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The Crisis of Big Science

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Superconducting Super Collider Laboratory/Photo Researchers

Construction of an underground shaft for the Superconducting Super Collider in Texas. The SSC was supposed to be the largest particle accelerator in the world, but its funding was canceled by Congress in 1993.

Last year physicists commemorated the centennial of the discovery of the atomic nucleus. In experiments carried out in Ernest Rutherford's laboratory at Manchester in 1911, a beam of electrically charged particles from the radioactive decay of radium was directed at a thin gold foil. It was generally believed at the time that the mass of an atom was spread out evenly, like a pudding. In that case, the heavy charged particles from radium should have passed through the gold foil, with very little deflection. To Rutherford's surprise, some of these particles bounced nearly straight back from the foil, showing that they were being repelled by something small and heavy within gold atoms. Rutherford identified this as the nucleus of the atom, around which electrons revolve like planets around the sun.

This was great science, but not what one would call big science. Rutherford's experimental team consisted of one postdoc and one undergraduate. Their work was supported by a grant of just £70 from the Royal Society of London. The most expensive thing used in the experiment was the sample of radium, but Rutherford did not have to pay for it—the radium was on loan from the Austrian Academy of Sciences.

Nuclear physics soon got bigger. The electrically charged particles from radium in Rutherford's experiment did not have enough energy to penetrate the electrical repulsion of the gold nucleus and get into the nucleus itself. To break into nuclei and learn what they are, physicists in the 1930s invented cyclotrons and other machines that would accelerate charged particles to higher energies. The late Maurice Goldhaber, former director of Brookhaven Laboratory, once reminisced:

> The first to disintegrate a nucleus was Rutherford, and there is a picture of him holding the apparatus in his lap. I then always remember the later picture when one of the famous cyclotrons was built at Berkeley, and all of the people were sitting in the lap of the cyclotron.

1.

After World War II, new accelerators were built, but now with a different purpose. In observations of cosmic rays, physicists had found a few varieties of elementary particles different from any that exist in ordinary atoms. To study this new kind of matter, it was necessary to create these particles artificially in large numbers. For this physicists had to accelerate beams of ordinary particles like protons—the nuclei of hydrogen atoms—to higher energy, so that when the protons hit atoms in a stationary target their energy could be transmuted into the masses of particles of new types. It was not a matter of setting records for the highest-energy accelerators, or even of collecting more and more exotic species of particles, like orchids. The point of building these accelerators was, by creating new kinds of matter, to learn the laws of nature that govern all forms of matter. Though many physicists preferred small-scale experiments in the style of Rutherford, the logic of discovery forced physics to become big.

In 1959 I joined the Radiation Laboratory at Berkeley as a postdoc. Berkeley then had the world's most powerful accelerator, the Bevatron, which occupied the whole of a large building in the hills above the campus. The Bevatron had been built specifically to accelerate protons to energies high enough to create antiprotons, and to no one's surprise antiprotons were created. What was surprising was that hundreds of types of new, highly unstable particles were also created. There were so many of these new types of particles that they could hardly all be elementary, and we began to doubt whether we even knew what was meant by a particle being elementary. It was all very confusing, and exciting.

After a decade of work at the Bevatron, it became clear that to make sense of what was being discovered, a new generation of higher-energy accelerators would be needed. These new accelerators would be too big to fit into a laboratory in the Berkeley hills. Many of them would also be too big as institutions to be run by any single university. But if this was a crisis for Berkeley, it wasn't a crisis for physics. New accelerators were built, at Fermilab outside Chicago, at CERN near Geneva, and at other laboratories in the US and Europe. They were too large to fit into buildings, but had now become features of the landscape. The new accelerator at Fermilab was four miles in circumference, and was accompanied by a herd of bison, grazing on the restored Illinois prairie.

