BIG SCIENCE AND THE LHC

Gian Francesco Giudice

CERN, Theory Division, Geneva, Switzerland

The Large Hadron Collider (LHC) [1], the particle accelerator operating at the European laboratory CERN near Geneva, has already achieved remarkable results. On 30 March 2010 for the first time proton beams were smashed at the record high energy of 3.5 Tera-electronvolts (just as if each proton were accelerated by a voltage of 3500 billion Volts). Successes have followed each other at an impressive rate, far beyond even the most optimistic expectations. By the end of October 2010, the total production of proton crashes (the "integrated luminosity", in technical jargon) was almost 50 inverse picobarns, the equivalent of about 5,000 billion proton collisions. The conversion of the accelerator to the phase in which the colliding beams were made of lead ions, instead of protons, was smooth and fast. This allowed a four-week period of accumulation of data, which has provided us with new information on the behavior of matter at high density. A new phase of proton collisions at high intensity started in March 2011. Already on 22 April 2011 the LHC set the new record for proton beam intensity (previously held by the Tevatron, the collider operating at Fermilab, the laboratory near Chicago) with 4.6×10^{32} cm^{-2}s^{-1} (the equivalent of about 50 million proton collisions per second). A few weeks later this value was almost doubled. The LHC detectors have performed stunningly, recording with staggering precision and efficiency the mountain of data coming from the collisions. At the moment this article is being written, we have already entered the phase of direct exploration of phenomena never studied before in previous experiments. There is every indication that discoveries are imminent.

Awaiting these new physics results, I would like to address a question that, although foreign to the immediate research goals of the LHC, necessarily concerns every large scientific project that requires enormous financial, technological, and intellectual investments – the so-called phenomenon of *Big Science* [2]. The magnitude, the complexity, and the profundity of aims of the LHC project arouse admiration and awe in the majority of people who learn about it. Nevertheless, doubts, misgivings, and even fear sometimes surface both outside and inside the scientific community regarding anything having to do with Big Science. So the question is: should society support large research projects in basic science?

The emergence of Big Science

The Manhattan Project is often considered the event that started Big Science, establishing a new and tighter relationship between science and society, and creating a new methodology in scientific investigation. Without entering into moral considerations about its goals, we must admit that the Manhattan Project defined the *modus operandi* that has become a trademark of Big Science. This *modus operandi* works as follows. A large number of scientists is involved in a project whose target is well defined, although it requires crossing the limits of known science and technology. Large funds are made available to the project, but the goal has to be reached within an established time. The scientists must adapt to work in interdisciplinary groups which, in the case of the Manhattan Project, mixed theoretical and experimental physicists with engineers and mathematicians. Moreover, the project is put under the direct control of administrative bodies external to the academic environment.

In reality the Manhattan Project was only an episode that accelerated an already inevitable evolutionary process. Well before the war period, swift scientific and technological progress had propelled science beyond its natural academic borders. On one hand, science was having a crucial impact on society; on the other, science was requiring financial resources that could be found only outside the limited world of universities and research institutions.

The construction of ever more advanced and costly instruments was becoming the decisive factor for progress in many scientific fields. One example is stellar astronomy, which was always in need of more powerful optical telescopes. This race towards cutting-edge instrumentation led to the completion in 1917 of the legendary 100-inch Hooker telescope at Mount Wilson Observatory. With this instrument Edwin Hubble discovered that the Andromeda nebula is much more distant from us than the size of the Milky Way, thus proving that our galaxy is only one out of a multitude of galaxies that dot the night sky. This discovery changed forever our vision of the universe by greatly widening its horizons. With that same instrument Hubble made his famous observations on galaxy recession, thereby demonstrating that our universe is undergoing continuous expansion. Without the planning that led to the 100-inch Hooker telescope, these revolutionary discoveries would not have been possible. Although astronomy was employing exceptionally powerful and expensive instruments, the observations with optical telescopes, however, involved only small groups of scientists and did not yet have the typical characteristics that one usually identifies with Big Science projects. This would later change with the coming of radioastronomy.

