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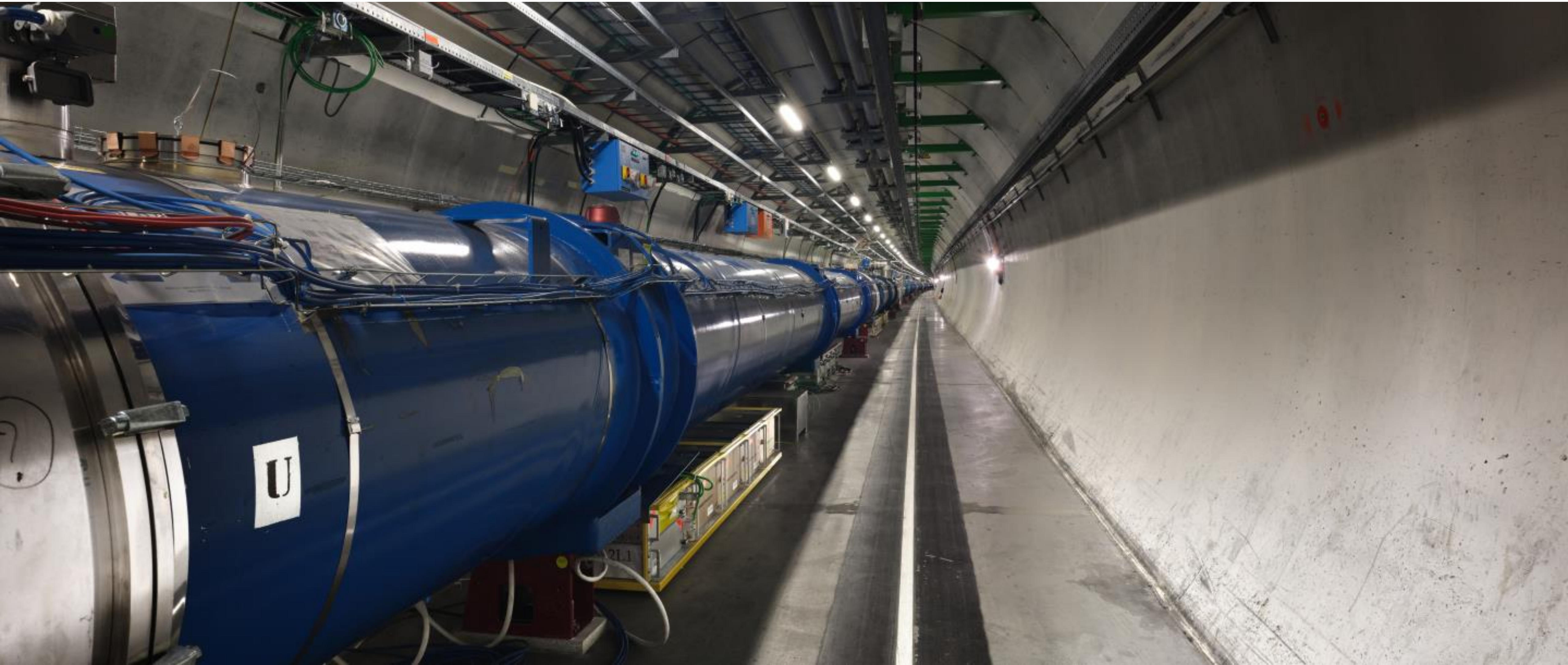
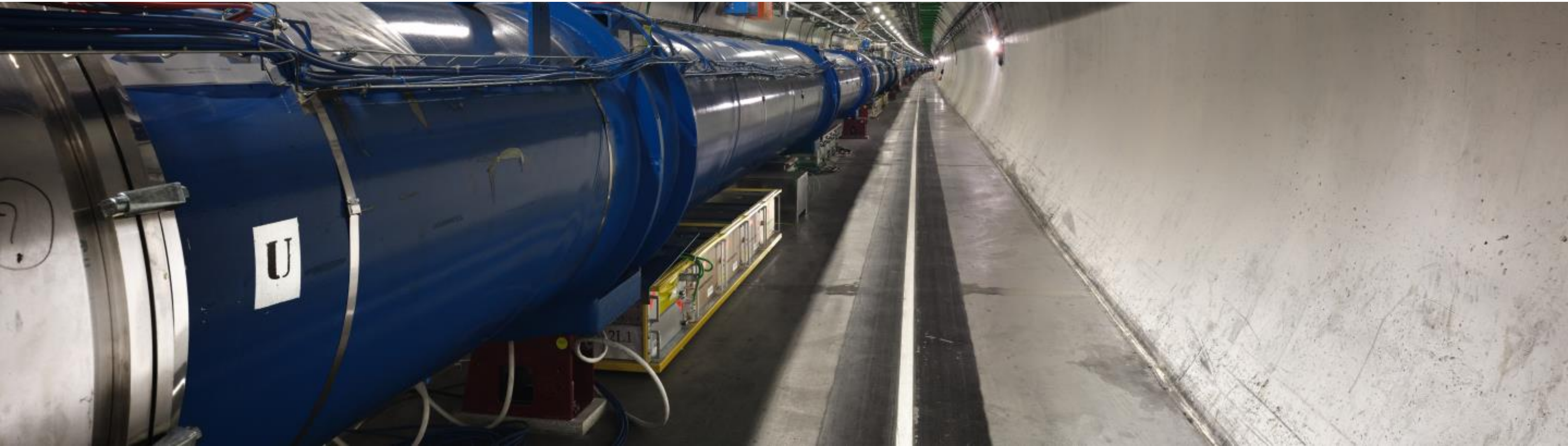
Nothing to see



Nothing to see

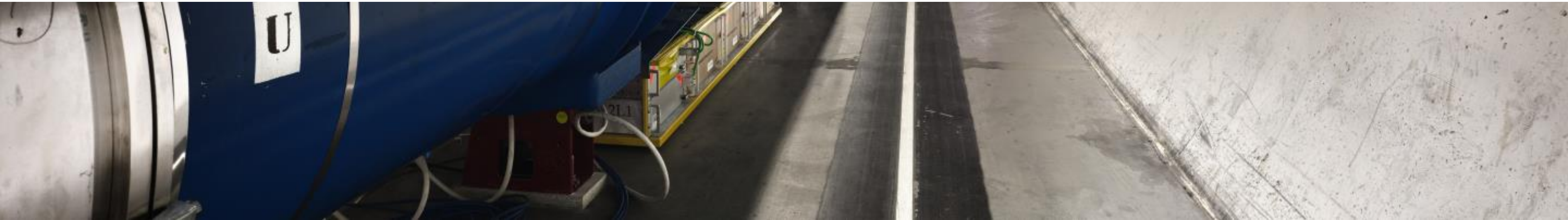
No no-lose theorem



Nothing to see

No no-lose theorem

No concerns about black holes



Nothing to see

No no-lose theorem

No concerns about black holes

A very different socio-economic context

Challenges and opportunities

Future colliders generally a step up in scale, and thus cost & environmental impact

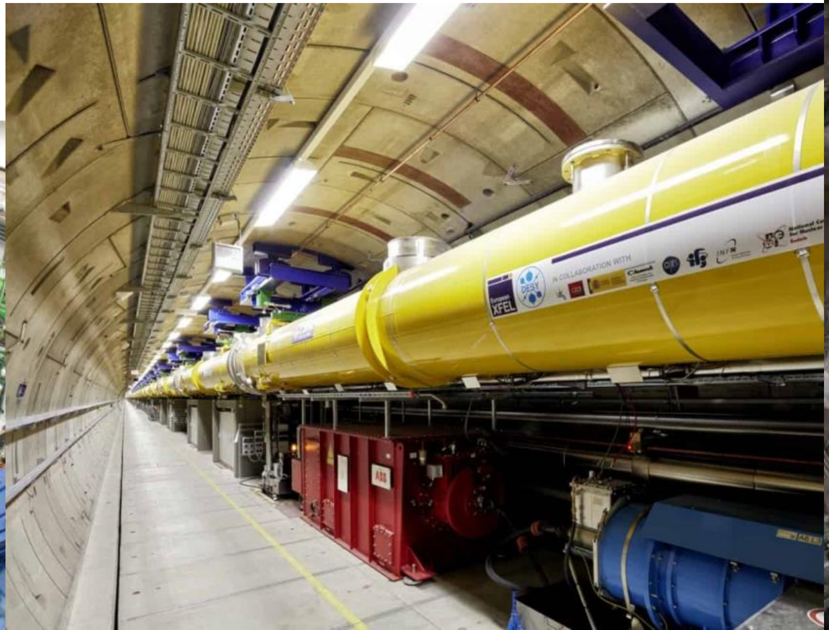
- Large colliders are **investments, not costs**, with structural socioeconomic benefits that give countries and regions a competitive edge.

