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The probability of discovery

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ABSTRACT

In 2009 the Large Hadron Collider (LHC) turned on and became the most complex scientific instrument ever put into operation by mankind. The LHC is what is called a "discovery machine", meant to explore new limits at the high-energy frontier. Any cost–benefit analysis for such an instrument for fundamental research has to gauge the opportunities and risks of such a facility, and in particular major discoveries have a significant role in that balance. In this paper we discuss the challenges and uncertainties of discoveries in fundamental science, using the recent history and expected near future of the LHC as an example.

1. Introduction

In 2004 a book appeared called "the Probability of God". It claimed to give a scientific statistical analysis of the deriving the probability for a divine entity and the so-called ultimate truth, it by necessity is based on certain assumptions that cannot be really controlled, so ultimate any such calculation of a probability is not particularly inspiring, or even meaningful. In a similar, but nevertheless more controlled way the probability for a discovery is in many cases not strictly quantifiable, though there are exceptions. We'll discuss both scenarios below, as these unfold in particle physics – or high energy physics – today.

Scientific progress in modern times is only possible thanks to funding by national governments, funding agencies, international institutions such as the European community, and even private funding. Research is typically categorized as application-driven or curiosity-driven. The application-driven research is generally easy to motivate, pointing to the many technology developments that happened in, say, the last 50 years. This is no doubt correct but it is often overlooked that much of the application-driven research is based on our deep knowledge gained by curiosity-driven research. Big examples are quantum mechanics and relativity, which were new directions in our understanding of nature, discovered just over a hundred years ago and now the basis of many of our technological applications.

Both application-driven research and curiosity-driven research are now, and will remain in the future, necessary to have new breakthroughs in progress, which will continue to be for the benefit of all of mankind.

Society controls how the funding for research is spent and would ideally like to have a metric to referee, as always with limited resources, to select which directions to support with priority. By itself this is not a problem which one can solve in a unique and unambiguous way, due to the, by construction, various risk factors involved. Application-driven research is based on applying or extrapolating the present well-established knowledge into a new regime. While this can be technically challenging and does not necessary always lead to success, there is a clear path and evaluation procedure of the risks, using milestones. Examples of such challenging projects are the development of quantum computers and nuclear fusion as a new energy source. In particular for the last one, while one can design detailed projects on how to proceed, several intermediate stages are needed to check how these predictions match with the reality, possibly introducing deviations from the original project, or in the worst case leading to showstoppers. But the clear benefits for mankind of such a successful program are not challenged by anybody.

Curiosity-driven research may look at first as higher risk and less clearly of immediate benefit for society so one could have the tendency to give it less priority and thus be more critical on the funding for this branch. This would be a mistake however, as since mentioned above, present-day technology stands on the pillars of our advances in our understanding of the fundamental laws of Nature. So continuing fundamental research is not a luxury for a developed society, it is a necessity! Fundamental research is discovery-driven. It goes into new regimes and areas to explore the unknown. Theories at hand will often make predictions for what we may find there and experiments can explore that. Our biggest breakthroughs often take place when experimental results or new theoretical insights give surprising and unexpected results. The discovery of quantum mechanics is a typical case. But sometimes the

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experimental data may at the end perhaps not reveal the anticipated effect or breakthrough. Was the investment in the project then lost? I will argue this not to be the case in general: negative outcomes of an experiment can be as important as positive ones and can be the seed and start for a completely new direction in science. An important example is the Michelson–Morley experiment, which set out to find if the postulated aether existed, as a mysterious medium surrounding all of us, used for the transport of electro-magnetic waves. The data showed, somewhat to the surprise of many scientists at the time, that the speed of light is the same in all directions, eliminating the existence of a medium such as the aether. This 'failure' was an inspiration for Einstein and others and led to the development of special relativity.

The need for more and more sensitive experiments for future discoveries essentially always leads to the need for the best possible technologies. This need is usually associated with a strong research and development activity, leading to technological breakthroughs and advances for example in detector, computing or software areas. One of the best-known of these 'spin-off' applications is no doubt the "World Wide Web", a protocol that brought the internet to people's homes and their smart-phones. Right now we live at a time where it became unimaginable that we would not be constantly connected to the internet (for the better or the worse) but perhaps few remember this was invented 25 years ago in a place called CERN, in a particle physics laboratory by a few computer geeks, and driven by the need of the scientists of that laboratory to communicate 24/7 on their science measurements.

2. Particle physics

One direction of curiosity-driven research, with which the author is particularly familiar, is so-called high energy physics or particle physics. Particle physics aims to unveil the fundamental laws that govern the interactions and dynamics of the smallest constituents of matter, and to understand what the Universe is made of. Particle physics is a discipline that developed in the second half of the last century, after the discoveries of quantum mechanics, the structure of the atom and nucleus, and the first discoveries of several particle types in the first part. Detector techniques such as emulsions, cloud and bubble chambers, were used to discover that there were more particles in Nature than the ones assumed to that date (1930s) i.e. the electron, proton and photon. Many of these new particles were found in so called cosmic rays, i.e. beams of particles that come from outer space and hit the earth's atmosphere, leading to showers of particles that propagate through the atmosphere to the surface of our planet. Next they were produced in the first particle accelerators, or also called atom smashers, by converting energy of the incoming beam particle on the target via Einstein's best-known formula: $E = Mc^2$, i.e. the conversion between energy (E) and mass (M) via the speed of light (c).

