The Large Hadron Collider: lessons learned and summary

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The Large Hadron Collider (LHC) machine and detectors are now working superbly. There are good reasons to hope and expect that the new domain that the LHC is already exploring, operating at 7 TeV with a luminosity of $10^{33}$ cm$^{-2}$ s$^{-1}$, or the much bigger domain that will be opened up as the luminosity increases to over $10^{34}$ and the energy to 14 TeV, will provide clues that will usher in a new era in particle physics. The arguments that new phenomena will be found in the energy range that will be explored by the LHC have become stronger since they were first seriously analysed in 1984, although their essence has changed little. I will review the evolution of these arguments in a historical context, the development of the LHC project since 1984, and the outlook in the light of reports on the performance of the machine and detectors presented at this meeting.

Keywords: Large Hadron Collider; particle physics; CERN

1. Introduction

In this paper, I shall focus on the development of particle physics since the Large Hadron Collider (LHC) was first discussed, in a longer historical connect, and the outlook now that the LHC is starting to produce data. There are technical and political lessons to be learned from the history of the LHC, but I want to focus on the science, although I will say something about the politics. It is easy to summarize where we stand: while theorists have been stuck in a groove, the accelerator physicists, the engineers and the experimenters have built a superb collider and excellent detectors, which are now performing beyond expectations. The LHC has not yet led to any major discoveries, although it is beginning to put useful constraints on some models, but this is not unexpected, as significant data collection is only just beginning. The prospects for the next few years are, however, very exciting.
2. Some history

The first written reference to what became the LHC appears in 1977 in the note books of John Adams, then technical Director General of CERN, soon after the first discussions of the Large Electron–Positron collider (LEP) in 1975–1976 and four years before LEP was approved.\(^1\) John, who did not like electron machines, wrote that, if LEP were built then, the tunnel should be made large enough to accommodate superconducting magnets to enable protons to be accelerated to above 3 TeV. This idea was becoming well known in 1978, when I recall stressing that not only should the tunnel be wide enough, but its length should be as long as possible in order to provide the maximum potential for a future hadron machine, when presenting the theoretical summary of the major 1978 LEP Summer Study in the CERN auditorium and to the CERN Scientific Policy Committee.

The first serious discussion of the LHC took place in a workshop in Lausanne in March 1984. This workshop was a response to developments in the USA, where the particle physics community was rallying in support of the proposed Superconducting Super Collider (SSC) following the cancellation of the ISABELLE project in 1983. The opening paragraph of my theoretical summary talk [3, p. 27] contains the statement:

A large hadron collider has always seemed an obvious option to follow LEP and it is clearly becoming time to start R and D on suitable magnets. It is less clear that it is sensible to discuss the physics which 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, whose centre of mass energy we take to be 10 to 20 TeV. This crystal gazing is unusually hazardous following the recent tantalising hints of new discoveries from UA1 and UA2, which remind us that it runs the risk of rapid redundancy.

(I have retained the reference to hints that turned out to be spurious as a reminder that we can expect some false dawns as analysis of the LHC data proceeds.)

What has happened in the intervening 27 years?

3. Rip Van Winkle

To put developments since the Lausanne workshop in context, consider what a Rip Van Winkle, versed in physics, who fell asleep for successive periods of 27 years during the last century, would have understood if he had woken up in a talk on fundamental micro-physics. On waking up in 1930, after falling asleep in 1903, he would have understood nothing. Waking again in 1957, he would have understood almost nothing, and the same would have been true when he then re-awoke in 1984 (I will later recall briefly what was known in 1903, 1930, 1957 and 1984).

On waking in 2011, after sleeping since 1984, however, he would have understood almost everything, although if shown the LHC detectors he would have been amazed by their sophistication and performance, and he would have been baffled by discussion of data analysis, as he would not have heard of the

\(^1\)For the political history of the LHC, see Llewellyn Smith [1], Evans [2] and §4 of this paper.
World Wide Web, let alone the Grid (in retrospect, it is amazing that a 1983 review of future computing needs at CERN did not mention PCs).

Near the beginning of my talk at the Lausanne workshop, I presented the following ‘fairly standard list of deficiencies of the Standard Model’:

— the origin of mass;
— the origin of flavour;
— the origin of CP violation; and
— the connection between electroweak, strong and gravitational forces.

This is very similar to lists presented in this meeting.