The race towards low temperatures, since its inception, required always more advanced and complex instruments. The main protagonists of this race were James Dewar of the University of Cambridge and Heike Kamerlingh Onnes of the University of Leiden. Onnes, a great experimentalist with a decidedly pragmatic inclination, organized his laboratory almost like a factory (jokingly called "The Brewery") and even obtained funding and contributions from the refrigeration industry. These organizational abilities contributed much to his success. On 10 July 1908 he liquefied helium, the last element that was still known only in the gaseous state. In order to produce a small glass (6 cl) of liquid helium he had to reach the record temperature of −269 degrees Celsius, about 4 degrees above absolute zero. This result paved the way to the later discovery of superfluidity, although not made by Onnes. However, in 1911 Onnes, using liquid helium to cool mercury, discovered the astounding phenomenon of superconductivity, according to which some materials completely lose their electrical resistivity below a well-defined critical temperature.

Incidentally, it is worth noting that the physical phenomena associated with superconductivity and superfluidity are crucial for the functioning of the LHC. Inside the underground tunnel of the LHC, 1200 tons of superconducting cables transport the exceptionally intense electric currents (up to

12,800 amperes) that generate the magnetic fields used to guide the proton beams. Dipole magnets are aligned along the 27-km tunnel and superfluid helium is employed to maintain them at the temperature of -271 °C (1.9 degrees above absolute zero). Without knowledge of superconductivity and superfluidity, the LHC would not even be imaginable.

The race towards smaller distances followed a similar path. The newborn science called "atomic physics" needed ever more expensive equipment and, in particular, the rising cost of radium determined which universities and laboratories could afford to do research on atomic and nuclear structure. Radium was the most commonly used element as a source of α radiation, the necessary probe for penetrating inside atoms. But at the beginning of the last century the cost of radium reached 160,000 dollars (at that time) per gram, being by far the most precious substance in the world.

In the wake of continuously rising costs, the importance of the managerial capabilities of scientists also grew. At the beginning, especially in Anglo-Saxon countries, necessary funds were found among industrialists, philanthropists, and other science benefactors. When it eventually became necessary to turn to the public sector as well, the first need encountered by scientists was communicating their results and scientific activity to the general public outside the academic world. Even in a society with no television or web, the diffusion of scientific ideas was not such a difficult problem. Judging from how the big names of relativity and quantum mechanics were greeted when they were traveling and giving public lectures, it seems clear that scientific themes were highly popular. Einstein was an undisputed cultural icon. Even the less dazzling Paul Dirac was able to attract crowds. When he gave a public lecture on a cricket field in Baroda, India, thousands of people came and it was necessary to use a cinema screen for the audience who did not find space inside the stadium [3]. The second, and much harder, problem was to find support among politicians and administrators. In the field of nuclear and subnuclear research, Ernest Rutherford in Great Britain and, later, Ernest Orlando Lawrence in the United States excelled in their ability to draw both public and private funds for research.

World War I established yet another link between science and government authorities, because science participated rather directly in war activities. Chemistry was at the forefront in designing and producing chemical weapons. In August 1914, the French army employed for the first time tear gas, while at the battle of Ypres in April 1915 the Germans started the use of poisonous gases made of chlorine, phosgene, and yperite (named after the Belgian city of Ypres, but commonly known as mustard gas). At first the Allied forces reacted by condemning the action, but then started to develop their own research programs in chemical weapons, which were used for the first time towards the end of 1915. It is estimated that chemical warfare caused serious injuries, often permanent, to more than a million soldiers on both sides, killing 90,000 of them (out of which 56,000 were Russian).

Physics participated in the war with wireless communication, which allowed for a new organization of military field actions, and with instruments for detecting submarines by means of acoustic techniques, the precursors of sonar. Even innocent mathematics was not immune to engaging in warfare, for cryptography became, in the hands of the military, another weapon as well.

Many scientists participated in war committees and found themselves sitting around the same table with military officers, politicians, and industrialists. The same situation came up again, even more tragically, in World War II. Science was involved in two major projects. The first was the development of radar, in which the United States invested 3 billion dollars (at that time). The second one, costing about 2 billion dollars, was a dreadful scientific challenge, fueled by fear that the Nazis would be the first to achieve it: the Manhattan Project [4]. This project represented a decisive step in the evolutionary process of Big Science because it introduced a special methodology, rather unusual for the traditional standards of scientific research at that time.

