cm$^{-2}$ s$^{-1}$’. Roy Schwitters (later the Director of the SSC) asserted that the ‘increase in cost for the detector when going from $10^{32}$ to $10^{33}$ had been estimated to be a factor 3 to 4’. And Giorgio Brianti (who led the LHC project until the end of 1993) stated that while it was ‘conceivable that the luminosity [of the LHC] could eventually approach or even exceed $10^{33}$’ there had been a ‘consensus at the Lausanne workshop that the number of events per bunch crossing should not exceed one’ (in 2013, the average number was about 3).
(b) The CERN long range planning group

In 1987, a long range planning group, set up by the CERN Council in 1985 and chaired by Carlo Rubbia, recommended that a proton–proton (rather than antiproton) collider with centre of mass energy 13–15 TeV, based on 8–10 T magnets, with a luminosity of $10^{33}$ cm$^{-2}$ s$^{-1}$, should be the first choice for CERN’s next major facility [5]. The group suggested that first operation could be possible in 1995, in parallel with the operation of LEP (this was not the first or the last of Rubbia’s claims that proved over-optimistic: during the ICFA workshop he had asserted that the LHC could be built for less than $400$ million). The group noted that a 2 TeV electron–positron collider would have comparable ‘reach’ to the LHC, and could enable detailed study of phenomena discovered at the LHC or SSC, but was not technically feasible.

The group further noted that the ‘Lesser energy of LHC [relative to SSC] can be partially ‘recovered’ with the help of a larger luminosity’, but ‘experiments at more than $10^{32}$ will require novel instrumentation’, and recommended intensified R&D on high field magnets.

(c) Magnet R&D and evolution of the design of the Large Hadron Collider

Magnet R&D for the LHC (by Brianti, Perin, Leroy, Rossi and others) had in fact already started in 1986. In 1988, a single bore 1 m magnet designed at CERN and built by Ansaldo reached 9 T at 1.8 K. In 1991–1993, CERN’s first 2-in-1 magnets (which were 1–1.3 m in length) also reached 9 T. The first long (10 m, still shorter than the final length of 14 m) 2-in-1 magnet did not operate until March 1994, after the proposal to build the LHC had been formally presented to the CERN Council. However, it worked well with excellent field quality (the SSC eschewed the 2-in-1 design, which would have lowered the cost, following bad experiences at Isabelle and fearing that a single 2-in-1 magnetic structure could compromise the field quality: given the width of the tunnel, 2-in-1 magnets were mandatory for the LHC).

Meanwhile, 2-in-1 quadrupole magnets had been developed by a CERN–CEA collaboration. These were combined with three 10 m dipoles to create a 1/8 sector ‘string’ which operated successfully in November 1994, shortly before the CERN Council approved the LHC.

In March 1992, it was still hoped that the LHC magnets could reach 10 T, which (with the then current design) would have led to proton collisions at $2 \times 7.7$ TeV, and a design luminosity of $1.6 \times 10^{34}$ cm$^{-2}$ s$^{-1}$. Parameters had also been worked out for LEP-LHC/ep collisions and for Pb–Pb collisions. By December 1993, the parameters were converging on those of the machine that was actually built—$2 \times 7$ TeV energy, 8.65 T magnetic field and $2.4 \times 10^{34}$ cm$^{-2}$ s$^{-1}$ design luminosity (the 13.5% reduction in field relative to the 1992 figure only resulted in a 9% reduction in energy because meanwhile the length of the dipole magnets, and hence the overall magnetic length, had been increased). In 1995, the idea of installing the LHC above LEP (which would have been extraordinarily difficult and expensive) was abandoned, and the Conceptual Design Report published in October 1995 essentially describes the machine that was built.

(d) Detector R&D and the evolution of the experimental programme

Intensified detector R&D at CERN was kick-started in 1986 when Antonio Zichichi obtained 40 million Swiss francs from the Italian government for the so-called LAA Project, which were mostly used to support a range of detector development (LAA actually stood for Lepton Asymmetry Analyser). CERN also funded detector R&D and set up a Detector R&D Committee (DRDC) in mid-1990. By March 1992, the DRDC had received 35 proposals and approved 24, which involved 800 people in 170 institutes in total.