By the end of the 50s the whole zoo of newly-found particles was so large that many scientists started to feel uncomfortable with the sheer amount of them. Hence new theoretical models were proposed that analyzed the patterns and suggested that there would be a more fundamental underlying structure for the particles called 'hadrons'. Hadrons (greek: thick) are subatomic particles that can take part in the strong interaction – one of the fundamental forces of Nature – that binds protons inside the nuclei of atoms. Hadrons differ from another class of particles called leptons (greek: light) such as the electrons and muons, by their interaction via the strong force. Leptons to date are still presumed to be point-like elementary particles. Physicists have theorized since the 1960s, and ample experimental evidence since has confirmed the picture that hadrons are made up of so called smaller entities called quarks. At first this was thought just to be a mathematical tool, but these quarks were actually discovered in 1969 at two-mile long accelerator at the SLAC laboratory near the Stanford Campus in Palo Alto, California. This discovery had far-reaching consequences for our understanding on the smallest building blocks we know of in Nature.

By the end of 70s we knew about the following fundamental particles: the electron, the muon, the tau-lepton, neutrinos, and 5 different types of quarks. In the 90s a sixth type of quark was discovered. We also knew that there were four fundamental forces: The well-known electromagnetic force, the nuclear strong force, mentioned above, the nuclear weak force (which is responsible e.g. for radioactive decays) and gravity. For the first three forces we have a quantum field theory with local gauge symmetry, derived from symmetry principles and picturing the interactions as the exchange of a field quantum of the theory between the fundamental particles. For the electromagnetic force, this field quantum is the well-known photon, for the strong force it is called the gluon and for the weak force these exchanged particles are the so called heavy W and Z bosons: they are about 100 times heavier that e.g. a proton (which is about 1 GeV in energy units, see below).

The set of fundamental particles plus the three fundamental interactions that can be described by gauge theories have been the basis of an extremely simple and at the same time very powerful 'model' to describe the fundamental laws of Nature: the so called Standard Model for Particle Physics (Guidice). It allows to describe all fundamental interactions we have observed so far and make predictions for new experiments (which have subsequently been verified). Nobel prizes have been awarded for those who brought critical insight into the development of the Standard Model over the last decades. Probably we should rather call it now the Standard Theory instead of Model.

Having the Standard Model gives a feeling of triumph, that with a few equations that fit on a T-shirt or a coffee mug (and actually are sold as such in the CERN souvenir shop) one can describe the fundamental particle interactions with great precision. Yet we are not completely happy with it!

Until a few years ago, one important missing part in the puzzle of the Standard Model was: what gives mass to the fundamental particles. We know that the mass of the electron is tiny but is clearly non-zero: it is 0.5 Mega Electron Volt or MeV, which is about 1/2000 of the proton mass. We also know that the quarks have masses, ranging from a few MeV to about 175,000 MeV for the heaviest one, the so-called top quark that was discovered in 1995 at the atom smasher called Tevatron, located near Chicago, US. However in the mathematical formulation of the Standard Model, to preserve gauge invariance, all particles had to have zero mass. It was not easy to introduce masses for particles and preserving gauge invariance at the same time. Yet, by drawing from ideas of superconductors and solid-state physics, a number of scientists in 1964 succeeded in doing exactly that by introducing what we call now the Englert–Brout–Higgs (BEH) mechanism in the theory. While this 'theoretical discovery' should lead to an immediate breakthrough, it took in fact some years before its value, and that of gauge theories in general, was fully appreciated. Indeed in the 60s it was not yet the time for the gauge theories, simply because the scientists were not yet able to make sense of the calculations: the results they got were infinities, which isn't very good for a theory that you want to use to predict something meaningful!

The breakthrough theoretical discovery to remedy that came in the early 70s, through a mathematical technique called "renormalization", a technique that allowed to do away with the infinities. From that moment on gauge theories gained strong support by the community, especially when the by theory predicted "neutral currents" were experimentally discovered. All worked out fine when one assumed that the BEH mechanism was at work. But this was a hypothesis. There was no proof that either this or possibly some entirely different mechanism was at work. The story on how this was solved is a major point of the hunt for discoveries at the LHC and will be discussed in detail in the next sections.

Another observation to challenge the Standard Model is the more and more emergent evidence that there is more matter in the Universe than 'meets the eye'. Already in the 1920s astronomical measurements of rotation curves of galaxy clusters showed a very odd effect. The rotational speed of the galaxies at the edge of the clusters was larger

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by more than a factor 10 than what would be expected from Newtonian dynamics and the amount of visible (luminous) matter distributed in these galaxy clusters. A way to explain this discrepancy with observation was to postulate a new kind matter that does not interact, or only very weakly interacts, with the matter world is made of. This new matter has been coined as 'dark matter' and is still one of the biggest unexplained mysteries today. The Standard Model at present does not account for it.