At the end of the hostilities, the United States awakened from the nightmare of war with an unshakable faith in science. Physicists, seen as the essential developers of military supremacy, enjoyed special consideration and particle physics became one of the main beneficiaries, for it was regarded as the natural heir of the science that led to the Manhattan Project. The cold war helped cement this privileged status. But the sympathy of military circles to particle physics was an ethically uncomfortable legacy, though a profitable one from the practical point of view. The physicists who during the war had become "better scientists if impurer men"[5] looked for a way of redemption by working on peaceful applications of nuclear energy or on basic research in the exploration of nature's secrets at the subnuclear level.

The post-war favorable wave not only affected particle physics, but impinged upon the whole of science. The United States witnessed a true explosion of publicly-funded scientific projects. Many US economists, influenced also by the theories of Joseph Schumpeter, saw in scientific research and in continuous technological innovation the key for constant economic growth. As economic growth brings employment and prosperity, it also implies a solution to the social problems of the poorer classes and a preemptive cure to possible political instability. Basic science and scientific research were essential links of this logical chain.

The opinion that scientific research was essential to ensure economic growth and employment was vigorously asserted in the influential report to President Truman by Vannevar Bush, completed on 14 July 1945 and entitled *Science, the Endless Frontier* [6]: "The simplest and most effective way in which Government can strengthen industrial research is to support basic research and to develop scientific talent." It is worth emphasizing how Bush identified in *basic* research the decisive factor on the way towards progress, claiming that technology is an inescapable consequence of leading-edge science. The goal of government is then to support and nurture the most advanced research institutions without paying much attention to the aspects of technological innovation. According to Bush, basic research is "the pacemaker of technological progress". In the same document Bush proposed the creation of what later became, in 1950, the *National Science Foundation*.

An analogous point of view [7] was reasserted in the report presented on 20 August 1947 by the committee chaired by the economist John R. Steelman: "Only through research and more research in the basic sciences can we provide the basis for an expanding economy, and continued high levels of employment."[8] The President's reply finally came on 13 September 1948, when Truman announced the main points of his program for scientific development: “First, we should double our total public and private allocations of funds to the sciences. [...] Second, greater emphasis should be placed on basic research and on medical research. Third, a *National Science Foundation* should be established. Fourth, more aid should be granted to the universities, both for student scholarships and for research facilities. Fifth, the work of the research agencies of the federal government should be better financed and coordinated.”[9]

In this period of phenomenal expansion of American scientific research, an event galvanized public attention and undermined the US administration’s conviction of possessing complete technological hegemony. On 12 April 1961, Yuri Alekseyevich Gagarin became the first man to journey into space. The United States reacted immediately. On 25 May 1961 President Kennedy spoke to Congress, addressing the nation with the famous words: “I believe that this nation should commit itself to achieving the goal, before this decade is out, of landing a man on the Moon and returning him safely to Earth.” Public opinion was firmly in favor of undertaking the venture. Congress, showing no hesitation, almost unanimously approved the colossal project, estimated between 20 and 40 billion dollars. Without entering into debate about the scientific value of the project, we find in the Apollo missions the same characteristic elements of the *modus operandi* of Big Science, though in a context very different from the Manhattan Project.

The so-called “missile gap” – that which was perceived as the potential technological lag of the United States with respect to the Soviet Union – had to be closed as soon as possible. Remedies were looked for not only in the space race but also in basic research and basic education, for example by strengthening school curricula in scientific subjects, especially mathematics.

In this climate of euphoria for large publicly-funded scientific projects, some expressions of doubt started to emerge not only from society, but also from within the scientific community. Among the most authoritative voices were the physicists Merle Tuve, Alvin Weinberg, Philip Anderson and the astrophysicist Fred Hoyle. At the time, Alvin Weinberg was a well-known and influential personality, having been nominated director of Oak Ridge National Laboratory in 1955, the laboratory which supplied the enriched uranium for the Manhattan Project [10]. In 1961 he published an influential essay [11] on the impact of large scientific projects and it was on this occasion that he coined the term “Big Science”. Weinberg wondered whether Big Science was ruining science, identifying some issues that are still worth discussing today. “In the first place, since Big Science needs great public support it thrives on publicity. The inevitable result is the injection of a journalistic flavor into Big Science which is fundamentally in conflict with the scientific method. [...] The spectacular rather than the perceptive becomes the scientific standard.” Weinberg was referring then to the space program; today unfortunately his words also bring to mind certain kinds of information about the LHC at times promoted by CERN.