By the mid-1970s the work of experimentalists at these laboratories, and of theorists using the data that were gathered, had led us to a comprehensive and now well-verified theory of particles and forces, called the Standard Model. In this theory, there are several kinds of elementary particles. There are strongly interacting quarks, which make up the protons and neutrons inside atomic nuclei as well as most of the new particles discovered in the 1950s and 1960s. There are more weakly interacting particles called leptons, of which the prototype is the electron.

There are also "force carrier" particles that move between quarks and leptons to produce various forces. These include (1) photons, the particles of light responsible for electromagnetic forces; (2) closely related particles called W and Z bosons that are responsible for the weak nuclear forces that allow quarks or leptons of one species to change into a different species—for instance, allowing negatively charged "down quarks" to turn into positively charged "up quarks" when carbon-14 decays into nitrogen-14 (it is this gradual decay that enables carbon dating); and (3) massless gluons that produce the strong nuclear forces that hold quarks together inside protons and neutrons.

Successful as the Standard Model has been, it is clearly not the end of the story. For one thing, the masses of the quarks and leptons in this theory have so far had to be derived from experiment, rather than deduced from some fundamental principle. We have been looking at the list of these masses for decades now, feeling that we ought to understand them, but without making any sense of them. It has been as if we were trying to read an inscription in a forgotten language, like Linear A. Also, some important things are not included in the Standard Model, such as gravitation and the dark matter that astronomers tell us makes up five sixths of the matter of the universe.

So now we are waiting for results from a new accelerator at CERN that we hope will let us make the next step beyond the Standard Model. This is the Large Hadron Collider, or LHC. It is an underground ring seventeen miles in circumference crossing the border between Switzerland and France. In it two beams of protons are accelerated in opposite directions to energies that will eventually reach 7 TeV in each beam, that is, about 7,500 times the energy in the mass of a proton. The beams are made to collide at several stations around the ring, where detectors with the mass of World War II cruisers sort out the various particles created in these collisions.

Some of the new things to be discovered at the LHC have long been expected. The part of the Standard Model that unites the weak and electromagnetic forces, presented in 1967–1968, is based on an exact symmetry between these forces. The W and Z particles that carry the weak nuclear forces and the photons that carry electromagnetic forces all appear in the equations of the theory as massless particles. But while photons really are massless, the W and Z are actually quite heavy. Therefore, it was necessary to suppose that this symmetry between the electromagnetic and weak interactions is "broken"—that is, though an exact property of the equations of the theory, it is not apparent in observed particles and forces.

The original and still the simplest theory of how the electroweak symmetry is broken, the one proposed in 1967–1968, involves four new fields that pervade the universe. A bundle of the energy of one of these fields would show up in nature as a massive, unstable, electrically neutral particle that came to be called the Higgs boson.[1] All the properties of the Higgs boson except its mass are predicted by the 1967–1968 electroweak theory, but so far the particle has not been observed. This is why the LHC is looking for the Higgs—if found, it would confirm the simplest version of the electroweak theory. In December 2011 two groups reported hints that the Higgs boson has been created at the LHC, with a mass 133 times the mass of the proton, and signs of a Higgs boson with this mass have since then turned up in an analysis of older data from Fermilab. We will know by the end of 2012 whether the Higgs boson has really been seen.

The discovery of the Higgs boson would be a gratifying verification of present theory, but it will not point the way to a more comprehensive future theory. We can hope, as was the case with the Bevatron, that the most exciting thing to be discovered at the LHC will be something quite unexpected. Whatever it is, it's hard to see how it could take us all the way to a final theory, including gravitation. So in the next decade, physicists are probably going to ask their governments for support for whatever new and more powerful accelerator we then think will be needed.

2.

That is going to be a very hard sell. My pessimism comes partly from my experience in the 1980s and 1990s in trying to get funding for another large accelerator.

In the early 1980s the US began plans for the Superconducting Super Collider, or SSC, which would accelerate protons to 20 TeV, three times the maximum energy that will be available at the CERN Large Hadron Collider. After a decade of work, the design was completed, a site was selected in Texas, land bought, and construction begun on a tunnel and on magnets to steer the protons.