Over the years, and thanks to a number of new discoveries happening on the way, some of which were quite unexpected, we find ourselves in a situation where we have the Standard Model that seems to work well at our present energy scale probed, i.e. a few hundred billion electron-volt (or also called Giga Electron Volt, GeV), but does not seem to be complete, and may not hold at very high energies (perhaps a billion billion electron volt) which tells us that perhaps this is just a model for the current energy regime. If dark matter consists indeed of a new particle, our table of fundamental particles known so far to us does not contain a candidate for it. Although not discussed in detail here, also the mysterious neutrinos have turned out to be more exciting than perhaps thought at first: they seem to have very tiny masses, much smaller than the ones of electrons. All these observations tell us that the Standard Model as we know it is not the end of the story, there is more!

The importance of understanding what the physics processes and dynamics are at the highest energies is nothing less than understanding what went on just after the Big Bang at the beginning of the Universe. For example dark matter must have been present and created at a very early stage of the formation of our Universe. How did the Universe develop from a presumed start of pure energy to the matter anti-matter asymmetry that we observe today? Possibly supersymmetry or extra space dimensions were at work; we will explore such a possibility in Section 5. But all these theoretical ideas have one thing in common: they need experimental verification. They require that we discover the characteristics of the phenomena that accompany these predictions. E.g. for supersymmetry this means we should detect a whole slew of additional particles with relatively high masses.

We need to test the predictions and search for new phenomena and particles. The accelerators are the scientific tools that allow us to produce massive particles or look indirectly for their effect on calculable processes. In this paper we will discuss the Large Hadron Collider (LHC), the scientific instrument which was benchmarked as one of the big science projects in this study report. But other experimental approaches are possible and are in fact vigorously pursued as well.

3. CERN and the Large Hadron Collider

CERN is a European organization created in 1954 to unify a post-war Europe again in common science research. Originally it had 12 member states, and started as a centre for nuclear research, but by 2014 had 21 member states, and is since long better-known as the European Laboratory for Particle Physics. CERN is located at the foot of the Jura mountains near Geneva, in Switzerland, at the border with France. It provides infrastructure and accelerators to conduct experiments in pursuit of mostly fundamental research. There is also spin-off with more direct applications that I will briefly mention at the end. The biggest community of CERN are the users, mostly experimental physicists that come literally from all over the world – from Chile to New Zealand and most of the world that is in between — to build and conduct experiments.

The data are produced at CERN, where the accelerators and experiments are, and with modern data-highway techniques distributed to the different laboratories and institutes around the globe. On the LHC experiments the 'sun never sets', in particular the larger collaborations. While scientists from Japan call it the day, European scientists are hard at work and the US scientist getting ready for breakfast. Data, results and queries need to be communicated and responded to in an efficient way. Information should always be accessible to all the involved scientists at any time and any place. This is what drove the development of the World Wide Web. The need was there, the technology was at hand, and the rest is history (Anon).

The latest large scientific equipment that came into operation at CERN, and for that matter, worldwide, is the LHC (Anon). This is a machine of extreme complexity and extraordinary size, a true technological marvel. The LHC is installed in a circular tunnel 100 m underground and has a length of 27 km. It consists of more than 1200 large superconducting dipole magnets, each delivering a very strong magnetic field of 8.4 Tesla, and these operate at a super-fluid Helium temperature of 1.9 degrees Kelvin. This ring-shaped accelerator has such a large size and such high magnetic dipole fields in order to contain a particle beam than can be accelerated up to an energy of 7 TeV. Two counter-rotating beams are brought into collision leading to a total energy of 2×7 TeV $= 14$ TeV in the so-called centre of mass system. Higher energies, as always desirable by the scientists, can only be reached by an even larger ring and/or even stronger magnets. Projects such as the LHC take a very long time to design, construct and be brought into operation, like perhaps 20 years, so scientists today are already now thinking ahead on possible new machines to reach even higher collision energies! First workshops on the LHC and its physics potential were already organized in the early 80s.

At specific points along the ring, the two beams in the machine circulating in the opposite direction are brought into collision. There are four interaction points at the LHC and all of them have detectors surrounding these points in order to detect and measure the particles that are produced in the collisions. It is in these collisions that the scientists hope to detect traces of new as of yet unknown particles that will help us to understand better the next layers in the laws of Nature. The experimental collaborations at the LHC are large. The four main experiments have a number of participants that range from 500 to about 4000 scientists and engineers.