The enormous size of Big Science projects requires constant oversight by the administrative bodies and Weinberg saw in this an abandonment of the true scientific motives: “Unfortunately, science dominated by administrators is science understood by administrators, and such science quickly becomes attenuated if not meaningless.” The true risk is an excessive bureaucratization of the large scientific projects. The public authorities, which have the fair duty of monitoring the expenses incurred by the project, can end up imposing decisions based on purely financial considerations, neglecting technical and scientific aspects. The administrative bodies, accustomed to operating quite differently from the scientific sector, can even involuntarily destroy the special vitality that thrives in a research environment. More than thirty years later, Wolfgang ("Pief") Panofsky, the exuberant physicist who long served as director of the SLAC laboratory at Stanford, would identify in bureaucratization the main cause that led to the closing of the SSC, the powerful proton accelerator planned in the United States: "The sheer size of the undertaking, the micromanagement by DOE [the Department of Energy, the government body in charge of the SSC], and the intensity and frequency of external oversight all led to a bureaucratic internal culture at the laboratory. In the name of cost control, technically needed changes and design trade-offs were discouraged. Decisions on technical alternatives were distorted by 'political acceptability' and were at times made late or not at all. [...] Key scientific and technical people were generally placed low in the decision chain."[12]

In his article on Big Science Weinberg performed a calculation that today makes us smile (or should it instead make us frown?). Extrapolating the rate of growth of the expenses for scientific research since the end of the war to 1961, he reached the conclusion that in about twenty years science would financially ruin the United States. The danger has certainly been averted, as one can easily infer from the present situation of public investments in research. However, Weinberg's worry gives us a measure of the exceptional leap towards research undertaken by the United States during the post-war period.

The social transformations and the ideological movements that started in the 1960's and 1970's were accompanied by a disillusionment towards science. Within society grew the awareness that technology brings not only progress, but can also lead to environmental damage and social injustice. Herbert Marcuse, the philosopher who much influenced the generation of the 1968 protests, maintained that science, by its own nature, induces an inhuman way of thought and that technology is an engine of oppression. In his ideas we encounter a typical limitation of thought at the time, namely the inability to distinguish clearly between science and technology, which are identified and indissolubly associated with war.

In the meantime, the Vietnam war, besides fueling popular discontent, revealed the limits of advanced US military technology. The US army, in spite of its sophisticated weaponry, was kept in check by the poorly equipped but resolute North Vietnamese army. Moreover, large public spending began to weigh upon the internal budgets of many Western countries. The capability and will to sustain large scientific projects started to dwindle.

The fall of the Berlin Wall in November 1989 and the consequent dissolution of the Soviet Union in 1991 dissipated the specter of the cold war and, with it too, the motivations of national prestige that have influenced some political factions to support large scientific projects. In October 1993 the US Congress decided to cancel the SSC project, the accelerator that would have collided protons with energy almost three times larger than the LHC. The project had been approved more than six years earlier and was currently in the construction phase, having already spent almost 2 billion dollars.

Many are the reasons that led to the unfortunate closing of the SSC [13], but here I want to mention only one issue which, although not necessarily the most important, is particularly relevant for this discussion. The project was approved during the Reagan administration, in an epoch of revival of public spending, in which however national defense concerns predominated over scientific motivations. This is the period of the Strategic Defense Initiative (the so-called "Star Wars" project), estimated at the time at around 60 billion dollars, and of the Freedom Space Station. The cancellation of the SSC was voted instead under the Clinton administration, after the end of the cold war and, more importantly, in a period in which Congress was firmly determined to reduce the growing US public deficit. It is worth noting that only two days before voting against the SSC, Congress had expressed support, although with a majority of a single vote, for the continuation of the International Space Station (a synthesis of the Freedom Station with similar projects initiated by Russian, European, and Japanese space agencies). This happened in spite of the fact that the Space Station was estimated to cost more than three times the LHC, that the cost was continuously rising, and that the scientific motivations for the construction of the Space Station were rather weak. The international element and the previous agreements with foreign countries certainly worked in favor of the Space Station.

