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Steven Weinberg (5/3/1933 – 7/23/2021)

Nobel Prize in Physics, 1979. Recognized Contributions to Physics: Electroweak Interaction, Weinberg Angle, Weinberg-Witten Theorem, Joos-Weinberg Equation, Asymptotic Safety, Axion Model, Effective Action, Minimal Subtraction Theme, Technicolor, Unitary Gauge

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Welcome to Texas

I am very grateful to the organizers of this meeting for making it possible for me to talk to you without my actually having to get on airplane – something I like to avoid as much as possible. Today I am going to talk a bit about the process of scientific research, using my own field elementary particle physics as an example. I will then go beyond this formulation to some larger issues that face not only scientists, but society in general.

Plato, as you may know, had very clear ideas about how to pursue knowledge about the natural world. His idea was that you do it by thinking about it. If you want to learn astronomy or learn about the heavens, for Plato you must first think about the way the heavens should behave. Plato did acknowledge that it might,

sometimes, be helpful actually to look at the sky. However, he thought that looking would be helpful in the same way that a mathematician, working out the principles of geometry, might be helped by drawing an occasional diagram. For Plato, these diagrams were not essential to the progress of mathematics, but they focused the mind. In this way, the observation of nature, according to Plato, was not essential to the progress of science. Plato, of course, was wrong about this, as he was wrong about the great deal else.

The opposing view was taken millennium later by Sir Frances Bacon, the Lord Chancellor of England in the reign of King James the first. Bacon believed that the way to learn nature is by patient observation, without theoretical preconceptions. For Bacon, after a great deal of data has been amassed, the answer of nature of physical reality will become apparent. Bacon also was wrong, although scientists following Bacon often describe themselves as proceeding in a true Baconian fashion.

To truly make progress in science, we must abandon Bacon's method and involve ourselves in theoretical speculation proposed by Plato, as well as in observation. Indeed, progress in science depends on a peak interconnection between theory and observation. Theory often, but not always, crystallizes to explain observation in the form of the mathematics that Plato so much admired. Observation often, although not always, leads to insight, although not necessarily passive observation – but observation combined with interference with the Natural order (an experiment) as Galileo did when he rolled balls down in inclined plane.

In my old field of elementary particle physics we had something of a golden age of progress during the 1960 and 1970's, when experimental information became available that provided powerful stimulus to theory, and the theories came along that were relevant to experimental observation. These theories often inspired new theories, and also most importantly, these new theories could be tested by doing further experiments. As the result of this work in the 1960s and 1970s, which I and many other theorists and experimentalists were privileged to participate in, by the end of this period we developed a theory of matter and force called the Standard Model.

In the Standard Model there are various kinds of matter, including particles called quarks and other particles called leptons (a leading example of a lepton is the electron). We catalogued also different kinds of forces, including the strong, weak, and electromagnetic forces accounting for everything that we could observe in our laboratories. It was not a final theory. The Standard Model left out a force which only becomes manifest on astronomical scales – the force of gravitation. The Standard Model also had a number of arbitrary features. For example, we have to take masses of the various particles – quarks and leptons – from the observation, rather than explaining them on any rational basis. But it was a very successful theory that accounted for a vast quantity of experimental data, and we were very happy with it.

Since then, since I would say late 1970s and early 1980s progress has been quite disappointing. Well aware of the missing elements of the Standard Model, we nevertheless been unable to supplant them. This is not I think because of any wrong turn taken by any theorists or by experimentalists. Rather, it simply the fact that, with the experimental facilities available, we could not gain new data that would challenge the Standard Model to suggest extensions. Extensions to the Standard Model, we believed, would allow us to gain a more full understanding of nature, including understanding why masses of particles are what they are – bringing gravitation into picture.

The years from say the early 1980s to the present, 2010 has been the period of great frustration. Extremely attractive mathematical theories have been developed. I am talking about a theory called String Theory. I admire the work that has been done in String Theory. I haven't worked myself in it since 1980s, but it has led to nothing in the way of clear pathway to experimentation, which we know is the essence of progress in science.

We are hoping now that the progress will begin again with the advent of a large accelerator in Europe. The Large Hadron Collider has 17 miles of circumference underneath the ground, crossing the border between France and Switzerland, near Geneva. Two counter rotating beams of protons, the nuclei of the hydrogen atoms, will be made to collide at an energy much larger than any previous accelerator could reach. In these collisions, new kinds of matter will be produced, and we hope that these new kinds of matter would provide us the clues that we need to complete the Standard Model, and to go beyond the Standard Model.

I am not going to discuss what particular experiments will be done at the Large Hadron Collider – it will be a matter of years to work this out. But we are hopeful to have another exciting period of progress as we did in the 1970s, 1980s, 1960s.

I look forward to the work done at the Large Hadron Collider but not without some elements of sadness, because this all could have been done much earlier, and at a much higher energy, which would have given us much more capability to take the next big steps in elementary particle physics. In the late 1980 and early 1990s, just when we needed that, a large accelerator was planned and construction on it began here in Texas not very far south of Dallas near a town called Waxahachie. This would have been a much larger ring that would operate at a power some 3 times higher than that of the Large Hadron Collider, and it would have been completed a decade ago. With that collider we would now already be well on our way to understanding whatever new thing is to be discovered, and that we hope will be discovered at the Large Hadron Collider.