4. The discovery of the Higgs particle

One of the most important outstanding questions in particle physics over the last decades was whether the so-called BEH mechanism is the right answer for the mass-generating field for matter particles and gauge bosons. Matter particles are fermions and gauge particles are bosons, which relates to specific spin quantum number properties of the particle. The problem of mass appeared first in the electro-weak theory, describing in a common framework the electromagnetic and weak fundamental forces, where the photon appears mass-less, and as a consequence will have to move with the speed of light, but the Z and W bosons are very heavy. The BEH mechanism postulates that at some early stage in the creation of the Universe, in a small fraction of the first second, a scalar field came into existence that spanned the full space, which gave 'resistance' to the motion for different particle species. Photons can move freely while Z bosons are strongly hindered. Experimental evidence for the mechanism would be to observe the existence of this field directly but we do not know how to do that. However with each gauge field, there is at least one quantum of the field associated with it, namely a particle with the quantum properties of that field. Hence the experimental community set out to find this new particle. The particle was expected to be neutral and to be a scalar, as it represents a scalar field. The first Higgs Hunter guides, e.g. (Ellis et al., 1976), were written in the middle of the 70s when the Standard Model was on its way up in general acceptance. Within the Standard Model one could in fact predict roughly what a Higgs particle or also called Higgs boson would do when created in a high energy particle collision. For example it was pointed out that a Higgs particle is very unstable and can live only very shortly, a tiny part of a fraction of a second, so that in fact an experiment will never see a Higgs particle actually whizzing through its detectors. Essentially at the point of creation and even before it can move as much as a small fraction of a nano-meter, the particle will decay in particles already known to us, such as in two

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photons, or in two heavy quarks, or possibly even in two heavy gauge bosons. The theory can even predict the rates of the decays in the different particle species, but the predictions result on the a priori unknown mass of the Higgs boson.

For an experimentalist who is planning to make a discovery, this is good news. He can use that information to form a strategy on how he would want to design a detector that is looking into the debris that is produced in the collisions to find the key footprints of a Higgs particle that has decayed according to the patterns predicted by the theory. As an additional aid, since the Higgs boson is a particle, it has for any practical detection method at the current accelerators a well-defined value for its mass. Hence the kinematic distributions of e.g. two-photon data would show a pronounced peak around the value of the Higgs boson mass.

Now, there is one thing that the theory was not able to predict and that was what exactly *is* the mass of the Higgs boson. It could be as easily 1000 times lighter that the proton but at the same time about 1000 times heavier. So the scientists were condemned to search in the whole possible range. The reach in Higgs boson detection in an experiment, and therefore the success for a discovery of the experiment, is limited by three main conditions:

- Do the collisions at the experiment have enough energy to create the mass M of the Higgs particle, keeping in mind that $E = Mc^2$? This is given by the maximum energy of the accelerator beams and the type of beam particle used.
- Do we have enough collisions, accumulated in, say, one or a few years, to produce enough new particles with mass M? For any mass M we can calculate the probability of producing such a massive particle in a collision.
- When we start producing new particles of mass M, can we detect that we have a signal over the background, the latter being mostly Standard Model interactions that are produced at rates which can be a million times larger, and can mimic a new particle signal? Moreover the detectors should not allow for any spurious effects that could fake the existence of a new particle.

Experimentalists setting out for a discovery are well aware of these challenges that lay ahead. First there are the accelerators. The Higgs particle was searched for during the 70s, often not as a main goal of the experiment but as a side opportunity. Low and behold, in 1984 a new particle was reported from an experiment in electron-positron scattering at a machine in Hamburg, which showed a bump, and could be a new particle decaying in photons with a mass of 8.31 GeV. There was no other experiment that could immediately verify this result and it remained in the science community for a few months, until the experiment had collected a new data-set that showed no bump at all! While the mystique on the cause of the peak in the first data-set remains, which can go from statistical fluctuations generating a signal, to perhaps mistakes made in the analysis, it was not to be. The Higgs boson was not found yet!

The hunt for the Higgs would be on fully with the start-up of the Large Electron-Positron (LEP) collider, at CERN, which in its last year of operation, at the highest energy, also observed some indication in the data that a new particle with the characteristics of a Higgs was produced. Unfortunately the experiment had only a handful of events and the machine had to be stopped and dismounted in order to start the installation of the LHC. No signal ever reached a discovery level. A good account of this story is given in (Sample).

This begs the question on how to quantify what constitutes a discovery. When can we be sure that the signal is above any level of doubt? In case of a small signal over background we use the statistical significance of the signal over background. If that significance reaches a level of 3 standard deviations, or so called “3-sigma”, that means that we have one chance in 750 that the observed signal is in fact a statistical fluctuation of the background. While this is a small probability, it generally does not constitute enough proof for a discovery. In high energy physics we call a signal with this strength ‘evidence’ for a new signal. Many discoveries are first ‘evidence’ before they become a discovery. A discovery itself needs a statistical significance of 5-sigma, which is a chance of 1 in 3.5 million that the signal is in fact a statistical fluctuation of the background. The significance of the results of the ‘Higgs observation’ at LEP was about 2-sigma, so below the evidence level. The LEP result was also for a lower mass than what will be found later at the LHC.

The next collider to take over the hunt for the Higgs after LEP was the Tevatron proton anti-proton collider at the US particle physics laboratory called the Fermi National Accelerator Laboratory (FNAL), near Chicago. The maximum energy of the collider was 1.96 TeV (Tera Electron Volt), which at first seems more than sufficient to produce Higgs particles up to masses of 1 TeV. But this is not so. The reason is that the (anti-)protons are composite particles that contain quarks and gluons, which carry a fraction of the total proton energy. The quarks and gluons are the fundamental particles that will be involved in the processes producing the Higgs particles. At the Tevatron energy typically a Higgs search can be conducted for a mass up to a few hundred GeV. However there is a second more important issue, which are the event rates. The production of a Higgs particle is a fairly rare process. For example, at the LHC a Higgs particle is expected to be produced only once every 10 billion collisions. Anti-proton beams, used at the Tevatron cannot be made at the same high intensity as proton beams. This means that the luminosity, ie the ability to create a sufficiently high rate of collisions, of the machine is limited, and as it turns out, too limited to make a discovery. A serious luminosity upgrade – with corresponding detector upgrades – was what this machine would have needed to make the Higgs discovery!