My talk next focused on the question of the origin of mass, which for the W (and Z) boson is equivalent to asking: what is the origin of the third (longitudinal) polarization states (WL and ZL), which (assuming they are gauge bosons) are absent at the Lagrangian level, seemingly forcing them to be massless, like the photon? In 1984, as today, we knew two options, both of which very strongly suggest the existence of new phenomena below 1 TeV:

Either… WL and ZL are components of a fundamental (Higgs) field, which must have at least one component that shows up as a Higgs particle, with a mass almost certainly below 1 TeV. In this case, however, it is very hard to understand why the W and Z are so light on the scale of quantum gravity or Grand Unification unless Nature is supersymmetric, a possibility that I described as ‘very appealing’ because it ‘unifies fermions and bosons and is the maximal symmetry allowed by rather general theorems’, or (perhaps) if there are strong Higgs field self-interactions at the TeV scale that would lead to strong WL/ZL–WL/ZL interactions.

Or… WL and ZL are composites of new particles bound tighter by a very strong ‘technicolour’ force, in which case other composites should be found below 1 TeV. Then, as today, such models seemed ‘unacceptably complex and run into phenomenological difficulties’. However, the underlying picture is so attractive that its consequences should be explored and theorists should continue to look for new ways of generating fermion masses.

Like introductions to the LHC today, my talk went on to present parton luminosity curves and the phenomenology of Higgs bosons, technicolour, supersymmetry, heavy quarks, heavy Ws and Zs, WW scattering and models in which quarks are composite.

4. From 1984 to approval

The LHC was first presented in detail to the CERN Council at an open meeting in December 1991. As Chairman of the CERN Scientific Policy Committee, I was asked to present the scientific case. I presented the same arguments as in Lausanne, although my talk included an ‘oral interlude on particle physics and cosmology’ that discussed dark matter and quark–gluon plasma. We knew about dark matter in 1984, although it was not stressed as part of the case for the LHC, but it seems that not much—if any—thought was then given to accelerating heavy ions in the LHC in order to create quark–gluon plasma. I asserted that the higher design luminosity (by then 10^{34}) made the exploratory range of the LHC similar to that of the SSC for many purposes, although it made the experiments
much harder (which was an understatement as—at that time—it was not really
clear whether it would be possible to do experiments at 10^{34}), making the LHC
the most cost-effective route to 1 TeV, with heavy-ion physics and ep collisions
as a bonus. (At that time, it was proposed to install the LHC above LEP, which
would have been very difficult, in order to provide this option, but this possibility
was dropped in 1995 when it was decided to dismantle LEP.)

The basic two-in-one layout of the machine has not altered since the
Lausanne meeting, although at that time there was of course no detailed design,
and subsequently there have been many changes, such as the replacement of
superconducting by normal magnets in the transfer lines. The original design
luminosity of 10^{33} (which was then judged ‘challenging but useable’) had become
10^{34} by the end of the 1980s, when there was growing optimism that it would be
useable thanks to CERN’s detector R&D programme, which was driven by Walter
Hoogland as Research Director. The outlines of the experimental programme were
becoming clear by the time of the 1992 Evian workshop, where the concept of
the CMS (Compact Muon Solenoid) detector was presented together with that
of the proposed EAGLE and ASCOT detectors, which merged to form ATLAS,
and first ideas for B and heavy-ion experiments were put forward. In my talk at
the workshop [4], I stressed that there was a ‘small probability of any particular
non-standard scenario, but models stretch detector requirements’ and that the
detectors needed a ‘good combination of e, \mu, \tau, \gamma, W, Z, jets, missing E_T, b
tagging’, which indeed they display to an astonishing degree, as we have seen in
the experimental talks at this meeting. We continue to hope that these capabilities
will be adequate for discovering any ‘unknown unknowns’. At that time, it was
assumed that Moore’s law would in time produce sufficient computing power to
handle and analyse the data, which—with help from Grid computing, which was
not then foreseen—is proving to be the case.

5. Factors in the approval of the Large Hadron Collider and future projects

Before discussing progress in particle physics since 1984, I shall pause to consider
the factors that underwrote approval of the LHC and will be important when
future large particle physics projects are proposed. First, I want to emphasize
that, as discussed by Evans [2], the LHC was approved in 1994, initially for
construction in two stages as a ‘missing magnet machine’, with a review foreseen
in 1997 (which in the event happened in 1996) to decide whether, in the light
of any additional funding obtained from outside the CERN member states,
to move to single-stage construction. I stress this because a myth (which was
repeated at this meeting) seems to be spreading that the LHC was approved
in 1996.