Then in 1992 the House of Representatives canceled funding for the SSC. Funding was restored by a House–Senate conference committee, but the next year the same happened again, and this time the House would not go along with the recommendation of the conference committee. After the expenditure of almost two billion dollars and thousands of man-years, the SSC was dead.

One thing that killed the SSC was an undeserved reputation for over-spending. There was even nonsense in the press about spending on potted plants for the corridors of the administration building. Projected costs did increase, but the main reason was that, year by year, Congress never supplied sufficient funds to keep to the planned rate of spending. This stretched out the time and hence the cost to complete the project. Even so, the SSC met all technical challenges, and could have been completed for about what has been spent on the LHC, and completed a decade earlier.

Spending for the SSC had become a target for a new class of congressmen elected in 1992. They were eager to show that they could cut what they saw as Texas pork, and they didn't feel that much was at stake. The cold war was over, and discoveries at the SSC were not going to produce anything of immediate practical importance. Physicists can point to technological spin-offs from high-energy physics, ranging from synchotron radiation to the World Wide Web. For promoting invention, big science in this sense is the technological equivalent of war, and it doesn't kill anyone. But spin-offs can't be promised in advance.

What really motivates elementary particle physicists is a sense of how the world is ordered—it is, they believe, a world governed by simple universal principles that we are capable of discovering. But not everyone feels the importance of this. During the debate over the SSC, I was on the Larry King radio show with a congressman who opposed it. He said that he wasn't against spending on science, but that we had to set priorities. I explained that the SSC was going to help us learn the laws of nature, and I asked if that didn't deserve a high priority. I remember every word of his answer. It was "No."

What does motivate legislators is the immediate economic interests of their constituents. Big laboratories bring jobs and money into their neighborhood, so they attract the active support of legislators from that state,

Science Source

Ernest Rutherford holding the apparatus he used to *disintegrate a nitrogen nucleus, circa 1917*

and apathy or hostility from many other members of Congress. Before the Texas site was chosen, a senator told me that at that time there were a hundred senators in favor of the SSC, but that once the site was chosen the number would drop to two. He wasn't far wrong. We saw several members of Congress change their stand on the SSC after their states were eliminated as possible sites.

Another problem that bedeviled the SSC was competition for funds among scientists. Working scientists in all fields generally agreed that good science would be done at the SSC, but some felt that the money would be better spent on other fields of science, such as their own. It didn't help that the SSC was opposed by the president-elect of the American Physical Society, a solid-state physicist who thought the funds for the SSC would be better used in, say, solid-state physics. I took little pleasure from the observation that none of the funds saved by canceling the SSC went to other areas of science.

All these problems will emerge again when physicists go to their governments for the next accelerator beyond the LHC. But it will be worse, because the next accelerator will probably have to be an international collaboration. We saw recently how a project to build a laboratory for the development of controlled thermonuclear power, ITER, was nearly killed by the competition between France and Japan to be the laboratory's site.

There are things that can be done in fundamental physics without building a new generation of accelerators. We will go on looking for rare processes, like an extremely slow conjectured radioactive decay of protons. There is much to do in studying the properties of neutrinos. We get some useful information from astronomers. But I do not believe that we can make significant progress without also pushing back the frontier of high energy. So in the next decade we may see the search for the laws of nature slow to a halt, not to be resumed again in our lifetimes.

Funding is a problem for all fields of science. In the past decade, the National Science Foundation has seen the fraction of grant proposals that it can fund drop from 33 percent to 23 percent. But big science has the special problem that it can't easily be scaled down. It does no good to build an accelerator tunnel that only goes halfway around the circle.

3.