Enter the LHC! The main mission of the LHC was from the onset to go boldly where no man has gone before, and seek out new physics and new particles in the energy regime that it was set to explore. The LHC is labelled a “discovery machine”: the mission of the LHC, as it was defined in the 80s, was to

- find the mechanism of electro-weak symmetry breaking (aka the BEH mechanism or other)
- find physics beyond the Standard Model. We discuss that in the next section.

The design parameters of the LHC are a total centre of mass energy of 14 TeV and luminosity of $10^{34}\,\mathrm{cm}^{-2}\,\mathrm{s}^{-1}$ in technical units. This guarantees that with a few years of data taking we should produce at least a few hundred Higgs particles even for the high mass regions which have the lowest rate.

A second important ingredient for “planning for discoveries” is to have two independent experiments that will have similar but complimentary capabilities to discover the new particle. At the LHC there are two experiments with similar capabilities, but different choices for the detector technologies. These experiments are named ATLAS and CMS, and each has about 4000 scientists and engineers working in their collaboration. Moreover since each experiment has about 100 million read-out channels, several thousand kilometres of cables, very sophisticated event data triggers and data acquisition systems, over 100,000 lines of computer code for event reconstruction, detector calibration and simulation, errors and mistakes can never be 100% excluded even after a thorough bug finding campaign in the experiments.

It has been often argued or even questioned if it is worth the expenses to have two experiments that are basically equivalent in capabilities. Let's look at the economics first for the case the LHC. The total cost of the LHC is of order of 10 billion CHF (Swiss Francs, the official currency used for CERN projects) as reported in other contributions to this volume. Each of CMS and ATLAS experiments individually cost about 5% of that. Hence relatively speaking the price of the experiments is cheap in comparison to the cost of the whole project. Therefore it

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would be very unwise to save on the funding for experiments, and thus undermining the capabilities for a discovery that needs independent verification. Having two (or more) experiments that can hunt for an important discovery gives at a circular collider such as the LHC that:

- The amount of analysable collisions doubles, i.e. the effective increase of the luminosity of the machine increases by a factor two, if there are no penalties for serving two experiments at the same time with collisions.
- Two different teams hunting simultaneously for an important discovery also increases the self-pressure within a scientific environment. In such a 'race with two' none of the two experiments would like to be the experiment that 'confirms' the discovery announced earlier by the other experiment, but be there first. On the other hand no experiment wants to be wrong and announce a discovery prematurely which later on turns out to be a fluctuation or even worse, an analysis error. This is actually very effective for the self-critique within the experiments, and leads to much fewer spurious results being made public, which can be confusing and potentially very embarrassing. It suffices here to recall the recent story on the much publically acclaimed but tragic story on the neutrinos that moved faster than light.
- One can have completely independent groups using a different detector that by different choices of detector technologies will have different software, different systematic uncertainties on the measurements and possibly even different analyses methods for the discovery. In fact the competition certainly helps to push the analysing groups within an experiment towards new and more sensitive detection methods. This was certainly the case for the Higgs analyses at the LHC.

The experiments by themselves have to be up to the task for the discovery. Years of careful design typically went into the preparation before starting construction. In the case of ATLAS and CMS, both for the search for the Higgs particle and the search for particles from new physics scenarios, the detectors need to have the following capabilities: very good photon and lepton identification and measurement capabilities, excellent particle jet measurement capabilities, b-quark and tau-lepton identification, and hermeticity. These criteria have driven the different design choices of the experiments, but the overall budget has always been an important constraint. Note that the participating institutes in the experiments have to generate the funding.

With the accelerator in place and the experiments tested and well calibrated, the scene is set for hunting the Higgs particle. What is still needed is that the accelerator delivers the required amount of collisions. The LHC started its operation in 2010 at a centre of mass energy of 7 TeV, which is half of its design value. At this energy the expected Higgs production rate is less than half of the one at the highest reachable LHC energy, and the luminosity of the machine turned out to be lower too. This undermined at first the hopes for a discovery in the first run 2010–2012 of the LHC. However the analysis groups could compensate the reduced statistical power by developing more sensitive analyses methods.

By the end of 2011 the LHC had delivered 5 fb^{-1} of collisions to each of the two experiments. This was a critical data set for either showing the first hints of possible signal at some place in the mass spectrum, or starting to exclude that the Higgs particle existed at all. The exclusion of the Higgs boson would be a very significant discovery in its own right: similar to the results of the Michelson-Morley experiment one would be forced to abandon certain theoretically preconceived ideas, which could then inspire to a revolution and new thinking in science. Hence the data collected at the end of 2011 were very important and CERN organized a seminar in December where the two experiments were invited to report their latest findings. The data was indeed talking in very clear terms: there was no sign of the production of any new particle in the whole searched region of 110 to 700 GeV, except for a small region around 125 GeV, where the data at this point in time 'refused to exclude the presence of a new particle'! In fact a possible small signal was seen with a significance just shy of 3-sigma. The real exciting part was that both experiments saw the excess at exactly the same place!