- Studying nature at the smallest scales is part of a programme of “blue-skies” exploration which, during the past century, has **delivered the modern world**.

- Particle physicists are developing **cancer treatments** that save thousands of lives.

- Accelerator science drives advanced light sources that allow thousands of users from a broad range of disciplines to address **societal challenges**.

- And advanced superconducting devices



Societal impact:
much more than widgets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OPINION VIEWPOINT

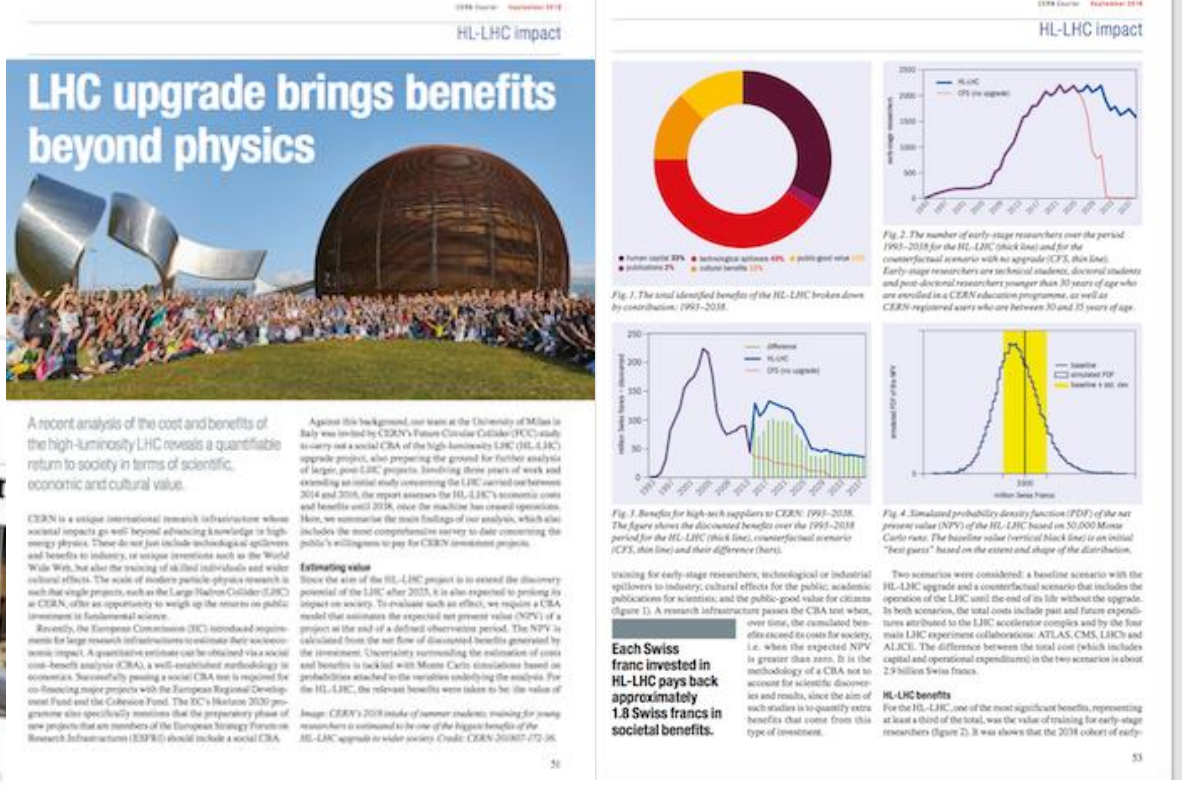
A price worth paying

Large research infrastructures are essential drivers of economic progress, and particle physicists have a duty to make this message loud and clear, argues Rolf Heuer.


OPINION VIEWPOINT

Engines of knowledge and innovation

If we don't dare spend money on projects that bring us to the future then we, as Europe, lose a competitive advantage, says Anna Panagopoulou.


HL-LHC impact

LHC upgrade brings benefits beyond physics

A recent analysis of the cost and benefits of the high-luminosity LHC reveals a quantifiable return to society in terms of scientific, economic and cultural value.

Each Swiss franc invested in HL-LHC pays back approximately 1.8 Swiss francs in societal benefits.

Schweizerische Eidgenossenschaft
Confédération suisse
Confederazione Svizzera
Confederaziun svizra

Start

Apart from the considerable and unique contribution that it makes to science and innovation, CERN's presence on Swiss soil also brings tangible economic benefits, particularly to the Canton of Geneva. CERN has played a key role in several technological breakthroughs that are now considered essential. This includes the World Wide Web and proton beam therapy (Hadron therapy) for cancer patients. In addition, CERN trains a large number of engineers and scientists, which helps to meet university and industry demand for highly skilled labour.

“CERN accelerators have drawn a similar power for a period of 40 years despite their vastly increased scientific output: from 1TWh for LEP2 to 1.2 TWh for the LHC and possibly 1.4 TWh at the HL-LHC.”

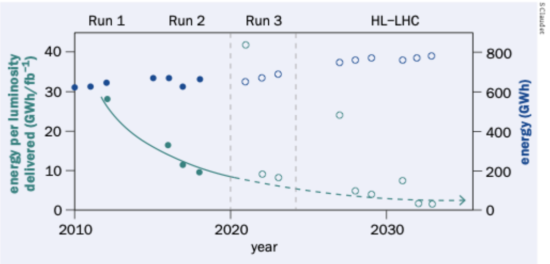
Large initial one-off costs of colliders can also be misleading: FCC would provide a rich physics programme for >50 yr (c.f. JWST, a $10B mission for 5-10 yr)

OPINION VIEWPOINT

Less, better, recover

For the LHC and future facilities, it is vital that each MWh of energy consumed brings demonstrable value to CERN’s scientific output, says Serge Claudet.

Serge Claudet *is chair of the CERN energy management panel.*



Greener physics *Energy consumed (blue) and per luminosity delivered (green) by previous (solid circles) and future (open circles) LHC runs.*

The famous “Livingston diagram”, first presented by cyclotron co-inventor Milton Stanley Livingston in 1954, depicts the rise in energy of particle accelerators as a function of time. To assess current and future facilities, however, we need complementary metrics suited to the 21st century. As the 2020 update of the European strategy for particle physics demonstrated, such metrics exist: instead of weighing up colliders solely on the basis of collision energy, they consider the capital cost or energy consumption with respect to the luminosity produced.

Applying these metrics to the LHC shows that the energy used during the upcoming Run 3 will be around three times lower than it was during Run 1 for similar luminosity performance (see “Greener physics” figure). The High-Luminosity LHC (HL-LHC) will operate with even greater efficiency. In fact, CERN accelerators have drawn a similar power for a period of 40 years despite their vastly increased scientific output: from 1 TWh for LEP2 to 1.2 TWh for the LHC and possibly 1.4 TWh at the HL-LHC.

The GWh/fb^{-1} metric has now been adopted by CERN as a key performance indictor (KPI) for the LHC, as set out in CERN’s second environmental report published last year. It has also been used to weigh up the performance of various Higgs factories. In 2020, for example, studies showed that an electron–positron Future Circular Collider is the most energy efficient of all proposed Higgs factories in the energy range of interest (*Nat. Phys.* **16** 402). But this KPI is only part of a larger energy-management effort in which the whole community has an increasingly important role to play.

There will be no future large-scale science projects without major energy-efficiency and recovery objectives

In 2011, with the aim to share best practices amongst scientific facilities, CERN was at the origin of the Energy for Sustainable Science at Research Infrastructures workshop series. A few years later, prompted by the need for CERN to move from protected-tariff to market-based electricity contracts, the CERN energy management panel was created to establish solid forecasts and robust monitoring tools. Each year since 2017, we send virtual “electricity bills” to all group leaders, department heads and directors, which has contributed to a change of culture in the way CERN views energy management.

Best practice

Along with the market-based energy contract, energy suppliers have a duty by law (with tax-incentive mechanisms) to help their clients consume less. A review of energy consumption and upgrades conducted between CERN and its electricity supplier EDF in 2017 highlighted best practices for operation and refurbishment, leading to the launch of the LHC-P8 (LHCb) heat-recovery project for the new city area of Ferney-Voltaire. Similar actions were proposed for LHC-P1 (ATLAS) to boost the heating plant at CERN’s Meyrin site, and heat recovery has been considered as a design and adjudication parameter for the new Prevessin Computer Centre. Besides an attractive 5–10 year payback time, such programmes make an important contribution to reducing CERN’s carbon footprint.