Astronomy has had a very different history from physics, but it has wound up with much the same problems. Astronomy became big science early, with substantial support from governments, because it was useful in a way that, until recently, physics was not.[2] Astronomy was used in the ancient world for geodesy, navigation, time-keeping, and making calendars, and in the form of astrology it was imagined to be useful for predicting the future. Governments established research institutes: the Museum of

Hellenistic Alexandria; the House of Wisdom of ninth-century Baghdad; the great observatory in Samarkand built in the 1420s by Ulugh Beg; Uraniborg, Tycho Brahe's observatory, built on an island given by the king of Denmark for this purpose in 1576; the Greenwich Observatory in England; and later the US Naval Observatory.

In the nineteenth century rich private individuals began to spend generously on astronomy. The third Earl of Rosse used a huge telescope called Leviathan in his home observatory to discover that the nebulae now known as galaxies have spiral arms. In America observatories and telescopes were built carrying the names of donors such as Lick, Yerkes, and Hooker, and more recently Keck, Hobby, and Eberly.

But now astronomy faces tasks beyond the resources of individuals. We have had to send observatories into space, both to avoid the blurring of images caused by the earth's atmosphere and to observe radiation at wavelengths that cannot penetrate the atmosphere. Cosmology has been revolutionized by satellite observatories such as the Cosmic Background Explorer, the Hubble Space Telescope, and the Wilkinson Microwave Anisotropy Probe, working in tandem with advanced ground-based observatories. We now know that the present phase of the Big Bang started 13.7 billion years ago. We also have good evidence that, before that, there was a phase of exponentially fast expansion known as inflation.

But cosmology is in danger of becoming stuck, in much the same sense as elementary particle physics has been stuck for decades. The discovery in 1998 that the expansion of the universe is now accelerating can be accommodated in various theories, but we don't have observations that would point to the right theory. The observations of microwave radiation left over from the early universe have confirmed the general idea of an early era of inflation, but do not give detailed information about the physical processes involved in the expansion. New satellite observatories will be needed, but will they be funded?

The recent history of the James Webb Space Telescope, planned as the successor to Hubble, is disturbingly reminiscent of the history of the SSC. At the funding level requested by the Obama administration last year, the project would continue, but at a level that would not allow the telescope ever to be launched into orbit. In July the House Appropriations Committee voted to cancel the Webb telescope altogether. There were complaints about cost increases, but as was the case with the SSC, most of the increase came because year by year the project was not adequately funded. Funding for the telescope has recently been restored, but the prognosis for future funding is not bright. The project is no longer under the authority of NASA's Science Mission Directorate. The technical performance of the Webb project has been excellent, and billions have already been spent, but the same was true of the SSC, and did not save it from cancellation.

Meanwhile, in the past few years funding has dropped for astrophysics at NASA. In 2010 the National Research Council carried out a survey of opportunities for astronomy in the next ten years, setting priorities for new observatories that would be based in space. The highest priorities went first to WFIRST, an infrared survey telescope; next to Explorer, a program of mid-sized observatories similar in scale to the Wilkinson Microwave Anisotropy Probe; then to LISA, a gravitational wave observatory; and finally to an international X-ray observatory. No funds are in the budget for any of these.

Some of the slack in big science is being taken up by Europe, as for instance with the LHC and a new microwave satellite observatory named Planck. But Europe has worse financial problems than the US, and the European Union Commission is now considering the removal of large science projects from the EU budget.

Space-based astronomy has a special problem in the US. NASA, the government agency responsible for this work, has always devoted more of its resources to manned space flight, which contributes little to science. All of the space-based observatories that have contributed so much to astronomy in recent years have been unmanned. The International Space Station was sold in part as a scientific laboratory, but nothing of scientific importance has come from it. Last year a cosmic ray observatory was carried up to the Space Station (after NASA had tried to remove it from the schedule for shuttle flights), and for the first time significant science may be done on the Space Station, but astronauts will have no part in its operation, and it could have been developed more cheaply as an unmanned satellite.