In retrospect, the approval of the LHC may seem to have been inevitable,
but it did not seem so at the time. On a Saturday in May 1993, Carlo
Rubbia (the Director General of CERN) arranged a meeting with me (as DG
Designate), Giorgio Brianti (then the LHC project leader) and Lyn Evans (whom
I had nominated to take over from Giorgio, who was due to retire, at the
beginning of 1994, when my appointment as DG began), 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 over to me the responsibility for producing

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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. Over the summer, Lyn worked tirelessly to simplify the design and reduce the cost, while I worked on the long-term plan in close collaboration with Horst Wenninger.

I will not give another account of the struggle to get the LHC approved (see [1] and [2]). The major factors that underwrote the eventual success were:

— a robust scientific case;
— uniqueness;
— unanimous support of the particle physics community; and
— the technical success of CERN.

The last three factors merit elaboration.

**Uniqueness.** I do not think that the LHC would have been approved if the SSC had not been cancelled (the UK and, I believe, Germany would have resisted strongly). The case certainly became easier to make after the SSC was definitively cancelled\(^2\) in October 1993. During 1994, a US Department of Energy panel led by Sidney Drell (on which Lorenzo Foa, who became Research Director of CERN in July 1994, played an important role) recommended that the USA should join the project.\(^3\) With Japan, Russia and others also signed up, the argument that the major players in particle physics should collectively continue to push back the frontiers of micro-physics by building the LHC seemed (to me at least) compelling.

**Unanimous support of the particle physics community.** This was not at first a given. An emergency meeting of the International Committee for Future Accelerators (ICFA, a body that comprised the directors of the world’s major particle physics laboratories), at which I represented CERN, was held at CERN in November 1993 to consider the future outlook following the cancellation of the SSC. Some participants argued that the LHC proposal should be put on hold, pending the development of a proposal to build a linear collider (in the USA or Japan), and that the two proposals should then be put together to the world’s governments. This idea was laid to rest by Bjorn Wiik, the Director of DESY (a laboratory whose support for the LHC was vital inside Europe), saying ‘if you want to take two large tankers through a narrow channel, you should do it in series, not in parallel’. A resolution adopted by ICFA in

\(^2\)The writing had been on the wall for some time, during which I thought that the LHC project should be developed as a fall-back in case the SSC was cancelled.

\(^3\)With an eye on what would be acceptable to members of the US Congress (especially those from Texas, who weresmarting from the cancellation of the SSC), who had been told that the SSC would be far superior to the LHC, the panel recommended that the USA should contribute $400 million to the LHC for the machine plus detectors, with the caveat that, if a proposed ‘bump’ in the US high-energy physics budget was not approved, the contribution should be less. Although the bump did not materialize, the Department of Energy and the National Science Foundation together eventually agreed to contribute $531 million, after prolonged negotiations and lobbying by major US universities where scientists had signed up to the LHC experiments (which CERN, planning for success, allowed long before funding was secured). This was not commensurate with the size of US involvement, which subsequently proved a source of tension, but given that the representatives of the US particle physics community had recommended a cap of $400 million, a contribution of $531 million was better than might have been hoped.
January 1994, which stated that the LHC was ‘now the correct next step for particle physics at the high-energy frontier’, and hoping for quick approval, was extremely helpful. Much-needed support from the UK community wavered slightly in 1996, making it harder to try to stave off cuts in the CERN budget requested by Germany with the strong support of the UK, when the Director General of Research Councils told the British particle physics community that without the cuts there would be no funding to support UK participation in the LHC experiments.

Technical success of CERN. It was extremely helpful that discussions of LHC funding by the CERN Council in 1994 and 1996 almost always followed the announcement of new record performances at LEP.

When future projects are put forward for approval, I believe that a longer list of factors will come into play, namely:

— robust scientific case;
— major discoveries at the LHC;
— public support;
— continued technical success;
— unanimous support of the now global particle physics community; and
— ‘reasonable’ budget envelope.

Again, three factors deserve elaboration:

Major discoveries at the LHC. The absence of new discoveries at the LHC would be an important clue that could stimulate new theories, but it is almost impossible to imagine obtaining funding for a new facility to test such theories on the basis of a dog that did not bark in the night.4

Public support. The start-up of the LHC in 2008 attracted unprecedented public attention, greatly stimulated by claims that it could produce black holes capable of destroying the Universe (the British papers did not take this seriously but gave it a lot of publicity, e.g. in the run-up, the Sun had a headline ‘Nine Days to Save the World’ and the Evening Standard billboard on 10 September 2008 read ‘World Survives Big Bang’). The LHC has continued to attract a lot of media attention and I think that public opinion will play a much bigger role in the future than in the past in decisions on funding new facilities.