Energy efficiency and savings are an increasingly important element in each CERN accelerator infrastructure. Completed during Long Shutdown 2, the East Area renovation project led to an extraordinary 90% reduction in energy consumption, while the LHC Injectors Upgrade project also offered an opportunity to improve the injectors’ environmental credentials. Energy economy was also the primary motivation for CERN to adopt new regenerative power converters for its transfer lines (*CERN Courier* January/February 2022 p39). These efforts build on energy savings of up to 100 GWh/y since 2010, for example by introducing free cooling and air-flow optimisation in the CERN Computer Centre, and operating the SPS and the LHC cryogenics with the minimum of necessary machines. CERN buildings are also aligning with energy-efficiency standards, with the renovation of up to two buildings per year planned over the next 10 years.

This year, a dedicated team at CERN is being put together concerning alignment with the ISO50001 energy-management standard, which could bring significant subsidies. A preliminary evaluation was conducted in November 2021, demonstrating that 54% of ISO expectations is already in place and a further 15% is easily within reach.

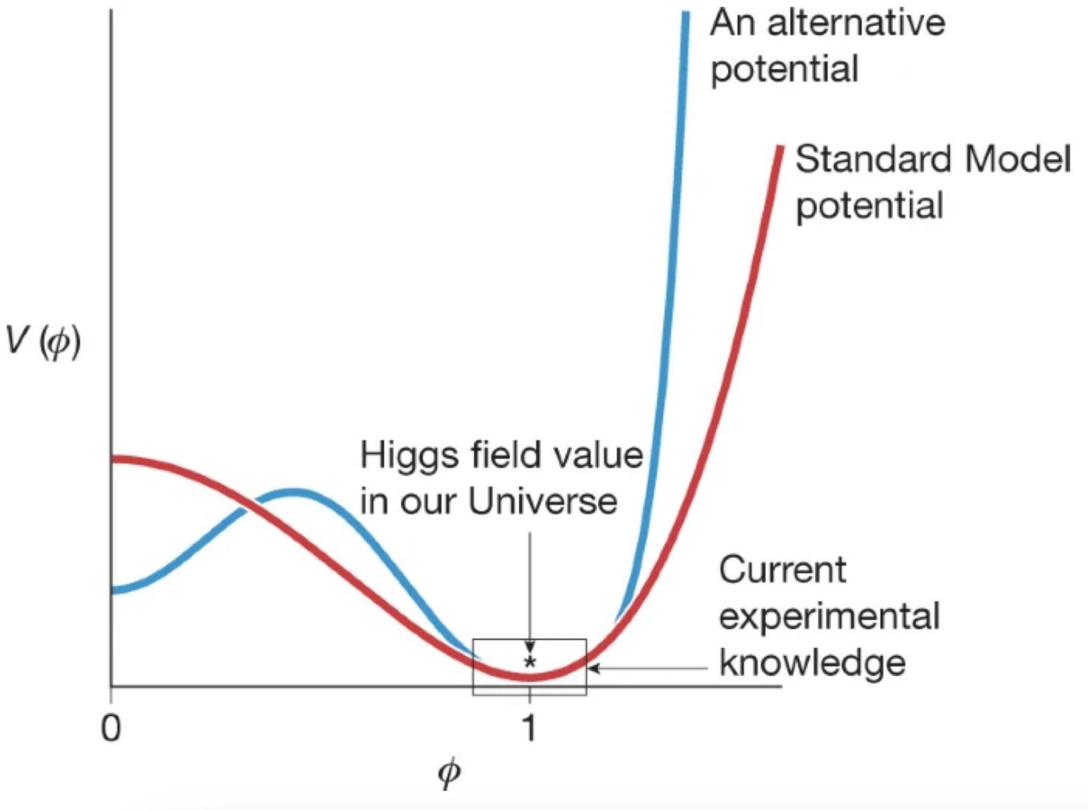
The mantra of CERN’s energy-management panel is “less, better, recover”. We also have to add “credible” to this list, as there will be no future large-scale science projects without major energy-efficiency and recovery objectives. Today and in the future, we must therefore all work to ensure that every MWh of energy consumed brings demonstrable scientific advances.

Challenges and opportunities

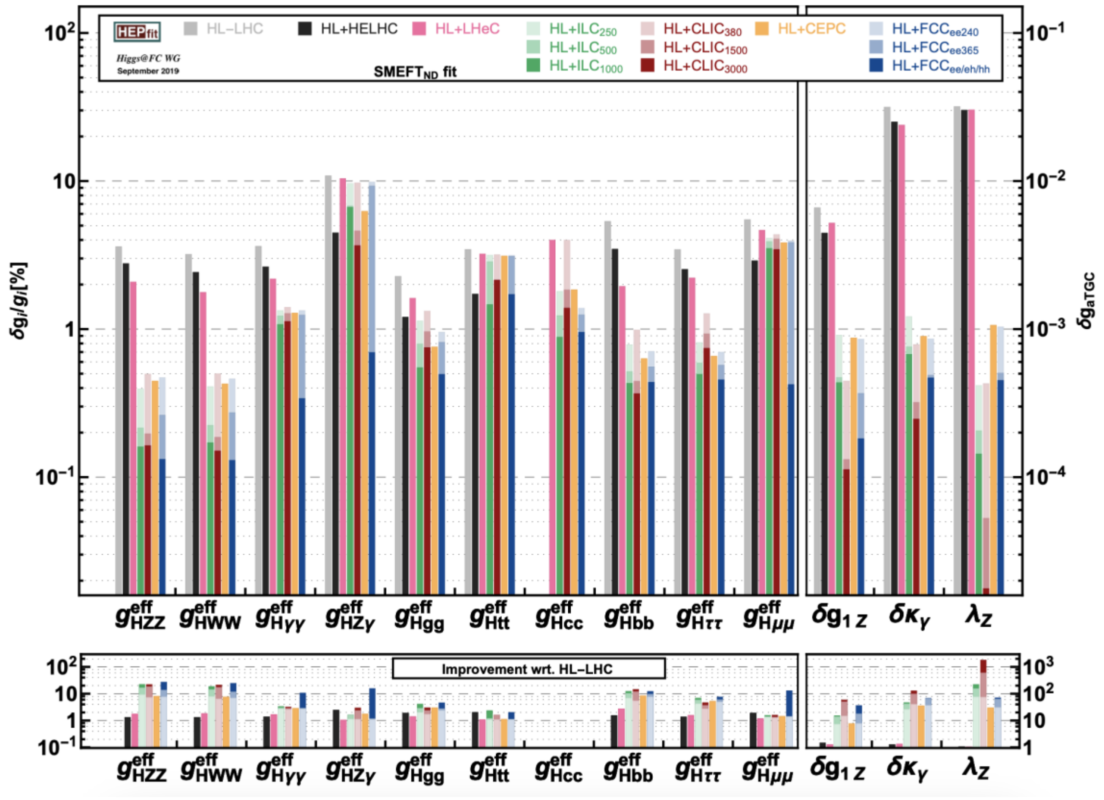
Future colliders generally a step up in scale, and thus cost & environmental impact

Beyond once-in-a-generation discoveries, progress in particle physics can seem highly technical to outsiders