The cancellation of the SSC was a traumatic event for the particle physics community around the world. It marked the end of an era, but not the end of large projects in basic science. It certainly represented an important step in the evolutionary process of Big Science, because it highlighted the need for new characteristics in large scientific projects. A broad international collaboration and a view beyond the interests of a single country proved to be essential elements for the success of such projects. The LHC, built by a consortium of European member states of CERN with a substantial contribution from almost all the main countries in the world, has superbly achieved this vision.

Is Big Science real science?

The charm of science is usually associated with the image of the brilliant idea, born in the silence of a sleepless night and elaborated with mathematical calculations on a simple notebook: an individual's creation that ends up revolutionizing the foundations of human understanding of nature. Or else we imagine a scientist who, in the solitude of a laboratory, designs and performs an extraordinary experiment, discovering new and completely unexpected phenomena. At first sight, Big Science seems the precise antithesis of all this.

This contrast does not necessarily correspond to reality. As shown previously, the natural development of a leading-edge scientific field leads inevitably to the need of large undertakings and ambitious projects. Even fields traditionally regarded as Small Science (such as molecular biology or climate science) have recently required programs with typical Big Science features (such as the Human Genome Project or supercomputing for studying climate changes). These large scale projects are not necessarily the opposite of the more poetic and traditional view of science, but are rather its natural completion and enrichment. The two methods of investigation are not in contrast, because both share the same scientific ethics and the same ultimate goals. Both methods are necessary for science to advance beyond the limits of knowledge. It is like comparing a painting by a Renaissance master with the epic construction of a Gothic cathedral. The advancement of art needs both.

Whether we like it or not, Big Science is an irreplaceable instrument for modern science. Wherever science makes progress, sooner or later there will be the need for large and expensive instruments, for goal-driven organized undertakings, for tight collaboration with scientists of different disciplines. The duty of scientists and of science funding agencies is to employ wisely the special instrument of Big Science for projects of indisputable scientific excellence, free from motivations of national prestige or propaganda, and devoid of any military interest.

A common criticism against Big Science is the claim that it transforms research from a method of scientific investigation into an industrial process that stultifies creativity. In reality Big Science is only a technical necessity and not a dismantlement of the traditional goals, values, and motivations of science. The methods of investigation have changed, but not the principles that drive scientists. Enrico Fermi offers an excellent example. During his life, the great Italian physicist experienced all the various ways of doing science: the pensive and individualistic style of theoretical physics (with the statistics of half-integer spin particles and with the theory of β decay), the spontaneity and enthusiasm of Small Science methods (with the experiments on slow neutrons carried out by the *Via Panisperna*'s Boys in the goldfish pond of the Physics Department garden), the goal-driven and organized structure of Big Science (with the Chicago Pile and with the Manhattan Project). A Big Science project thrives on individual creativity too, and the LHC gives ample evidence for it.

Another criticism originates from the conflict between two epistemologically different positions which, depending upon the context, have been called "intensive research" and "extensive research" [14], "reductionism" and "constructionism" [15], or "fundamentalist" and "generalist physics" [16]. It is an empirical fact – and not a philosophical assertion – that nature shows an ordering, at least up to the distances explored until now. Simpler elements emerge at smaller distances. Furthermore, the physical laws that govern the simplest elements reveal fundamental and universal properties. These physical laws allow us not only to understand the particle world, but also to describe the large-scale structure of the universe and to reconstruct its history since its very first instants. Reductionism aims at discovering these laws and it is driven by the curiosity of human intellect for comprehending the ultimate workings of nature.

Knowledge of the fundamental physical laws is often not sufficient for describing, from a practical and quantitative point of view, the complexity of many natural phenomena. In other words, knowledge of the equation does not imply the capability of deriving the solution suitable for describing the phenomenon. The mathematical description of the emergent properties of a complex system requires physical laws that are completely different from those of the fundamental theory. Here enters constructionism, which aims at discovering the emerging laws.

Both programs, the reductionist and the constructionist, have their scientific validity and their intellectual interest. The mere existence of these different approaches demonstrates the richness and variety of the scientific panorama. It would be dangerous to claim that all scientific research should follow a single path.