One could argue whether at this point in time the experiments had already their hands on a discovery and could claim so. According to our strict criteria for discoveries, discussed before, this was not the case and therefore the experiments made no claims, acting responsibly. But of course those not part of the experiments were less bound by such safety rules. At an occasion of a workshop I participated a theorist said during his presentation with tongue in cheek: "The Higgs was discovered at 125 GeV last month. Somebody should tell those experimenters".

The December 2011 results where a strong stimulus for the accelerator and the experiments to push even further. Typically the experimenters could increase their sensitivity by another 25% and the machine delivered by summer 2012 another 5 fb^{-1}, at a centre of mass energy of 8 TeV this time. Many of these new ideas were the work of very clever PhD students and young postdocs! This allowed the experiments to give an update of their results. Both experiments could report in summer 2012, on the 4th of July, that the excess observed in December was further growing and had now reached a significance of 5-sigma: the discovery of an completely new particle! This discovery was an absolute triumph for particle physics as a whole. The theory proposed almost 50 years earlier had turned out to be correct, and despite not being able to predict the mass of the Higgs particle, the experiments could find it and discover it. The scientific papers on the discovery can be found in (CMS Collaboration, 2012; ATLAS Collaboration, 2012).

If we look back to this discovery, what was the probability that it would occur? Once the accelerator demonstrated its capability to deliver the needed high rate of collisions, and once the experiments demonstrated to perform close to the design values, the probability for success was very high. The analyses themselves got more sophisticated and sensitive than originally anticipated. The outcome of the experiment would be that either we discover a Higgs boson, or we exclude it exists, at least in the way predicted by the Standard Model. It would not mean that we could exclude it existed all-together since a non-standard model Higgs particle could still exist and be produced at a much reduced rate or with decays in unusual and unexpected channels. In mid-2011, when most of the Higgs mass range was already scanned for a new particle, and only the region between 115 and 130 GeV was not yet within reach, and we did not see any sign of a Higgs particle yet, the first ideas on how to deal with announcing the exclusion of the Standard Model Higgs were discussed. Since this would be a discovery as well, some scientist believed that we should maintain also here the 5-sigma strict condition, which would take a long time to achieve! It may well be very fortunate that we never had to go to this plan-B. We certainly needed to avoid malicious press headlines like "6000 scientists fail to find the Higgs", which is not at all the same as finding evidence the Higgs does not exist. The latter statement would actually have been sensational in a way, because it would send theorists back to the drawing board and makes us experimentalists hunt for signatures in our detectors for any of the possible alternative scenarios even stronger. In all, the Higgs particle was probably the largest collective discovery made in the modern history of science, involving indeed more than 6000 scientists. It made even the cover page of the magazine The Economist of the July 2012 issue.

5. The future: discoveries of new physics?

The discovery of the Higgs particle has reinforced the notion that new physics should be found at the LHC, so that we can anticipate future discoveries. The value of the Higgs mass of 125 GeV itself is not very

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stable against high energy effects, and unless there is an uncomfortable large fine-tuning of the parameters in Nature, it looks that new physics would be necessary.

This is but one of the reasons why scientists believe that the Standard Model is not the full story. The LHC experiments have been designed for the search for new physics as a major objective. New physics here can mean the direct production of new particles, evidence for new interactions, or indirect signatures in precision measurements of Standard Model processes. But the situation is different from the Higgs hunt though! First and foremost we do not actually know:

– At which energy will we be able to see experimental signatures of new physics?
– How exactly will these signatures look like?

Many of the theories that provide extensions of the Standard Model predict effects in the energy range of the LHC, but we have no guarantee that this will be the case. In that sense there is a much higher risk for a potential discovery. Certainly, in case of a null-result, we can always claim that we are excluding an ever-increasing region of the possible phase space for the masses of new particles or exotic couplings, but it will be never as dramatic as excluding a Standard Model-like Higgs particle. Also the precise nature of the experimental signature itself is not known. While for the Higgs particle the expected production processes and decays were well predicted, for this kind of search we have much fewer constraints. The most likely scenario is that some kind of new heavy particle will be produced at the LHC, thanks to the high beam energy and $E = Mc^2$, but there are also other possibilities. Moreover these new heavy particles are in general expected to be very short lived and to decay basically at the place of production, so that the experiment will see only its decay products, typically the usual standard model particles, and the experiments will need to reconstruct the "mother particle" from the measured debris. But even that is not sure. In many new physics scenarios the new particles can be long-lived on a detector-traversing time scale and in fact may show very unusual signatures in the CMS and ATLAS (and other) experiments.