Continued technical success. Surprisingly, there was no backlash from the Press (in the UK at least) when the LHC broke down shortly after the start-up—reports typically said that such events were normal, and to be expected, in a totally new very hi-tech project. There was, however, a backlash in the US Congress, which, with memories still alive of the money that was written off when the SSC was cancelled, held back funding that was being requested for upgrades of LHC detectors. Another serious breakdown could seriously jeopardize public and political support for future projects.

4A. Conan Doyle, Silver Blaze. Inspector Gregory: ‘Is there any other point to which you would like to draw attention?’ Sherlock Holmes: ‘The curious incident of the dog in the night-time.’ Gregory: ‘The dog did nothing in the night-time.’ Holmes: ‘That was the curious incident.’
6. Scientific progress from 1984 to 2011

Since 1984, the mystery of dark matter has been deepened by the discovery of dark energy, but otherwise not much has happened, except

— the demonstration that the Standard Model works at the one-loop level, and that, provided Nature is supersymmetric, the electroweak and strong coupling constants converge at high energy, as expected in Grand Unified Theories; I discuss the significance of this further in the next section;
— the very important discovery at LEP that there are only three light neutrinos;
— the expected discovery of the top quark, and the not unexpected (but extremely important) discovery of neutrino masses; and
— the discovery of a few small and not necessarily significant deviations from the Standard Model, for example in B decay [5] and \((g - 2)_\mu [6]\).

Do we have the clues, concepts and tools we need to make further progress?

7. Do we have the clues we need?

Table 1 shows some of the clues that were available at the beginning of the 27-year periods considered above and the tools that were then in use, and also notes some key discoveries that occurred in the subsequent 27 years. It is clear that the progress that was made between 1903 and 1930, between 1930 and 1957 and between 1957 and 1984 would not have been possible without new information from experiments in new energy domains.

Although retrospectively things may look different, it appears today that we need more clues in order to make progress. We hope and expect that they will be found in the new domain that the LHC is already exploring, operating at 7 TeV with a luminosity of \(10^{33} \text{ cm}^{-2} \text{ s}^{-1}\), or the much bigger domain that will be opened up as the luminosity increases to over \(10^{34}\) and the energy to 14 TeV.

8. Do we have the concepts we need?

Clearly, we need new ideas, which we hope the LHC will inspire. It is less obvious that we need new concepts. As Abdus Salam liked to say ‘Nature is economical in concepts, but extravagant in their realisation’.

To counterbalance the emphasis I have given to the lack of progress ‘beyond the Standard Model’ in the last 27 years, it is worth stressing the enormous conceptual progress that is embodied in the Standard Model, and the importance of the LEP experiments in showing that the Standard Model is a genuine field theory. This progress is summarized in table 2, on which a few comments are in order:

— The importance of the demonstration that quantum field theory is the correct language of particle physics may only be evident to those who remember the time in the late 1950s and 1960s when there was a
Table 1. Evolution of fundamental physics over successive 27-year periods.

<table>
<thead>
<tr>
<th>clues</th>
<th>ideas</th>
<th>tools in use</th>
</tr>
</thead>
<tbody>
<tr>
<td>1903</td>
<td>atomic spectra ?</td>
<td>cathode rays (keV)</td>
</tr>
<tr>
<td></td>
<td>Michelson–Morley Lorentz, …</td>
<td></td>
</tr>
<tr>
<td></td>
<td>radioactivity ?</td>
<td></td>
</tr>
<tr>
<td></td>
<td>photoelectric effect ?</td>
<td></td>
</tr>
<tr>
<td>no Rutherford scattering, Davisson–Germer, …</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1930</td>
<td>deviations from R scattering nucleus has structure</td>
<td>alphas (MeV)</td>
</tr>
<tr>
<td></td>
<td>A/Z neutron</td>
<td></td>
</tr>
<tr>
<td></td>
<td>continuous β spectrum E not conserved</td>
<td></td>
</tr>
<tr>
<td>quantum field theory formulated, but no knowledge of ν, μ, π, K, …</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1957</td>
<td>strange particles higher symmetries</td>
<td>cosmic rays</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Bevatron (GeV)</td>
</tr>
<tr>
<td>parity violation ?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>no strange resonances, ρ, ω, K∗, …, deep inelastic scattering, ν experiments, …</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1984</td>
<td>number of forces GUTs, strings</td>
<td>SppS (630 GeV, 10³⁰)</td>
</tr>
<tr>
<td>origin of mass Higgs</td>
<td></td>
<td></td>
</tr>
<tr>
<td>number of generations ?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>quark and lepton masses ?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>stability of Higgs SUSY</td>
<td></td>
<td></td>
</tr>
<tr>
<td>no top, ν masses, …</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2011</td>
<td>neutrino masses ?</td>
<td>LHC (7 TeV, 10³²)</td>
</tr>
<tr>
<td>dark energy ?</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(g − 2)μ ?</td>
<td>just starting</td>
<td></td>
</tr>
<tr>
<td>? extra dimensions?</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

widespread belief that this was not the case, and Geoffrey Chew famously declared that ‘Like an old soldier, field theory will not die, but simply fade away’.