Hard to convey what further exploration of the smallest scales will bring …

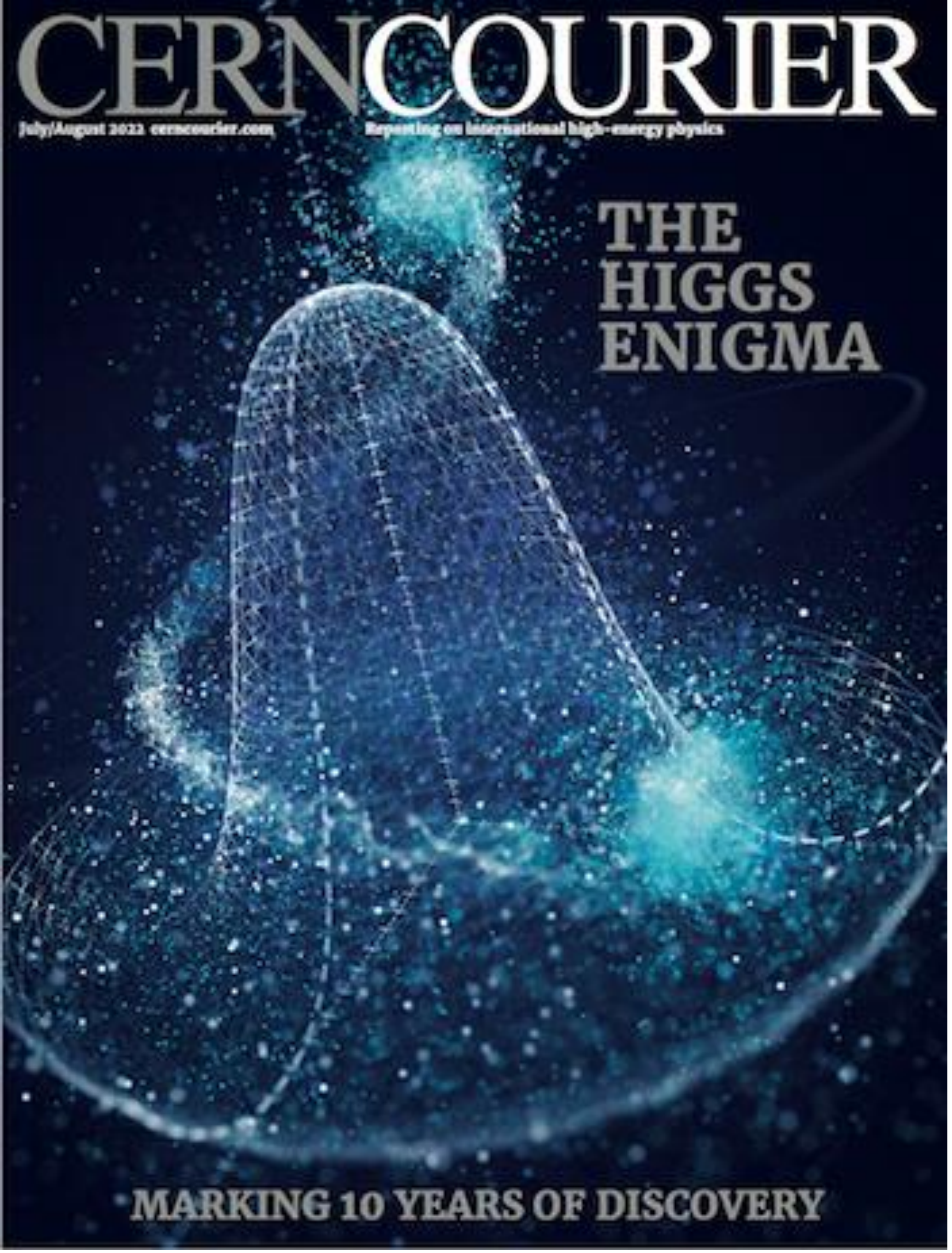


(Snowmass Higgs Forum Report 2209.07510)



(arXiv:1905.03764v2)

.. but it is the same narrative of fundamental exploration that ESA, NASA,.. have



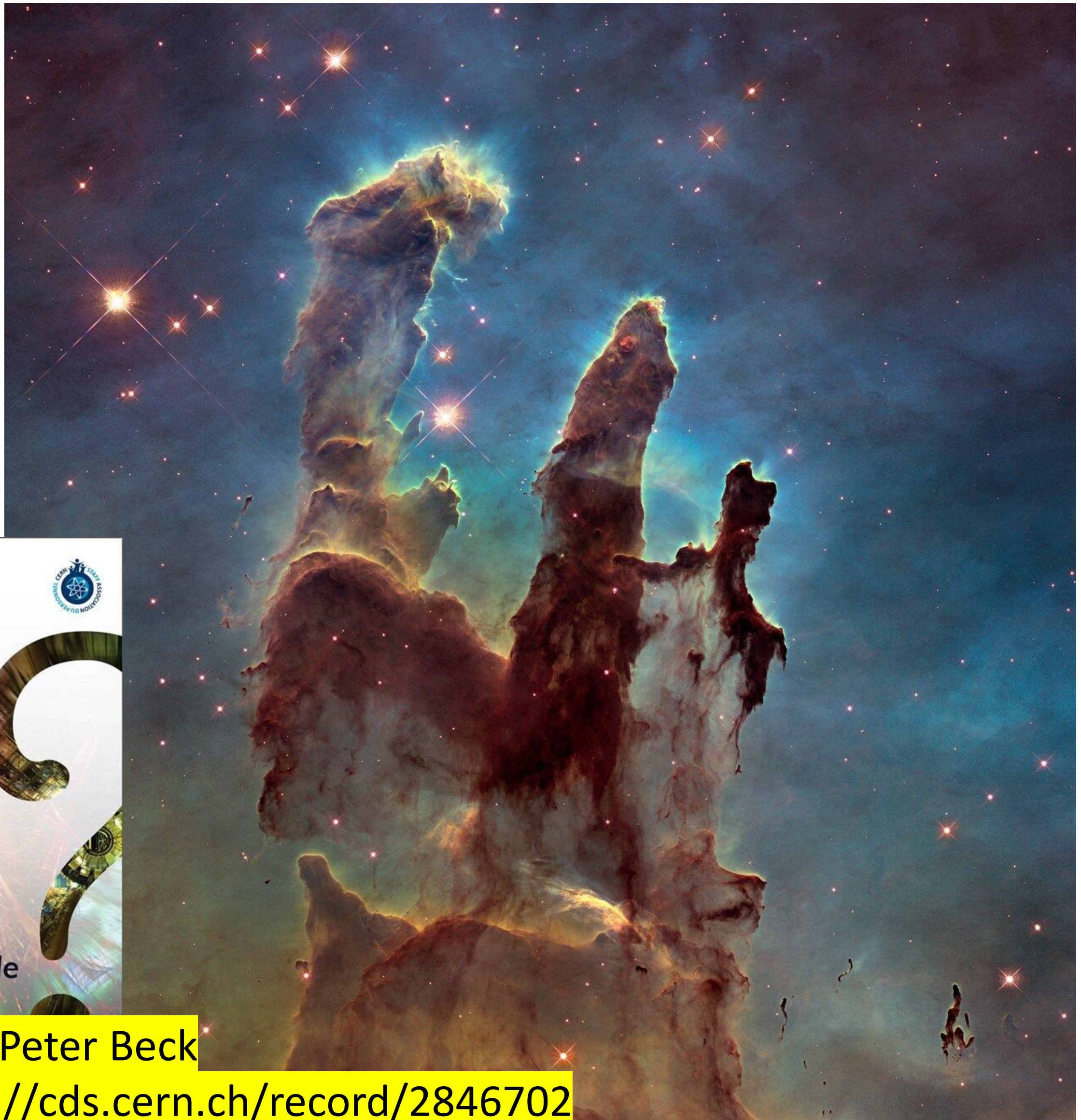
As "a fragment of vacuum" with the starkest of quantum numbers, the Higgs is connected most problematic sectors of the SM, to the evolution of the universe

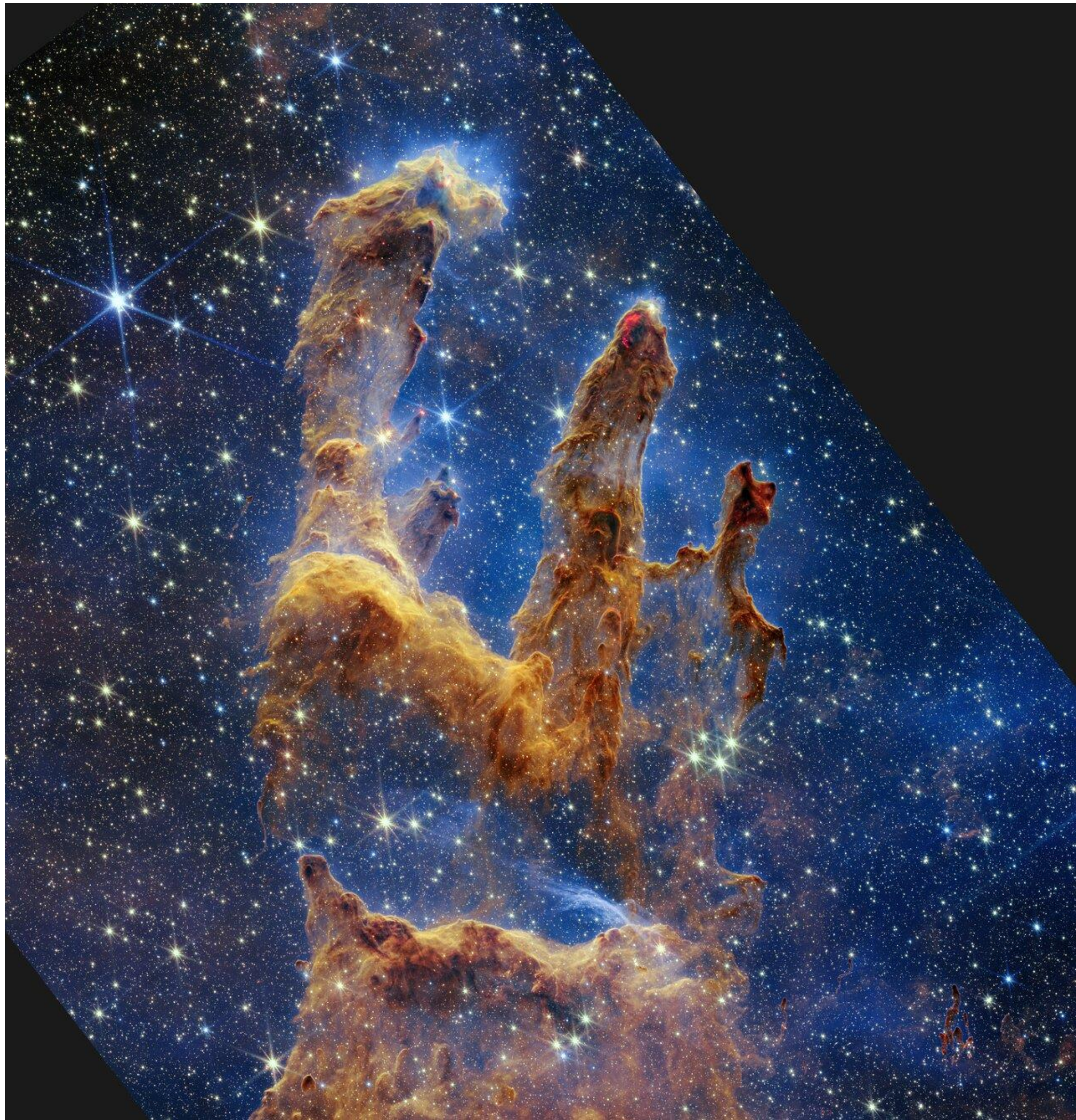
- The electroweak phase transition and possible baryogenesis;
- The existence of other, hidden sectors relevant to dark matter;
- Fermion mass hierarchy (via Yukawa couplings - a new interaction);
- Ultimate stability of the universe, e.g. via self-interaction;
- Fine tuning vs naturalness.