The distinction between research fields in reductionism (high energy physics, cosmology) and constructionism (such as solid state physics, astronomy, molecular biology) no longer corresponds to the separation between Big Science and Small Science since both sectors have developed their own large projects. Moreover, the distinction between reductionism and constructionism seems linked to the image of a certain field at a particular historical moment, without corresponding to a real difference in the basic motivations of the scientists active in research. For instance, nuclear physics was considered in the past a reductionist science, but is thus no longer; inside astronomy a constructionist soul coexists with the reductionist activity of observational cosmology. All this would seem to indicate that this distinction is more a subject for science historians than for the scientists themselves.

This problem of semantics acquires a less academic flavor when different research sectors compete with different projects for public funding. A commonly expressed fear is that Big Science projects could absorb all available resources, suffocating the activities of smaller and less organized sectors. In principle this is a valid worry because diversification of research is vital for scientific development. In practice, however, science public funding is never a simple zero-sum game. The decision-making mechanisms are more complicated and the approval of large projects is not necessarily in contradiction with a robust program of scientific diversification. As a matter of fact, large and small projects have always risen and fallen together in the past. For example, there is no evidence that, after the SSC was cancelled, the scientific fields that openly opposed the continuation of the project enjoyed any financial benefit.

If a distinction has to be made, it is better to make it between those projects and directions of research that drive real advancement in knowledge and those that lead to blind alleys or propose repetitive experiments of little scientific value. Science needs different investigational methodologies to create the opportunities favorable for making progress. "There is no illusion more dangerous than the belief that the progress of science is predictable. If you look for nature's secrets in only one direction, you are likely to miss the most important secrets, those which you did not have enough imagination to predict."[17] So Freeman Dyson concluded one of his anti-SSC sermons. But his perceptive words do not necessarily undermine the reasons for big scientific enterprises. Even an adamant Small Science advocator and Big Science critic such as Dyson concurs that the extraordinary progress in astronomy and particle physics of the last sixty years was made possible only by a wise admixture of large and small projects. There were triumphs and unexpected discoveries, as well as failures and errors, in both Big and Small Science, but the final success could never have been achieved without the existence of both large and small projects. The stability of an ecosystem needs animals of different sizes. But the size of an animal species does not establish its aptness for survival, which is instead determined by its interrelations with bigger and smaller creatures. So it happens in science: there can be no long-term growth in a system where the large projects absorb the totality of resources, nor where there is prejudicial objection against large projects.

Are large basic-science projects too expensive or even useless?

Addressing the House in 1992 during a discussion of the motivations of the SSC, the Republican New York Representative Sherry Boehlert, a tenacious opponent of the project, asserted: "The SSC will not cure cancer, will not provide a solution to the problem of male pattern baldness, and will not guarantee a World Series victory for the Chicago Cubs."[18] I cannot disagree with him. But to address the question on whether society should embark upon large basic-science projects it may be more appropriate to consider other arguments.

Let us first analyze one aspect of the issue of costs, related to the management of large projects by non-scientist administrators, a point already raised by Weinberg in his 1961 article. An inflexibility of the budget that fails to allow for studying alternatives and for dealing with contingencies is a dangerous policy. Equally dangerous is rigidity in maintaining the initial project design, in spite of new technological or scientific developments. Such policies, for the sake of cost control, can turn into far greater financial losses or even into scientific failure. According to Dyson, the setback of the Shuttle Program was largely caused by the problem of premature choice imposed upon the NASA engineers. "Premature choice means betting all your money on one horse before you have found out whether she is lame. Politicians and administrators responsible for large projects are often obsessed with avoiding waste. To avoid waste they find it reasonable to choose one design as soon as possible and shut down the support of alternatives. So it was with the shuttle. [...] The evolution of science and technology is a Darwinian process of the survival of the fittest. In science and technology, as in biological evolution, waste is the secret of efficiency. Without waste you cannot find out which horse is the fittest. This is a hard lesson for politicians and administrators to learn."[17]

In order for one to form an opinion on the choices that society must face regarding big scientific enterprises, it is useful to review their costs. Table 1 contains a summary of the costs of some large projects. The data should be interpreted with great caution because the way costs are estimated varies enormously from project to project and, in some cases, a single program shares so many different aspects that it is virtually impossible to quantify reliably the expenses.

Note that the expenses for the Manhattan Project (which amount to 0.6% of the US military expenses during World War Two) were mostly due to the plants for uranium separation and plutonium production at Oak Ridge and Hanford. The activity in the laboratory at Los Alamos, where the physicists were gathered, cost only 4% of the total.