— Gauge symmetries were originally introduced for speculative aesthetic reasons, but, following the discovery of asymptotic freedom (‘anti-screening’), it now seems that they may be the only consistent quantum field theories.

— The demonstration that Nature hides both chiral and gauge symmetry is exciting because hidden symmetry opens the possibility of simple theories generating very complex phenomena, including perhaps the extravagance of the Standard Model.

— Supersymmetry is often sold on the basis that it potentially solves the hierarchy problem/stabilizes the W and Z masses, but it seems to me that more powerful (although admittedly purely aesthetic) arguments are the beauty of the idea of unifying bosons and fermions (against which must be set the extravagance of having to invoke the existence of so many new particles), and the fact that it is the last of the four possible symmetries of Lagrangian field theory (the others being Lorentz invariance, internal symmetries and gauge symmetries), and (see the next point) provides a natural framework for introducing gravity.
Opinion piece. Lessons learned

Table 2. Concepts embodied in the Standard Model and some extensions thereof.

<table>
<thead>
<tr>
<th>Concept</th>
<th>Details</th>
</tr>
</thead>
<tbody>
<tr>
<td>quantum field theory</td>
<td>forces $\sim$ exchange of particles: particles + interactions $\leftrightarrow$ force (not a separate concept) [particles ('matter' and 'force carriers') $=$ fluctuations of fields]</td>
</tr>
<tr>
<td>✓ experiment</td>
<td></td>
</tr>
<tr>
<td>local (‘gauge’) symmetry</td>
<td>conventions to be chosen locally $\Rightarrow$ existence of force-carrying particles: form of interactions + properties fixed $\approx$ observations, except: gauge theory $\rightarrow$ mass $= 0$—only true for photon and graviton</td>
</tr>
<tr>
<td>✓ experiment</td>
<td></td>
</tr>
<tr>
<td>hidden symmetry</td>
<td>allows mass $\neq 0$ and unification of electromagnetic and weak forces: requires additional ‘Higgs’ particle(s)</td>
</tr>
<tr>
<td>✓ presumably</td>
<td></td>
</tr>
<tr>
<td>anti-screening</td>
<td>strong interactions $\rightarrow$ weaker at short distances; may converge $\rightarrow$ electroweak force at ‘Grand Unified’ scale</td>
</tr>
<tr>
<td>✓ experiment</td>
<td></td>
</tr>
<tr>
<td>supersymmetry?</td>
<td>connects matter and force-carrying particles: helps stabilize theory</td>
</tr>
<tr>
<td>local supersymmetry?</td>
<td>requires existence of gravity!</td>
</tr>
</tbody>
</table>

— The idea that (like the symmetries that underwrite the electroweak and strong forces) supersymmetry should be a local symmetry requires the existence of gravity, which makes this idea worthy of very serious consideration.

The concepts in table 2 are elegant, although nobody has been able to use them to construct a model that fits experiment without introducing a large number of seemingly arbitrary Higgs field couplings, and it is thought that they cannot provide a satisfactory quantum theory of gravity. Only time and the stimulus of new experiments will tell whether we need new concepts as well as additional clues.

9. The Large Hadron Collider: today and tomorrow

As described in the meeting, the LHC machine and detectors are now working superbly. The prospects for major progress are good and the stage is set for exciting years ahead. As I concluded in Lausanne in 1984 [3, pp. 46–47]:

The problems of the 1960s—the nature of hadrons, the nature of the strong force, the nature of the weak force—have been solved. We now confront deeper problems—the origin of mass, the choice of fundamental building blocks (the problem of flavour), the question of further unification of forces including gravity, the origin of charge and gauge symmetry. It is only to be expected that many of the first attempts to grapple with these problems will be misguided. As ever, we must rely on experiment to reveal the truth.

I hope that the long wait for experimental clues will soon be over.
The author acknowledges all those at CERN and around the world who have worked so hard to make the LHC a success. Special thanks go to my colleagues in the CERN Directorate during the five years (1994–1998) when we put forward the proposal to build the LHC and construction began.

References