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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