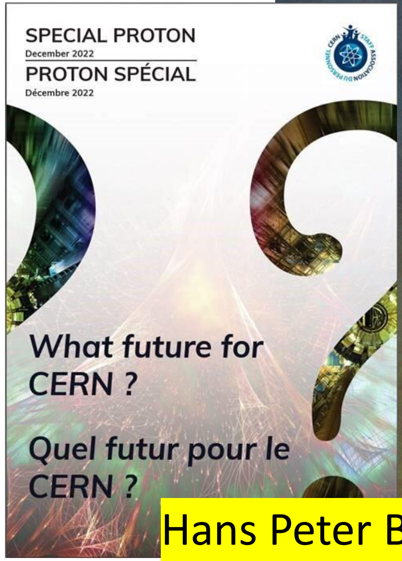
MEASUREMENTS of the Higgs boson's couplings to other particles and the shape of its potential offer direct access to explore these mysteries.

Current picture of the Higgs is "fuzzy"
LHC & HL-LHC will take us far, but only future colliders can fully open potential new-physics vistas

Value of exploring the largest scales rarely questioned







Hans Peter Beck
https://cds.cern.ch/record/2846702

Linking the smallest and largest scales

- Particle physics and cosmology are increasingly overlapping, scientifically and technologically, offering fascinating science narratives and exciting opportunities.

- Collaborations growing. CERN & Einstein Telescope, CERN & SKA, CERN & Euclid, Fermilab & DESI, …

- Early-career particle physicists can look forward to working as one with astroparticle physicists, cosmologists and others to reach the next level of understanding in fundamental physics – especially the dark universe

And important to communicate our goals in the context of *other* curiosity-driven research that are easier to grasp, from exploring the depths of the oceans, extrasolar planets, consciousness, …

Challenges and opportunities

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Beyond the discoveries of new particles, progress in particle physics can be highly technical to outsiders

No guarantee of what will be found

“The spirit of basic research is precisely to follow those paths with unknown destinations; it’s how humanity reached the level of knowledge that sustains modern life.
As particle physicists, as long as the aim is to answer nature’s outstanding mysteries, the path is worth following.”

“We should all renew the enthusiasm that built the LHC, be outspoken about the profound ideas we explore, and embrace the journey that the discovery of the Higgs boson has opened.”

OPINION VIEWPOINT

Physics is about principles, not particles

We should all renew the enthusiasm that built the LHC, be outspoken about the profound ideas we explore, and embrace the journey that the discovery of the Higgs boson has opened, says Veronica Sanz.

Boldly go *The spirit of basic research is to follow those paths with unknown destinations.*

Veronica Sanz *is a theorist at Universitat de Valencia and the University of Sussex.*

Last year marked the 10th anniversary of the discovery of the Higgs particle. Ten years is a short lapse of time when we consider the profound implications of this discovery. Breakthroughs in science mark a leap in understanding, and their ripples may extend for decades and even centuries. Take Kirchhoffs’ black-body proposal more than 150 years ago: a theoretical construction, an academic exercise that opened the path towards a quantum revolution, the implications of which we are still trying to understand today.

Imagine now the vast network of paths opened by ideas, such as emission theory, that led to no fruition despite their originality. Was pursuing these useful, or a waste of resources? Scientists would answer that the spirit of basic research is precisely to follow those paths with unknown destinations; it’s how humanity reached the level of knowledge that sustains modern life. As particle physicists, as long as the aim is to answer nature’s outstanding mysteries, the path is worth following. The Higgs-boson discovery is the latest triumph of this approach and, as for the quantum revolution, we are still working hard to make sense of it.

Particle discoveries are milestones in the history of our field, but they signify something more profound: the realisation of a new principle in nature. Naively, it may seem that the Higgs discovery marked the end of our quest to understand the TeV scale. The opposite is true. The behaviour of the Higgs boson, in the form it was initially proposed, does not make sense at a quantum level. As a fundamental scalar, it experiences quantum effects that grow with their energy, doggedly pushing its mass towards the Planck scale. The Higgs discovery solidified the idea that gauge symmetries could be hidden, spontaneously broken by the vacuum. But it did not provide an explanation of how this mechanism makes sense with a fundamental scalar sensitive to mysterious phenomena such as quantum gravity.

The no-lose theorem we enjoyed when planning the LHC was an exception to the rules of basic research

Now comes the hard part. From the plethora of ideas proposed during the past decades to make sense of the Higgs boson – supersymmetry being the most prominent – most physicists predicted that it would have an entourage of companion particles with electroweak or even strong couplings. Arguments of naturalness, that these companions should be close-by to prevent troublesome fine-tunings of nature, led to the expectation that discoveries would follow or even precede that of the Higgs. Ten years on, this wish has not been fulfilled. Instead, we are faced with a cold reality that can lead us to sway between attitudes of nihilism and hubris, especially when it comes to the question of whether particle physics has a future beyond the Higgs. Although these extremes do not apply to everyone, they are understandable reactions to viewing our field next to those with more immediate applications, or to the personal disappointment of a lifelong career devoted to ideas that were not chosen by nature.

Such despondence is not useful. Remember that the no-lose theorem we enjoyed when planning the LHC, i.e. the certainty that we would find something new, Higgs boson or not, at the TeV scale, was an exception to the rules of basic research. Currently, there is no no-lose theorem for the LHC, or for any future collider. But this is precisely the inherent premise of any exploration worth doing. After the incredible success we have had, we need to refocus and unify our discourse. We face the uncertainty of searching in the dark, with the hope that we will initiate the path to a breakthrough, still aware of the small likelihood that this actually happens.

Those hopes are shared by wider society, which understands the importance of exploring big questions. From searching for exoplanets that may support life to understanding the human mind, few people assume these paths will lead to immediate results. The challenge for our field is to work out a coherent message that can enthuse people. Without straying far from collider physics, we could notice that there is a different type of conversation going on in the search for dark matter. Here, there is no no-lose theorem either, and despite the current exclusion of most vanilla scenarios, there is excitement and cohesion, which are effectively communicated. As for our critics, they should be openly confronted and viewed as an opportunity to build stronger arguments.

We have powerful arguments to keep delving into the smallest scales, with the unknown nature of dark matter, neutrinos and the matter–antimatter asymmetry the most well-known examples. As a field, we need to renew the excitement that led us where we are, from the shock of watching alpha particles bounce back from a thin gold sheet, to building a colossus like the LHC. We should be outspoken about our ambition to know the true face of nature and the profound ideas we explore, and embrace the new path that the Higgs discovery has opened.

Royal visit to CERN

The Duke of Edinburgh paid an informal visit to CERN on April 28th.

Flying his own "Heron" from Malta to Britain, he had landed at Geneva airport the evening before.

The Royal party arrived at CERN at 10 o'clock in the morning of April 28th. With the Duke were Rear-Admiral C. D. Bonham-Carter, H. E. Sir William Montagu-Pollock, British Ambassador to Switzerland, Mr. D. Balfour, H. M. Consul-General in Geneva and Mr. H. L. Verry, United Kingdom delegate to the CERN Council.