Hence it appears that the experimenters need to be very open-minded to discover new physics in these conditions. The collisions the experiment selects to record, which is by necessity only a small fraction of the total collisions that are produced. Typically the experiments at the LHC pay great care to this selection procedure during the data-taking since the losses made there are unrecoverable. Next a plethora of different analyses are carried out looking for all possible different signatures of new physics one can think of. Systematically the phase space of new physics is being probed, first with simple and robust searches and, when nothing found, with more refined and sophisticated searches, so to leave no stone unturned. This process has been going on at the LHC for the last 4 years, but no discovery of new physics has been claimed so far.

A special class of new physics scenarios are the "Supersymmetry" ones, or SUSY for short. SUSY is often called "the last symmetry Nature does not seem to have used yet". Indeed it postulates a symmetry between so-called boson and fermion particles. In the Standard Model the fermions are the particles that matter is made of, and bosons are particles that are exchanged in interactions. SUSY then requires that for each fermion we know in the Standard Model there is a supersymmetric heavy boson partner, and vice versa. In other words every particle we know today would have a heavy brother somewhere. This looks a bit artificial but in fact such a theory could deliver us many answers to the questions we have today. For example what keeps the mass of the Higgs stable at 125 GeV, why the top quark is so heavy, a way to unify the three main fundamental interactions at a very high energy scale, and perhaps most intriguing of all: a candidate for a dark matter particle. Indeed, if supersymmetry would come with a preserved quantum number, one could only pair-produce supersymmetric particles at the LHC, but each of these particles would decay in a possibly long decay chain and at the end of that chain one particle would be left that cannot shake off its "SUSY quantum number" and is doomed to live forever. Such a particle would be typically about a 100 times heavier than the proton and a have all the right properties for a dark matter particle.

Due to the reasons given above SUSY is one of the prime candidates for what we may find beyond the Standard model at the LHC. Much of the expert literature discusses possible manifestations of SUSY in the experiments. Its connection with dark matter, a phenomenon that does not fit in the present Standard Model is of course a big motivation and prerogative. The fact that new experimental evidence for dark matter is mounting (structure formation, gravitational lensing,…), even though we have not been able to detect it directly yet, is a strong asset. In fact the LHC analyses are now even preparing for 'generic' dark matter searches. Indeed SUSY may be a strong candidate for being able to explain the dark matter in our Universe, but it is by no means the only possible scenario that can lead to dark matter, so a generic search can be more rewarding.

SUSY and other new physics scenarios have been searched for at the LHC with the data collected in 2010–2012. No discovery was made so far. Does that reduce the probability for a discovery in the near future? In fact the accelerator started operating again in 2015 and will for the first time reach a close to the design centre of mass energy, namely 13 TeV and deliver an even higher collision rate compared to before. This is extremely important. Higher energy is important to allow for higher masses to be produced, so this will lead to a significantly higher reach for new physics compared to the first run of the LHC. Higher collision rate is important as we expect these new heavy particles to be produced at relatively low rates. More collisions lead to a higher probability to discover such new particles.

Can one predict a probability for the discovery of SUSY at the LHC? In fact the author has been involved in studies over the past year, conducted by a team of experimentalists and theorists, to predict where SUSY could be found i.e. what would be the signatures for that. Perhaps surprisingly for an outsider but indeed we can make accurate evaluations of the likeliness for finding SUSY thanks to our understanding on the dynamics of fundamental particle interactions, the analysis of the characteristics of the theory, the capabilities of the experiments and sophisticated statistical tools. The understanding of the fundamental interactions allows us to look at a bigger picture where we include relevant precision measurements, potentially sensitive to SUSY, made over the last years at various other experiments, including e.g. the amount of dark matter that we believe there to be in the Universe. Other observables include rare decays, the anomalous magnetic moment of the muon and so on (Buchmuller et al., 2014). The most recent constraints come from the LHC experiments, with the detection of a Higgs particle with a mass at 125 GeV, and from – the so far unsuccessful- searches for signatures for SUSY, which lead to excluded regions in this SUSY parameter phase space. The new physics theory is defined already to such a level of detail that it allows us to define a phase space in the new physics parameters that we can probe. Statistical tools are deployed based on either a frequentist or Bayesian paradigm to analyse the probabilities. The parameter phase space is 5-dimensional for the most constrained hypotheses for the underlying SUSY model, but can be as large as 19-dimensional for more relaxed scenarios. Hence Markov Chain techniques and more recently Multi-Nest techniques are deployed to efficiently sample the SUSY parameter space. When using the frequentist approach one calculates at each probed SUSY phase space point a global χ^2 function of the predicted versus measured observables, which gives the probability that such a point is allowed/excluded by the existing data and search limit results. To have a good coverage of the phase space one typically needs to generate of the order of a billion SUSY points to get stable results, so this is a very computing intensive study. The results are however extremely interesting. First they do show that we can get very good fits, i.e. find regions where the present measurements of precision observables and all

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boundary conditions of the dark matter abundance and the search limits at the LHC can be reconciled and SUSY particles could live. Studies of this kind show the preferred mass range for the SUSY particles would be of the order of 3 to 4 TeV, a region that we can cover with the LHC operating at its design luminosity, provided we get enough collisions.