Ideas about possible LHC experiments were discussed in a series of workshops (at La Thuile 1987, Barcelona 1989 and Aachen 1990) culminating in a workshop organized by Walter Hoogland (one of CERN’s then three Research Directors) at Evian in March 1992 [6], at which I gave the introductory theoretical talk. During the meeting, expressions of interest for four general purpose detectors were presented—two (ASCOT and EAGLE) with a relatively small central solenoid...
surrounded by a large outer toroidal magnet and two (CMS and another that would have reused the magnet of the LEP Detector L3) involving very large solenoids—together with expressions of interest for two heavy ion experiments, three CP violation/B physics experiments and two neutrino experiments (the proceedings [6] contain brief descriptions of expressions of interest related to low transverse momentum physics, measurement of the total cross section and physics with a jet target, which were not presented).

The newly formed Large Hadron Collider Committee (LHCC) asked for formal letters of intent to be submitted by 1 October 1992, by which time ASCOT and EAGLE had merged to form ATLAS.

(e) Engagement of the CERN Council

Carlo Rubbia, who became Director General in 1989, stimulated growing support for the LHC in the European particle physics community, although some were signing up for the SSC which had been approved in 1987. The response of members of the CERN Council, who were of course well aware of the SSC, ranged from cautious to positive. Sir William (Bill) Mitchell, who served as President of the CERN Council from 1990 to 1993 and was a strong supporter of the LHC, organized a special enlarged, open Council session to examine all aspects of the project in December 1991, at which as Chairman of the CERN Scientific Policy Committee I presented the scientific case for the LHC.

The Minutes record that in the discussion Carlo Rubbia stated that ‘the press had over-exaggerated the competitive aspect of the LHC and SSC. That was a very superficial view, whereas a closer look at the scope of the two machines revealed a lot of complementarity. There was room for a machine with the highest energy and for another that would go for the highest luminosity and which in addition would have other avenues (ep collisions, heavy-ion collisions) to explore once SSC had started operation. Moreover, the existence of both machines would allow a very useful cross-verification of experiments’. At the end of the meeting, Bill Mitchell presented conclusions on behalf of the Council which included the statement that ‘LHC is the right machine for the advance of the subject and the future of CERN’ and asked for more detailed information on the project before the end of 1993 ‘so that Council may move towards a decision on the LHC’.

(f) Towards the proposal to build the Large Hadron Collider

In retrospect, the approval of the LHC may seem to have been inevitable, but it did not seem so at the time. On Saturday 1 May 1993, Carlo Rubbia convened a meeting with me (as Director General Designate: I succeeded Carlo on 1 January 1994), Giorgio Brianti (then the LHC project leader) and Lyn Evans (who I had nominated to succeed Giorgio), to discuss Giorgio’s latest costing of the LHC. When he saw the figures, Carlo said that he thought the proposal would fail and handed me the responsibility for producing a complete proposal and long-term plan for CERN, for presentation to the CERN Council in December 1993, as requested by the Council in December 1991. Not only did the material cost look high, but responses to a survey of CERN’s group leaders, which asked how many people they would need to build the LHC, had suggested that a 20% increase in staff would be required, which was clearly unaffordable. Over the summer, Lyn worked tirelessly to simplify the design and reduce the cost, while I worked on the long-term plan and future manpower needs in close collaboration with Horst Wenninger.

The cancellation of the SSC in October 1993 (which is described below) moved approval of the LHC from the improbable to the possible, and a very positive external review by Robert Aymar (a fusion expert who became Director General of CERN in 2004) prepared the ground for the presentation in December. The review reported as follows:

— the design goals (energy and luminosity) are reasonable and realistic;
— there is no doubt that 8.65 T can be achieved with an adequate safety margin;
— the technical choices (2-in-1 magnets operating at 1.8 K) are the only appropriate ones;
— the cost estimates are accurate and there are enough potential savings to avoid the need for contingency; and
— the proposed schedule is OK.

In retrospect, these were rather remarkable claims given that R&D on the magnets and other systems was still underway, and in particular not a single long 2-in-1 dipole had been tested. I do not know if the Council remembered this when they asked Aymar to report on the origin of the cost overruns that emerged in 2001.