After being greeted at the entrance to the Administration Building by Mr. F. de Rose, President of the Council of CERN, Mr. Adams and Mr. Dakin, Sir John Cockcroft and Sir Harry Melville, U. K. delegates to the Council of CERN and Prof. P. Scherrer, representing the Swiss Confederation, the Duke went to the CERN Council Chamber.

Mr. de Rose gave there a short introductory talk: "The reason for creating CERN was that in our various countries it was realized after the war that the tools for the people who try to understand matter, were

The Duke of Edinburgh

The Duke of Edinburgh was born on June 10th, 1921.

He served in the Royal Navy throughout the war, in the Home Fleet, in the Mediterranean and the Far East.

The Duke of Edinburgh has shown particular interest in scientific and industrial developments. In 1951, he was appointed President of the British Association for the Advancement of Science. Since that time he has paid numerous visits to scientific and industrial establishments of all kinds, both in the United Kingdom and the Commonwealth.

In 1959 he represented the British Association for the Advancement of Science at scientific meetings in India and Pakistan.

June 1960

Prince Philip turned to his host, president of Council François de Rose, and asked:
"What have you got in mind for the future? Having built this machine, what next?"

De Rose replied:
"Well, that's a big problem. We have a group who are investigating new principles of acceleration to see whether it is possible to go into higher energies than 25 GeV. But before we present a new project we will have to be absolutely sure that if is feasible and that it is justified. **For the moment we are going to work with the present 25 GeV machine to see what results we can get, because no one has ever explored what happens when you bombard matter at such an energy. We do not really know whether we are going to discover anything new by going beyond 25 GeV."**

Different era: Another early issue likened the 120 million Swiss Franc cost of the PS to "10 cigarettes for each of the 220 million inhabitants of CERN's 12 Member States".

Challenges and opportunities

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Beyond the discoveries of new particles, progress in particle physics can be highly technical to outsiders

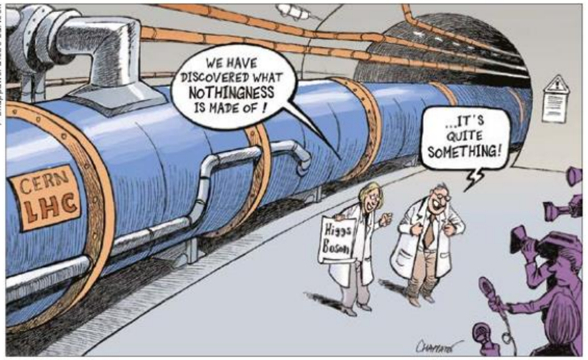
No guarantee of what will be found

Disunity and despondency within the field

Viewpoint

We need to talk about the Higgs

The discovery of the Higgs boson marks the beginning, not the end, of a fascinating journey.



P Chappatte/Globe Cartoon

The LHC's discovery of the Higgs boson in 2012 captured the world's attention, but is too often said to have closed the door to new physics.

By Tim Gershon

It is just over five years ago that the discovery of the Higgs boson was announced, to great fanfare in the world's media, as a crowning success of CERN's Large Hadron Collider (LHC). The excitement of those days now seems a distant memory, replaced by a growing sense of disappointment at the lack of any major discovery thereafter.

While there are valid reasons to feel less than delighted by the null results of searches for physics beyond the Standard Model (SM), this does not justify a mood of despondency. A particular concern is that, in today's hyper-connected world, apparently harmless academic discussions risk evolving into a negative outlook for the field in broader society. For example, a recent news article in *Nature* led on the LHC's "failure to detect new particles beyond the Higgs", while *The Economist* reported that "Fundamental physics is frustrating physicists". Equally worryingly, the situation in particle physics is sometimes negatively contrasted with that for gravitational waves: while the latter is, quite rightly, heralded as the start of a new era of exploration, the discovery of the Higgs is often described as the end of a long effort to complete the SM.

Let's look at things more positively. The Higgs boson is a totally new type of fundamental particle that allows unprecedented tests of electroweak symmetry breaking. It thus provides us with a novel microscope with which to probe the universe at the smallest scales, in analogy with the prospects for new gravitational-wave telescopes that will study the largest scales. There is a clear need to measure its couplings to other particles – especially its coupling with itself – and to explore potential connections between the Higgs and hidden or dark sectors. These arguments alone provide ample motivation for the next generation of colliders including and beyond the high-luminosity LHC upgrade.

So far the Higgs boson indeed looks SM-like, but some perspective is necessary. It took more than 40 years from the discovery of the neutrino to the realisation that it is not massless and therefore not SM-like; addressing this mystery is now a key component of the global particle-physics programme. Turning to my own main research area, the beauty quark – which reached its 40th birthday last year – is another example of a long-established particle that is now providing exciting hints of new phenomena (see p23). One thrilling scenario, if these deviations from the SM are confirmed, is that the new physics landscape can be explored through both the b and Higgs microscopes. Let's call it "multi-messenger particle physics".

How the results of our research are communicated to the public has never been more important. We must be honest about the lack of new physics that we all hoped would be found in early LHC data, yet to characterise this as a "failure" is absurd. If anything, the LHC has been more successful than expected, leaving its experiments struggling to keep up with the astonishing rates of delivered data. Particle physics is, after all, about exploring the unknown; the analysis of LHC data has led to thousands of publications and a wealth of new knowledge, and there is every possibility that there are big discoveries waiting to be made with further data and more innovative analyses. We also should not overlook the returns to society that the LHC has brought, from technology developments with associated spin-offs to the training of thousands of highly skilled young researchers.

The level of expectation that has been heaped on the LHC seems unprecedented in the history of physics. Has any other facility been considered to have produced disappointing results because only one Nobel-prize winning discovery was made in its first few years of operation? Perhaps this reflects that the LHC is simply the right machine at the right time, but that time is not over: our new microscope is set to run for the next two decades and bring physics at the TeV scale into clear focus. The more we talk about that, the better our long-term chances of success.

Tim Gershon is a professor at the University of Warwick, UK, and a member of the LHCb collaboration, with research interests including flavour physics, CP violation and heavy-quark physics.

April 2018

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While there are valid reasons to feel less than delighted by the null results of searches for physics beyond the Standard Model, this does not justify a mood of despondency."

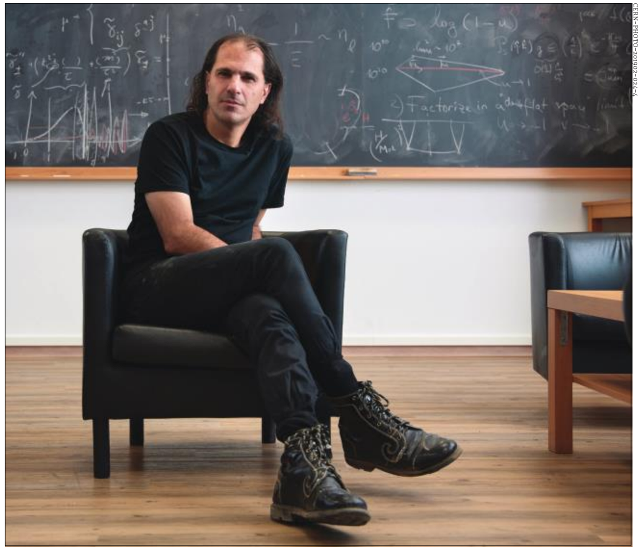
- *Tim Gershon*

OPINION INTERVIEW

In it for the long haul

We have conquered the easiest challenges in fundamental physics, says Nima Arkani-Hamed. The case for building the next major collider is now more compelling than ever.

How do you view the status of particle physics?
There has never been a better time to be a physicist. The questions on the table today are not about this-or-that detail, but profound ones about the very structure of the laws of nature. The ancients could (and did) wonder about the nature of space and time and the vastness of the cosmos, but the job of a professional scientist isn't to gape in awe at grand, vague questions – it is to work on the *next* question. Having ploughed through all the "easier" questions for four centuries, these very deep questions finally confront us: what are space and time? What is the origin and fate of our enormous universe? We are extremely fortunate to live in the era when human beings first get to meaningfully attack these questions. I just wish I could adjust when I was born so that I could be starting as a grad student today! But not everybody shares my enthusiasm. There is cognitive dissonance. Some people are walking around with their heads hanging low, complaining about being disappointed or even depressed that we've "only discovered the Higgs and nothing else".