The International Space Station was partly responsible for the cancellation of the SSC. Both came up for a crucial vote in Congress in 1993. Because the Space Station would be managed from Houston, both were seen as Texas projects. After promising active support for the SSC, in 1993 the Clinton administration decided that it could only support one large technological project in Texas, and it chose the Space Station. Members of Congress were hazy about the difference. At a hearing before a House committee, I heard a congressman say that he could see how the Space Station would help us to learn about the universe, but he couldn't understand that about the SSC. I could have cried. As I later wrote, the Space Station had the great advantage that it cost about ten times more than the SSC, so that NASA could spread contracts for its development over many states. Perhaps if the SSC had cost more, it would not have been canceled.

4.

Big science is in competition for government funds, not only with manned space flight, and with various programs of real science, but also with many other things that we need government to do. We don't spend enough on education to make becoming a teacher an attractive career choice for our best college graduates. Our passenger rail lines and Internet services look increasingly poor compared with what one finds in Europe and East Asia. We don't have enough patent inspectors to process new patent applications without endless delays. The overcrowding and understaffing in some of our prisons amount to cruel and unusual punishment. We have a shortage of judges, so that civil suits take years to be heard.

The Securities and Exchange Commission, moreover, doesn't have enough staff to win cases against the corporations it is charged to regulate. There aren't enough drug rehabilitation centers to treat addicts who want to be treated. We have fewer policemen and firemen than before September 11. Many people in America cannot count on adequate medical care. And so on. In fact, many of these other responsibilities of government have been treated worse in the present Congress than science. All these problems will become more severe if current legislation forces an 8 percent sequestration—or reduction, in effect—of nonmilitary spending after this year.

We had better not try to defend science by attacking spending on these other needs. We would lose, and would deserve to lose. Some years ago I found myself at dinner with a member of the Appropriations Committee of the Texas House of Representatives. I was impressed when she spoke eloquently about the need to spend money to improve higher education in Texas. What professor at a state university wouldn't want to hear that? I naively asked what new source of revenue she would propose to tap. She answered, "Oh, no, I don't want to raise taxes. We can take the money from health care." This is not a position we should be in.

It seems to me that what is really needed is not more special pleading for one or another particular public good, but for all the people who care about these things to unite in restoring higher and more progressive tax rates, especially on investment income. I am not an economist, but I talk to economists, and I gather that dollar for dollar, government spending stimulates the economy more than tax cuts. It is simply a fallacy to say that we cannot afford increased government spending. But given the anti-tax mania that seems to be gripping the public, views like these are political poison. This is the real crisis, and not just for science. [3]

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1. 1
In his recent book, *The Infinity Puzzle* (Basic Books, 2011), Frank Close points out that a mistake of mine was in part responsible for the term "Higgs boson." In my 1967 paper on the unification of weak and electromagnetic forces, I cited 1964 work by Peter Higgs and two other sets of theorists. This was because they had all explored the mathematics of symmetry-breaking in general theories with force-carrying particles, though they did

not apply it to weak and electromagnetic forces. As known since 1961, a typical consequence of theories of symmetry-breaking is the appearance of new particles, as a sort of debris. A specific particle of this general class was predicted in my 1967 paper; this is the Higgs boson now being sought at the LHC .

As to my responsibility for the name "Higgs boson," because of a mistake in reading the dates on these three earlier papers, I thought that the earliest was the one by Higgs, so in my 1967 paper I cited Higgs first, and have done so since then. Other physicists apparently have followed my lead. But as Close points out, the earliest paper of the three I cited was actually the one by Robert Brout and François Englert. In extenuation of my mistake, I should note that Higgs and Brout and Englert did their work independently and at about the same time, as also did the third group (Gerald Guralnik, C.R. Hagen, and Tom Kibble). But the name "Higgs boson" seems to have stuck. ↩

2. 2
   I have written more about this in "The Missions of Astronomy," *The New York Review* , October 22, 2009. ↩

3. 3
   This article is based on the inaugural lecture in the series "On the Shoulders of Giants" of the World Science Festival in New York on June 4, 2011, and on a plenary lecture at the meeting of the American Astronomical Society in Austin on January 9, 2012. ↩