(g) The rise and fall of the Superconducting Super Collider

The SSC was approved in 1987 on the basis of an estimated cost of $4.4 billion. By May 1990, however, the estimate had risen to $7.9 billion and the US Congress voted to limit the federal contribution to $5 billion, leaving the difference to be made up by Texas (which had pledged $2 billion) and overseas contributions. Following further cost escalations, the House of Representatives voted to end the project in 1992, but the budget was restored by the Senate. It was however finally cancelled in October 1993, by which time the official budget estimate was $11 billion. Factors in the demise of the SSC included the large cost increase, schizophrenia over whether the project was national or international, the use of a green field site and the management structure that was adopted.

Immediately after the cancellation of the SSC, ICFA convened a summit conference to discuss the fall-out, at which the leaders of the world’s major particle physics laboratories were present (I represented CERN). The meeting took place at CERN in November 1993 in a tense atmosphere. It was even suggested that the imminent presentation of the LHC proposal should be deferred pending development of a world programme with two major projects (a hadron collider and a linear electron–positron collider), which should be presented simultaneously to the world’s governments—an idea which was quickly dropped when Bjorn Wiik (Director of DESY, which was preparing a proposal for a linear collider) remarked ‘If you want to take two large oil tankers through a narrow channel, you do it in series, not in parallel’.

In January 1994, ICFA met again in a calmer atmosphere and declared that ‘LHC now offers the only realistic opportunity to study multi-TeV hadron collisions...LHC will remain a unique facility for the foreseeable future and ICFA considers that it is now the correct next step for particle physics at the high-energy frontier’, which was helpful in seeking support for the LHC around the world. Meanwhile, HEPAP had set up a Panel, chaired by Sidney Drell from SLAC, to advise on the way forward for US particle physics following the demise of the SSC. As the one European on the Panel, Lorenzo Foa—who was due to become CERN’s Director of Research in July 1994—played a key role in shaping perceptions of CERN’s plans, and in the summer of 1994 the Drell Panel recommended that the USA should join other nations in building the LHC. However, in order to avoid a veto by embittered US Congressional supporters of the SSC, who had been told that the LHC could not do the job, the Panel recommended a maximum US contribution of $400 million (for the machine and detectors), and that this should be reduced if a proposed ‘bump’ in the budget did not materialize, which it did not. This was nothing like commensurate with the number of Americans (already 550) who had expressed interest in working at the LHC, but—as a recommendation from the particle physics community itself—proved difficult to evade, although eventually we negotiated a contribution of $531 million (including $81 million from the National Science Foundation).

(h) Presentation of the proposal to the CERN Council

The Executive Summary of the comprehensive set of documents on the LHC and CERN’s long-term plans which we presented to the Council in December 1993 declared that There are compelling arguments that...fundamental new physics will appear in the domain of energy that...will be opened up by the LHC. A high luminosity proton–proton collider is
currently the only realistic choice for exploring this energy domain, and LHC is now the only possibility for such a collider after the cancellation of the SSC. A high-energy electron–positron collider would be appropriate for detailed studies above any previously identified energy threshold for new physics, once the technology to build such a machine is available. It went on to state that ‘The LHC…will provide an unparalleled ‘reach’ in the search for fundamental particles and interactions between them, and is expected to lead to new, unique insights into the structure of matter and the nature of the Universe. Studies of proton–proton collisions at LHC will provide the opportunity to find the so-called Higgs boson, or bosons, and thus should answer the question why some particles are massive while others are not. These experiments should find ‘supersymmetric’ particles, if they exist, thereby revealing a deep connection between constituent particles and particles that mediate the forces between them’.


(a) Approval for construction in two stages

The CERN Council’s reaction to the December 1993 proposal was generally positive, but Germany and the UK were unwilling to approve the requested increase in the budget, and we were asked to look for cost reductions and further savings. A revised plan (which proposed delaying commissioning from 2002 to 2003 or later, depending on what contributions could be found outside Europe, staging the detectors, and further cuts in the rest of the programme) was presented in June 1994. Seventeen CERN Members voted to approve the project, but Germany and the UK demanded further cost savings and substantial contributions from France and Switzerland.

In autumn 1994, Germany and the UK declared, further, that they would only approve the project on the assumption of 2% annual inflation to be (under-)compensated by a 1% annual increase in the CERN budget. It was clear that the LHC as proposed could not be built with a budget declining 1% a year in real terms. As the only way ahead, we proposed building the LHC in two stages as a ‘missing magnet’ machine (an idea deployed by John Adams at one stage when seeking approval of the SPS). After Stage 1, in which the LHC would have operated for some years at two-thirds of the ultimate energy with one in three of the bending magnets left out, the missing magnets would have been installed, enabling operation at full energy in Stage 2.