Nima Arkani-Hamed *of the Institute for Advanced Study in Princeton (photographed at CERN) spoke to* CERN Courier *in February while attending the CERN Winter School on Supergravity, Strings and Gauge Theory.*

So who is right?
It boils down to what you think particle physics is really about, and what motivates you to get into this business. One view is that particle physics is the study of the building blocks of matter, in which "new physics" means "new particles". This is certainly the picture of the 1960s leading to the development of the Standard Model, but it's not what drew me to the subject. To me, "particle physics" is the study of the fundamental laws of nature, governed by the still mysterious union of space–time and quantum mechanics. Indeed, from the deepest theoretical perspective, the very definition of what a particle *is* invokes both quantum mechanics and relativity in a crucial way. So if the biggest excitement for you is a cross-section plot with a huge bump in it, possibly with a ticket to Stockholm attached, then, after the discovery of the Higgs, it makes perfect sense to take your ball and go home, since we can make no guarantees of this sort whatsoever. We're in this business for the long haul of decades and centuries, and if you don't have the stomach for it, you'd better do something else with your life!

Isn't the Standard Model a perfect example of the scientific method?
Sure, but part of the reason for the rapid progress in the 1960s is that the intellectual structure of relativity and quantum mechanics was already sitting there to be explored and filled in. But these more revolutionary discoveries took much longer, involving a wide range of theoretical and experimental

March/April 2019

"Having ploughed through all the 'easier' questions for four centuries, very deep questions finally confront us: what are space and time? What is the origin and fate of our enormous universe? We are extremely fortunate to live in the era when human beings first get to meaningfully attack these questions."

…

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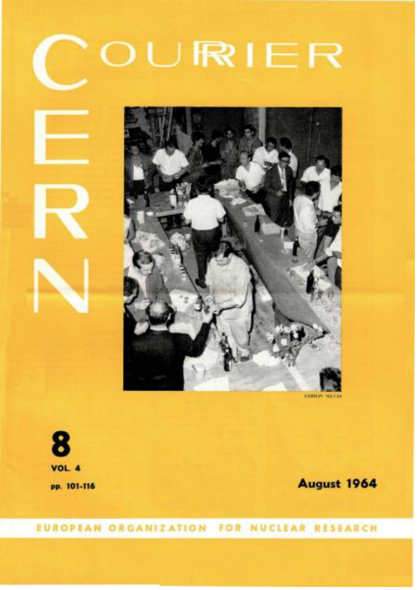
…

"I just can't stand all the talk of being disappointed by seeing nothing but the Higgs; it's completely backwards. I find that the physicists who worry about not being able to convince politicians are (more or less secretly) not able to convince *themselves* that it is worth building the next collider."



CERN Courier, August 1964

Mervyn Hine of the CERN directorate for applied physics addressed ECFA's "Summit program" for the construction in Europe of two projects -- a pair of intersecting storage rings (ISR, which would become the world's first hadron collider) and a new proton accelerator of a very high energy "probably around 300 GeV", which would be 10 times the size of the PS (and eventually renamed the SPS).
Hine estimated the total annual cost to be about 1100 million Swiss Francs by 1973, "in step with a minimum growth for total European science", and concluded:

"The scientific case for Europe's continuing forcefully in high-energy physics is overwhelming; the equipment needed is technically feasible; the scientific manpower needed will be available; the money is trivial.
Only conservatism or timidity will stop it."

Practical considerations - audiences and timing

Public and outsiders

-> Need simple messages (e.g. "Exploring the dark universe" , "A bigger bang", ..) but aside from local audiences, not the time for a major comms push on future colliders – we have the LHC & HL-LHC to talk about, and the assurance that there is innate interest in exploring the universe

"The absolute competence and dedication and hard work of those scientists and engineers was so refreshing compared to the crooks, bullies, liars and murderers that we write about every day… Perhaps people enjoyed reading about something positive, about people doing astounding work, about something far bigger than the world they normally encounter in the news." -- Ian Sample, The Guardian

Decision makers, industry leads, governments

-> Large accelerator RIs are investments, not costs, with structural benefits that give countries and regions a competitive edge; Cheaper than space; Strong and growing scientific and technological links between HEP and other domains; Recruiter of bright young minds into STEM; …

HEP community

-> A post-LHC collider is crucial for CERN and thus for the future of our field; Fascinating questions to explore, but how well is the physics case known?; need better communication – "in-reach" -- within the field, to wrench people out of sociological/historical silos so that they can communicate enthusiastically about *all* future colliders, bust self-defeatism and myths about cost and difficulty of future colliders, and regain the enthusiasm and confidence that built previous machines; if we can't be enthusiastic we will never persuade others

OPINION VIEWPOINT

We can't wait for a future collider

Future colliders are inherently "early-career colliders", and our perspectives must be incorporated into decision making, says Karri DiPetrillo.

K Nimkar

Karri DiPetrillo *is assistant professor at the University of Chicago and a member of the ATLAS collaboration.*

Imagine a world without a high-energy collider. Without our most powerful instrument for directly exploring the smallest scales, we would be incapable of addressing many open questions in particle physics. With the US particle-physics community currently debating which machines should succeed the LHC and how we should fit into the global landscape, this possibility is a serious concern.

The good news is that physicists generally agree on the science case for future colliders. Questions surrounding the Standard Model itself, in particular the microscopic nature of the Higgs boson and the origin of electroweak symmetry breaking, can only be addressed at high-energy colliders. We also know the Standard Model is not the complete picture of the universe. Experimental observations and theoretical concerns strongly suggest the existence of new particles at the multi-TeV scale.

The latest US Snowmass exercise and the European strategy update both advocate for the fast construction of an e^+e^- Higgs factory followed by a multi-TeV collider. The former will enable us to measure the Higgs boson's couplings to other particles with an order of magnitude better precision than the High-Luminosity LHC. The latter is crucial to unambiguously surpass exclusions from the LHC, and would be the only experiment where we could discover or exclude minimal dark-matter scenarios all the way up to their thermal targets. Most importantly, precise measurements of the Brout–Englert–Higgs potential at a 10 TeV scale collider are essential to understand what role the Higgs plays in the origin and evolution of the universe.

We haven't yet agreed on what to build, where and when. We face an unprecedented choice between scaling up existing collider technologies or pursuing new, compact and power-efficient options. We must also choose between centering the energy frontier at a single lab or restoring global balance to the field by hosting colliders at different sites. Our choices in the next few years could determine the next century of particle physics.

Snowmass local organizing committee

Speaking out *Participants of the Snowmass community workshop in Seattle in July 2022.*

The Future Circular Collider programme – beginning with a large circular e^+e^- collider (FCC-ee) with energies ranging from 90 to 365 GeV, followed by a pp collider with energies up to 100 TeV (FCC-hh) – would build on the infrastructure and skills currently present at CERN. A circular e^+e^- machine could support multiple interaction points, produce higher luminosity than a linear machine for energies of interest, and its tunnel could be re-used for a pp collider. While this staged approach has driven success in our field for decades, scaling up to a circumference of 100 km raises serious questions about feasibility, cost and power consumption. As a new assistant professor, I am also deeply concerned about gaps in data-taking and timescales. Even if there are no delays, I will likely retire during the FCC-ee run and die before the FCC-hh produces collisions.

In contrast, there is a growing contingent of physicists who think that a paradigm shift is essential to reach the 10 TeV scale and beyond. The International Muon Collider collaboration has determined that, with targeted R&D to address engineering challenges and make design progress, a few-TeV $\mu^+\mu^-$ collider could be realised on a 20-year technically limited timeline, and would set the stage for an eventual 10 TeV machine. The latter could enable a mass reach equivalent to a 50–200 TeV hadron collider, in addition to precision electroweak measurements, with a lower price tag and significantly smaller footprint. A muon collider also opens the possibility to host different machines at different sites, easing the transition between projects and fostering a healthier, more global workforce. Assuming the technical challenges can be overcome, a muon collider would therefore be the most attractive way forward.

Assuming the technical challenges can be overcome, a muon collider would be the most attractive way forward

We are not yet ready to decide which path is most optimal, but we are already time-constrained. It is increasingly likely that the next machine will not turn on until after the High Luminosity-LHC. The most senior person today who could reasonably participate is roughly only 10 years into a permanent job. Early-career faculty, who would use this machine, are experienced enough to have well-informed opinions, but are not senior enough to be appointed to decision-making panels. While we value the wisdom of our senior colleagues, future colliders are inherently "early-career colliders", and our perspectives must be incorporated.

The US must urgently invest in future collider R&D. If other areas of physics progress faster than the energy frontier, our colleagues will disengage, move elsewhere and might not come back. If the size of the field and expertise atrophy before the next machine, we risk imperilling future colliders altogether. We agree on the physics case. We want the opportunity to access higher energies in our lifetimes. Let's work together to choose the right path forward.