Certainly as new measurements and limits are becoming available, they will be used to update the predictions and probabilities, but the "chances for a (SUSY) discovery have never been better" and hence the scientists will analyse the upcoming data with vigour. If we are lucky then SUSY could be just outside the past reach of the LHC and a new major discovery will follow in the next few years. Discovering SUSY or whatever new physics that will reveal itself in the LHC data, will be even a much bigger deal than the Higgs discovery.

Finally, we note that experimenters are often guided by theorists, to search for certain signatures for new physics. This brings up an important point whether this is not too restricted and we can risk missing a discovery, as perhaps Nature has chosen a road that has not been thought of yet. This is indeed not completely excluded, and for that reason the experimenters also conduct more generic searches, simply making a systematic scan of many possible signatures. Such analyses are never as sensitive as the dedicated search analyses for specific signatures but would indicate if some significant deviation from background expectations starts to show up, and then a more dedicated analysis could be carried out on that signature. However, for the LHC run so far, no significant deviations have been found so far.

6. Summary and outlook

So what is the probability for a discovery at the LHC in future? Clearly where we feel on safe ground theory-wise, a discovery can be guaranteed, especially if also a null result is as important as to find the new particle. This was the case for the Higgs particle. Hence the investment for the science facility here was a safe bet and a return guaranteed, in economic terms.

For the physics beyond the Standard Model, which is desperately needed in order to understand more on the dynamics of early Universe, and to answer questions like "why are we here?", we have at this time less strict guidance from theory. We do have evidence that there is still much to learn from the presence for dark matter, but we can presently not be sure that this is a particle or if it is relevant for the physics program at the LHC. But most scientists would agree that at the LHC the scenario of dark matter/SUSY has the largest probability for a discovery.

What if we do not see any hint for new physics with all the data of the LHC collected, in 2030–2035? Is this the end of the road? If we do not have any new physics or discoveries, is the LHC still a success? Yes I believe it would be. There is of course the Higgs discovery already in the bag plus the strong gain in knowledge on measurements of the strong and electro-weak force, which is invaluable.

To what can these discoveries at the LHC contribute for humanity in general? If we look back at the beginning of the last century, we are still benefiting from the discoveries of those days that contribute to improve our technologies. One can remember what Michael Faraday, one of the pioneers on electricity in the 19th century allegedly said to William Gladstone, at the time British Chancellor of the Exchequer (minister of finance), when asked of the practical value of electricity: "Why, sir, there is every probability that you will soon be able to tax it."

We do not know today what practical applications could be based on the knowledge gained from the Higgs or from the new physics that is awaiting us. We do know for sure that a lot of the technology developments needed for the design and construction of the experiments, is reused for improving sometimes by a factor 10 or more the performance of detectors in the medical sector. Also accelerators themselves are now more and more used for medical treatment and other areas: there are more than 25,000 accelerators in use worldwide and less than 0.5% of these are used for physics research. Most accelerators are in use for radiotherapy and ion implantation. The benefits to society that came from these projects are not simple to calculate.

In all, more discoveries at the LHC are very likely, but not fully guaranteed. With curiosity-driven research we want exactly to find out what is next in the understanding of how Nature works. Maybe we will learn that even larger and more powerful instruments will be needed to unlock its mysteries. Given the timescales involved and anticipating the technical challenges that will have to be faced, that planning is already underway today.

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Prof. Dr. Albert De Roeck is a senior research scientist and staff member of the largest particle physics laboratory in the world, CERN, located near Geneva, Switzerland. CERN is the home of the new particle accelerator: the Large Hadron Collider (LHC). De Roeck is also a professor at the University of Antwerp (Belgium) and a visiting professor at the Institute of Particle Physics and Phenomenology in Durham (UK), University of California Davis and the British University in Cairo (Egypt). He obtained his PhD at the university of Antwerp on an experiment at CERN, studying the multi-particle dynamics in hadron–hadron interactions, by colliding meson beams on protons and nuclear targets. After his PhD, De Roeck spent 10 years at the German particle physics laboratory, DESY, where he and his team made very precise measurements of the quark and gluon structure of the proton, and performed precise tests of the strong force. At the end of the 90s his interest turned to the possibility to discover new physics at future particle colliders, in particular Supersymmetry and Extra Dimensions, and he returned to CERN. He first joined an experiment at the large electron-positron collider LEP, studying the strong force and searching for signals of new physics. During the last ten years, he played a significant role in the preparation of one of the experiments at the LHC: the Compact Muon Solenoid (CMS). De Roeck is now one the leaders in the CMS/LHC physics program and actively involved in physics analyses. De Roeck was also the deputy spokesperson of the experiment in 2010 and 2011, was the convener of the Higgs search physics group in 2013 and 2013, and had a leading role in the discovery of that particle in July 2012 in the CMS experiment.

He regularly gives seminars and lectures all over the world on the physics potential and results of the LHC project, and appears regularly in Television or Radio programs. His h-index at present is 129. In 2010 Prof. De Roeck got a doctor honoris causa degree from the University of Helsinki, Finland. Co-author of more than 1200 scientific papers, as an experimentalist he also has also been collaborating closely with leading theorists of the field.

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