We told the CERN Council that, although far from ideal (and in the long run more expensive than single stage construction), a two stage LHC would be acceptable provided the Council made a commitment that ‘Any contributions to the LHC from Non-Member States would be used to speed up and improve the project, not to allow reductions in the contributions of the Member States’, and we proposed a review of the construction timetable in 1997 to decide whether to proceed in a single stage.

The Council made the requested commitment to non-Member States and the LHC was approved for construction in two stages in December 1994, by which time France and Switzerland had agreed to make additional voluntary contributions (both agreed to index their contributions at 2% per year, France agreed to provide certain infrastructure close to CERN, and Switzerland agreed to pay for the excavation of the tunnel that houses the transfer line from the SPS to the LHC tunnel which lies in Swiss territory). Reassuringly, David Hunt, the UK Minister for Public Service and Science, wrote to me in late December 1994 that ‘We believed it was essential that the project was founded on a realistic, fair and sustainable basis. I believe that the planning and financial framework agreed unanimously by the CERN member states fulfils those necessary conditions which will ensure the LHC will be carried through to completion…Now it is down to you and

¹Some of the events described below, and above, are described in more detail in [7].
the CERN staff to make this project a great success’. This was forgotten, as was the Council’s commitment on the use of the contributions of non-Member States, when the UK supported Germany in successfully pressing for a large budget cut in 1996.

(b) Approval of single stage construction

In the years 1995–1996, we negotiated contributions to the LHC from Canada, India, Japan and Russia. Agreement was also reached with the USA, although following the election of a Republican majority in the US House of Representatives in October 1996, the new chair of the House Science Committee declared that he was unhappy with the details and it was only signed in late 1997, following further long negotiations and some changes in wording but none in substance. The CERN Council’s pledge to use the contributions of non-Member States to speed up and improve the project, and not to use them to reduce the Members’ contributions to a project which was going ahead anyway, was an important factor in the negotiations.

At this time, CERN’s immediate priority was to upgrade and fully exploit LEP. During 1995, we persuaded the CERN Council to fund a final stage in the upgrade, which was not easy given the very recent commitment to the LHC and the financial situation. In the following years, while fighting the attack on the CERN budget described below, we were preparing the ground to ask Council to allow operation of LEP to continue through the year 2000, which was finally agreed in June 1998.

In June 1996, I decided that we had obtained sufficient funding from non-Member States to make single stage construction of the LHC possible, and to bring forward the planned 1997 review of the construction timetable to December 1996. In August, however, there was a major set-back when Germany, where the economy was in bad shape following re-unification, asked for a cut of 8.5% in its contributions to CERN for two years and 9.3% thereafter, and similar cuts in its contributions to all other international scientific organizations. Germany was in fact already enjoying a temporary reduction in its contribution to CERN because of the difficulties caused by re-unification. However, before I could point this out, the UK—where the Director General of Research Councils, Sir John Cadogan, was trying to reduce expenditure on particle physics, for which he had little sympathy—seized the opportunity to demand a large reduction for all Member States.

Fighting this attack on the CERN budget had to be done quietly in order not to lose the contributions of the non-Member States, who appeared to have been misled. There was an extra difficulty in the UK where the particle physics community’s support for the CERN budget had been undermined by Cadogan who had told them that cutting the CERN budget was the only way to fund the UK’s contributions to the LHC detectors.

It was soon clear that, with the backing of both the UK and Germany, a large cut in the budget, which was already declining in real terms, was inevitable (in the end cuts of 7.5% in 1997, 8.5% for the next three years and 9.3% thereafter were agreed). We realized that consequently the only way to build the LHC at all, and not to renege on pledges to non-Member States, was to borrow money, an option which had always been vetoed in principle by Germany. In this case, however, during a visit to Bonn, Horst Wenninger and I persuaded the German minister to agree exceptionally to CERN taking a loan.