Additional slides

July 1963

“ECFA recommends that high priority should be given to the construction in Europe of:
(a) a pair of storage rings for operation in association with the existing CERN proton synchrotron;
(b) a new proton accelerator of a very high energy (probably about 300 GeV)**”**

300 GeV Project
Latest design thinking

The article on the 300 GeV project was composed before the news broke on 18 April that a new proposal was being presented for discussion to European governments and to European scientists. The following paragraphs bring out some features of the new proposal and can best be understood having read the article.

The initial proposal was for an accelerator of 300 GeV with conventional combined-function magnets in a ring of diameter 2.4 km. Using separated-function magnets an accelerator of 300 GeV could be built in a ring of 1.8 km diameter which could later accommodate a superconducting accelerator of about 800 GeV.

The new proposal is that the project be started with a tunnel of 1.8 km diameter capable of accommodating a 300 GeV accelerator using existing techniques but that initially only half the magnets be installed. Such a magnet ring would permit a maximum energy of 150 GeV. Should superconducting technology develop as hoped, the spaces could be filled with superconducting magnets which would permit a maximum energy of about 400 GeV. During the installation, the disturbance to experimental physics at 150 GeV would be minimal.

If the superconducting accelerator proved successful then the original conventional magnets could be removed, the whole ring filled with superconducting magnets and the maximum energy taken to 800 GeV or perhaps more.

On the other hand, should superconducting techniques not be mastered, the ring could be filled up with further conventional magnets at an additional cost of about 60 million Swiss francs and the accelerator taken to 300 GeV.

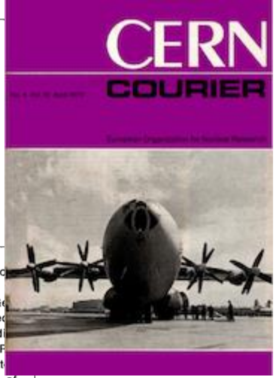
In this way, physics at high energy could start as early as is now possible with the future possibilities of completing the project as a conventional ac-

in the world and based on the mo modern technology.

The present impasse in the 300 Ge project is due to the difficulty of selec ing a site. At the same time it is di turbing to the traditional unity of CEF that only half the Member Stat (Austria, Belgium, Federal Republic of Germany, France, Italy, Switzerland) have so far adopted a positive attitude towards the project. The new proposal could possibly resolve these difficulties. With a diameter of 1.8 km, the accelerator could be built not only on one of the five sites previously under discussion, but also on a site adjacent to CERN-Meyrin. There is sufficient uninhabited ground on the opposite side of the Geneva-St. Genis road to take such a ring and a long ejected beam line. The ground is not ideal but experience in tunnelling the ISR beam transport lines indicates that it is practicable.

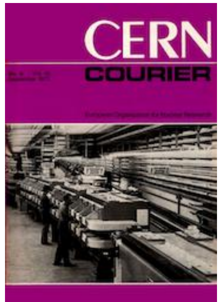
Such a possibility has been discussed before. The construction of a machine in the range of 300 GeV across the road from the existing Laboratory was first proposed by C.A. Ramm on 13 April 1961. An extension to higher energies using superconducting techniques was referred to in a paper of G. Piass on 27 April 1961. The new potential of the missing magnet design and the growing likelihood that pulsed superconducting magnets will be mastered, open up again the discussion of a site at CERN-Meyrin.

Significant economies would then be possible in the project by sharing development effort, overhead costs and services with the existing Laboratory. The conventional accelerator plus experimental facilities would cost approximately 1100 MSF instead of 1431 MSF and there could be similar savings in the cost of running the existing Laboratory. The personnel complement, for example, could stabilize at 5000 people instead of 7400 in two separate Laboratories. In subsequent exploitation of



April 1970

“The present impasse in the 300 GeV project is due to the difficulty of selecting a site. At the same time it is disturbing to the traditional unity of CERN that only half the Member States (Austria, Belgium, Federal Republic of Germany, France, Italy, Switzerland) have so far adopted a positive attitude towards the project. The new proposal could possibly resolve these difficulties. With a diameter of 1.8 km, the accelerator could be built not only on one of the five sites previously under discussion, but also on a site adjacent to CERN-Meyrin.**”**



September 1970

“In June the CERN Council voted a million Swiss francs for detailed studies on the feasibility of installing the 300 GeV European accelerator alongside the present CERN Laboratory. These studies included a number of drillings on the proposed site in order to discover the quality and shape of the underlying stratum of molasse.**”**

A giant LEP for mankind*

*With apologies to Neil Armstrong

During the past three years, the European high energy physics community has debated future facilities which would best respond to the foreseeable physics needs towards the end of this century. The consensus has come out strongly for an electron-positron machine to take colliding beam energies well beyond those which are accessible with PETRA at DESY and PEP at Stanford. This colliding beam machine is commonly known as the LEP (Large Electron-Positron) project.

The reason that LEP so rapidly attracted the enthusiastic support of the full European high energy physics community is that it is the ideal machine to address many of the most important physics issues to have emerged from the past five feverish years of discovery.

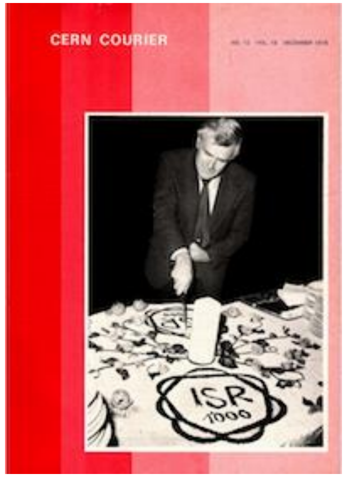
Neutral currents, a new form of the weak interaction, were seen at CERN in 1973. The J/psi particle was dramatically discovered at Brookhaven and Stanford in 1974, rapidly leading to the confirmation of a further type of quark — the charmed quark — and new families of particles. Multimuon neutrino events, discovered at Fermilab in 1975, underlined the existence of new phenomena which were shown, in 1977 at CERN, to be related to charmed particle production. The tau heavy lepton was found at Stanford in 1976 and the upsilon was found at Fermilab in 1977.

The theoreticians, remarkably quickly, made sense of much of what was being seen. Dominant in their thinking is the theory which unites our understanding of both the weak and electromagnetic interactions. This seems certain to be one of the great physics insights of this century. It will

To really clinch the present theories, experiments have to provide several further pieces of evidence and the ideal conditions for these experiments are those which would be provided by very high energy electron-positron collisions.

The emergence of LEP

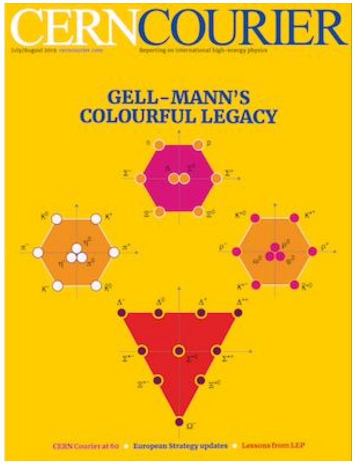
problems (such as orbit stability and magnet tolerances) would make life difficult with energies of 100 GeV per beam in a large diameter ring. In a second phase of the studies, effort concentrated on the design of a machine of smaller diameter (7 km) which would give 70 GeV per beam maximum energy and could reach 100 GeV per beam using superconducting r.f. cavities.



December 1978

"The idea proposed by Weinberg and Salam was to use the 'Higgs mechanism' to give the gauge particles their required mass. This can only be achieved at the expense of additional particles — the Higgs bosons. No trace has ever been seen of such particles.

The hope is that with LEP, physicists will have the tool to explore in depth the details of the symmetry breaking mechanism at the heart of weak interaction dynamics."



July/August 2019

Herwig Schopper:

"The first proposal for LEP was initially refused by the CERN Council because it had a 30 km circumference and cost 1.4 billion Swiss Francs...
The cost of LEP made some Member States hesitate because they were worried that it would eat too much into the resources of CERN and national projects.

After long discussions, Council said: yes, you build it, but do so within a constant budget. It seemed like an impossible task because the CERN budget had peaked before I took over and it was already in decline!"

We are in a different communications era..

Input to 2020 ESPPU:

The *International Particle Physics Outreach Group* emphasises the strategic relevance of concerted, global outreach activities for future colliders: "The success of such endeavours depends greatly on the establishment of broad public support, as well as the commitment of key stakeholders and policymakers throughout Europe and the world".

The *European Particle Physics Communication Network / Interactions.org emphasise:*

- Fast pace of change in social media & speed of dissemination of good news, bad news and rumours;
- The need to maintain trust and transparency in an era where there appears to be a popular backlash against expert opinion;
- Timescales and costs: Proposals for major international particle-physics experiments are infrequent, and when they are proposed, they seem disproportionately expensive when compared to other science disciplines.

.. Especially after financial crash, covid and Ukraine