The LHC costs, according to the CERN budget, are summarized in table 2. It should be mentioned that only manpower directly employed by the LHC is counted under the heading "Personnel". However, a large fraction of CERN personnel works in practice for the LHC. Moreover, the data in table 2 do not include operation costs nor the contributions to the construction and functioning of the particle detectors from universities and laboratories outside CERN. For example, the material costs of the largest detector (ATLAS) were 540 million Swiss francs, and CERN contributes to the various detectors an amount that varies between 14% and 20%.

Just for comparison, the LHC costs roughly as much as a large project in public civil engineering. For example, the 8-km long Øresund bridge, completed in 2000 to connect Denmark to Sweden, cost about 4 billion euro. The 40-km long bridge over the Strait of Messina, planned to connect Sicily to the rest of Italy, is today valued around 6 billion euro, but presumably the cost will grow. The cost estimates for the 2012 Olympic Games in London have already passed 10 billion euro.

It is difficult to set the right price for the value of knowledge, for the cultural impact of scientific discoveries, for the desire to understand the organizing principles of nature and to decipher the universe. It is easier, however, to identify a reciprocal causal link between progress of knowledge and economic, social, and industrial development. One strengthens the other in a symbiotic relationship, as happened in Great Britain at the end of the nineteenth century, in Germany at the beginning of the twentieth century, and in the United States in the post-war period. Advancements in basic science rarely have immediate effects on technology, but their value for society appreciates with time. Today's technological sectors were subjects of basic research in the past.

The only legitimate yardstick for measuring the importance of a basic science project is its impact on science itself. Economic and technological relevance does not always lead to the best science choices and thus it does not always translate into a better investment for society. Nevertheless, the enormous costs of large scientific projects justify accurate analyses of possible economic or technological spin-offs on the part of the funding agencies. These evaluations depend on the specific case, but Big Science projects present some common features that often translate into special opportunities. Quite independently of the scientific goals of the project, which are its true and only *raison d'être*, I would like to highlight some general aspects of Big Science that can have beneficial effects on society. Needless to say, one could make an equally long list of arguments supporting the social advantages of the Small Science approach.

1) The large concentration of not only financial, but also especially intellectual, resources in the same research center creates a situation that can hardly be achieved in traditional academic institutions. This yields very fertile ground, naturally open towards innovation, also beyond the planned objectives of the project. For example, it is not by chance that the web was born at CERN, even if its creation was not one of the direct goals of the laboratory.

2) From the point of view of applications, the fruit of basic research is usually slow to ripen. This lag between scientific discovery and its technological spinoff is naturally filled by the methodology of Big Science. This is because, while applications of the ultimate goal of the project are nearly impossible to foresee, the real technological relevance of large projects lies in the research developed to accomplish them. The LHC provides an excellent example. Nobody can say with certainty today if and how the discovery of the Higgs boson or of any other exotic particle could constitute the seed for some practical application. But the research that led to the construction of the LHC has already translated into many useful spinoffs: accelerator development has produced hadrotherapy for treating cancer and synchrotron light with its many uses as an "X-ray microscope"; particle detector development has produced various medical diagnostic techniques and real-time analyses; information development has produced the web and the GRID.

3) The need for advanced technologies and the consequent close relationship with private companies offer benefits to the industrial sector that go beyond the simple profits assessed in terms of

contracts. The scientists' request for cutting-edge prototypes pushes industry towards new manufacturing techniques, whose development would be too risky in a mere market environment.

4) Basic research, because of its universal character free from economical or military interests, is particularly suited for international collaboration and the large projects are the best vehicles for it. Such projects offer the opportunity for participation in great scientific challenges by countries which, by themselves, would not have adequate resources. Moreover, large scientific ventures can strengthen peaceful international ties and even start collaborations between hostile nations, creating opportunities for political rapprochement. In the climate of the cold war, Alvin Weinberg, in spite of his aversion towards Big Science, understood this special role of large scientific projects: "If high-energy physics could be made a vehicle for international cooperation [...] between East and West [...] the expense of high-energy physics would become a virtue."[27] A laudable present-day example comes from SESAME (Synchrotron light for Experimental Science and Applications in the Middle East), a project for research with synchrotron light based in Jordan and operated by a scientific collaboration of Israel, Iran, and other Middle-East countries including the Palestinian Authority.