In December 1996, single stage construction of the LHC was approved by the CERN Council, accompanied by a large cut in the budget (and also a one-year reduction in CERN salaries), on the basis of the contributions offered by non-Member States (although at that time the agreement with the USA had not been signed) and that CERN could take a large loan. I presented a plan which, on paper, made this possible, but it rested on a large number of assumptions. I repeatedly stressed the risks and that Council would have to be ready to help and deal with the consequences should the risks materialize. Reassuringly, this seemed to be accepted, and the head of the German delegation stated that ‘a greater degree of risk would inevitably have to accompany the LHC’.
(c) Approval of ATLAS and CMS

Approval of the LHC detectors by CERN progressed in parallel with approval of the LHC itself by the Council. In July 1993, the LHCC evaluated three letters of intent to build general purpose detectors and selected ATLAS and CMS to proceed to write technical proposals, which were approved in 1996. Formal approval to move to construction followed in July 1997, within material expenditure cost ceilings of 475 million Swiss francs for each detector, and subject to oversight and monitoring by bodies and mechanisms that had been established (including Resource Review Boards for each detector on which the national agencies that were providing funding were represented). Meanwhile, ALICE and LHCb were following similar steps towards approval, which followed a few years later.

4. Coming of age

By the end of 1998, when I handed over to Luciano Maiani as Director General of CERN, the LHC was well past the point of no return—a formal agreement had by then been signed with the USA and around half of the contracts (by value) had been signed (overall they were in line with CERN’s costs estimates)—although the situation was clearly fragile, as I had warned the Council in December 1996. I will not elaborate or dwell on developments and difficulties (such as the large cost over-run that was announced 2001, and problems with the cryo-line) in the decade leading up to switch-on as I do not consider them part of the genesis of the LHC (and I only observed them from a distance).

It is however worth dwelling on the publicity that the LHC attracted in the days leading up to switch-on, and has enjoyed ever since, which was triggered by the attempt of an American to get an injunction to stop the project on the grounds that it might destroy the Earth or the whole universe. On 1 September 2008, the UK’s Sun newspaper reported this under the headline ‘End of the world due in nine days’, which was followed by a reasonable description of the LHC’s aims (surely a first for the Sun, which clearly did not treat the threat seriously). An accompanying item entitled ‘Don’t panic there’s time to try out every position in the Kama Sutra’ listed nine things to do before the end of the world: the sixth—‘Drive to Switzerland for a ringside seat of doomsday’—was no help to me as I already had an air ticket, but the ninth—‘Cancel the milk and papers’—was helpful.

On the afternoon of 10 September, the Evening Standard’s bill board read ‘WORLD SURVIVES ‘BIG BANG’; the LHC had entered popular culture. In fact, although the start-up had gone immaculately, and beams had circulated in both directions that morning, there had been no collisions, and the world would have survived even if the doomsters had been right. Alas, still before any collisions, an electrical fault nine days later had catastrophic knock-on effects and repairs and improvements took until November 2009, when the LHC re-started. But then it worked, wonderfully well, albeit not yet at full energy.

5. Concluding remarks

The LHC is working way beyond expectations (well above the design luminosity for the energy reached up to now), the detectors are also working way beyond expectations (at luminosities which were deemed unusable when the LHC was first discussed seriously in 1984), and the data analysis is breaking new ground (after approval in 1994, I joked that it was lucky that the LHC could not be built overnight as data analysis would have to rely on a decade of Moore’s law plus something new—which turned out to be the grid). All hail to those who built the machine and the detectors and have so rapidly analysed the data.

Approval of the LHC depended on

— a robust scientific case (exploration of a large new domain, with good reasons to expect discoveries);
— uniqueness;
— essentially unanimous support of the world’s particle physics community;
— the technical success of CERN (throughout the approval process LEP was breaking records); and
— being able to build the LHC without a budget increase (a real terms decrease was actually imposed).

Approval of future major particle physics projects will require

— a robust scientific case;
— major discoveries at the LHC (the Higgs qualifies, but something less expected would be a tremendous boon);
— unanimous support of world’s particle physics community;
— continued technical success of CERN; and
— a ‘reasonable’ budget envelope.

As a result of the publicity that the LHC has attracted, public support will help but it will not be sufficient unless the other conditions are satisfied. While it is not too early to start thinking about future projects, in my opinion it is premature to argue about priorities and to start to make the case pending the accumulation and analysis of a lot of data at the highest possible LHC energy. The LHC has performed beyond expectations: let’s hope that it will also contribute to our understanding beyond expectations.

References

3. Large Hadron Collider in the LEP tunnel, ECFA 84/85, CERN 84-10.