5) Large scientific projects provide a unique education and training opportunity for students and young researchers. For example, young people play an instrumental role in the LHC project. About half of the physicists involved in the ATLAS experiment are younger than 35 (and almost a third is younger than 30). These young people learn to tackle complicated problems, to master advanced technologies, to work in interdisciplinary teams. Not all of these young people will remain in the field of scientific research, but they will carry their unique skills and experience into other sectors of society. Investments in large scientific projects are also investments in future generations of capable and competent individuals.

6) Large projects are often irreplaceable tools for the advancement of basic science. Giving up this tool means giving up a piece of knowledge. But knowledge has a value that overcomes the boundaries of science, affecting all society. There is an intrinsic value to knowledge, linked to our awareness of the meaning of nature and of the role we play in the physical universe. This awareness influences our way of thinking and of acting as individuals and as a community, and thereby contributes to the intellectual growth of society. In this sense the value of basic science is not unlike that of any human artistic activity. The large scientific projects, easily capturing the public imagination, can transfer scientific knowledge in a particularly effective way and spread it through all layers of society.

Every civilization, every historical epoch leaves a legacy to future generations. I believe that the legacy of our society of the last hundred years will be found in the revolutionary scientific discoveries and in the swift technological progress. These have changed not only the way we live, but especially the way we think and comprehend our universe. The large scientific projects have had a catalyzing role in this process and the LHC is showing all the right characteristics to be remembered as such. It should not come as a surprise that there is growing excitement, not only among physicists, for the upcoming LHC results and for its exploration into the depth of matter towards worlds that, although apparently so foreign to our common experience, hide the essence of the physical laws governing the universe.

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Project and original cost		Estimate in today's currency
Manhattan Project Total cost: 2.2 bn$ in 5 years (1942-46) (63% Oak Ridge, 21% Hanford, 5% Special operating materials, 4% Los Alamos, 4% Research and development, 2% Government overhead, 1% Heavy water plants) Estimate at approval (1942): 0.148 bn$ in 3 years (1942-44)	[19]	16 bn€
Apollo Program Total cost: 19.4 bn$ in 14 years (1960-73) (17 missions with 6 Moon landings) NASA estimate in 1966: 22.7 bn$ in 13 years	[20]	70 bn€
Hubble Space Telescope (HST) Construction cost: 1.5 bn$ Initial estimate: 0.5 bn$ Estimated total cost: 6 bn$ in 15 years (1990-2014)	[21]	4 bn€
Superconducting Super Collider (SSC) Estimated cost at cancellation (1993): 11.8 bn$ Estimated cost at approval (1987): 4.4 bn$	[13]	12 bn€
International Space Station (ISS) Assembly started 1998; first crew in 2000 Estimate for development, assembly, & operation costs (1998): 96 bn$ Initial estimate: 17.4 bn$	[22]	70 bn€
Human Genome Project (HGP) Total cost of the scientific program in genomics: 3 bn$ in 14 years (1990-2003) (Human genome sequencing represents only a small fraction)	[23]	2 bn€
International Thermonuclear Experimental Reactor (ITER) Estimated construction cost (2010): 12.8 bn€ in 10 years (2008-2017)	[24]	13 bn€
Large Hadron Collider (LHC) Construction cost (accelerator & detectors; material only, personnel not included): 6 bnCHF		4 bn€

Table 1: Indicative estimate of the original costs of some large projects, in units of billions of dollars (bn$) or billions of Swiss francs (bnCHF). In the right column the costs are expressed in billions of today's euro (bn€). For the dollar revaluation I used the conversion factors in [25]. For the currency exchange I used 1 € = 1.4 $ =1.5 CHF. I chose an average value of the Swiss franc at the time of the LHC construction, rather than today's exchange rate.

	Personnel [bnCHF]	Material [bnCHF]	Total [bnCHF]
LHC machine and experimental areas (incl. R&D, injectors, tests and pre-operation)	1.224	3.756	4.980
CERN contribution to detectors (incl. R&D tests and pre-operation)	0.869	0.493	1.362
CERN contribution to LHC computing	0.085	0.083	0.168
Total CERN costs	2.178	4.332	6.510

Table 2: LHC costs in billions of Swiss francs (bnCHF), according to the CERN budget [26].