There was a great debate over building the Super Conducting Super Collider – the “Super Collider”, as it was popularly named – having to do with great cost of all of these accelerators. They are expensive – not expensive at the level of the 100’s of billions of dollars of man’s space flight – but expensive at the level several up to about 10 billion dollars. If the Super Conducting Super Collider here in Texas – if that been build according to the original proposals, and according to the original schedule – it would have cost somewhere in the neighborhood of 8 billion dollars. The cost would have been similar to what the Large Hadron Collider will have cost in Europe – not cheap by any means – and so naturally there was a great debate over whether this sort of experimental facilities was worth the money.

We in elementary particle physics of course had no doubt about it. We see ourselves part of the great historical tradition going back before the time of Plato to ancient priests who speculated about the nature of matter, and continuing through a number of great government supported facilities such as the museum of Alexandria, and House of Wisdom of Baghdad, and Tycho Brahe’s observatory Uraniborg, located on an island in the state of Denmark, near the border of Sweden. We see the sources of our tradition of pushing back the frontiers of human knowledge, making progress in understanding of the final laws of nature. So we had no doubt of the importance of this endeavor, but tax payers naturally were asking how important it was to them. And there were – apart from own desire to learn the laws of nature – valid arguments that we could make and did try to make.

One of the arguments had to deal with technological spinoffs. This sort of large technologically advance facility pushes the state of technology in ways that inevitably yield new technological capabilities, which turn out to be valuable for the society as a whole. A large accelerator like the Super Conducting Super Collider or the Large Hadron Collider in Europe involves an enormous engineering of high field magnets of the sort which were used in medical inventory for example, cooled by what are the largest facilities in the world for producing liquid helium. These facilities also push the state of the art in computing. For example, in the Large Hadron Collider, when the beams of protons collide there are about 100 million collision per second, every one of which is recorded. However, it is not possible to save that much information, and so in real time, without human intervention, computers study the raw data as it floods in. We have to sort out what are the interesting events and record the data on those events, throwing away the rest of the information – unimportant trivial data. That is a difficult task for computer programming. Perhaps the most visible example of a technological spinoff from high energy physics was the invention at CERN where the Large Hadron Collider resided of the hypertext markup language that is the basis of the world wide web, and the other computer programming ideas that went into the world wide web. These concepts were originally developed by high energy physicists as a means of communicating that large amounts of data with each other.

There are also intellectual synopses which are less obvious but also terribly important in developing elementary particle physics. We invent mathematical ideas which are invented to be applied to this very arcane field of elementary particle interactions. However, these mathematical ideas can, and do, then disseminate into the rest of science, very often into areas of science that have nothing to do elementary particles but have a great deal to do with technology. A classical example is a mathematical idea known as the normalization group. It isn’t really group theory, but this unfortunate name was given to it. This is a method of seeing how the description of a physical system changes as you increase or decrease the

resolution the fineness with which the system is observed. The method of normalization group, as I said, was developed in elementary particle physics in the 1950's. This idea in fact turned out to be absolutely indispensable in understanding many of the properties of ordinary material. Particularly, how matter behaves when it approaches critical points, as for example the sudden appearance of spontaneous magnetization in ordinary ferromagnets. The approach was not developed for that purpose, it was developed for the purpose of elementary particle physics, but good mathematics turns out to be valuable even in ways that had not been originally anticipated.

There was also a human spinoff in this work. By studying elementary particle physics, we trained generations of brilliant experimental and theoretical scientists who then went to work on problems that concern society more than elementary particle physics does directly. The most obvious example of this came in World War II. The United States and Britain, together, were tremendously aided by new technologies that came along. I am referring to the technologies, for example, of electromechanical computing which we used in code breaking, in the accurate development of artillery practices, and in the development of radar. Some of this work allowed the Anglo-American forces to track submarines on the surface from aeroplanes. Most famously this line of exploration allowed the evolution of the nuclear bomb. All of these technologies were developed by scientists who were not, before the war, in business of developing code breaking technology, or radar, or nuclear weapons. They were academic scientists who were engaged in pursuing physics and mathematics for its own sake, and yet when the war came their talents were needed. They were there and served their countries.

So I think there is a lot to be said and that can be said and that was said in favor of this kind of fundamental scientific research even where its practical utility is not apparent. Of course, there is also always the possibility that knowledge gained in these experiments will turn out in ways that we can't now foresee to have practical importance. For example, at the end of 19th century of experimentalists at the Cavendish laboratory at Cambridge were studying the motion of electric currents through evacuated glass tubes similar to modern cathode ray tubes. In the course of this work JJ Thomson came to the conclusion that these electric currents, and by inference all electric currents, are carried by tiny charged particles much lighter than individual atoms. These particles were given the name electrons.

This discovery and the ability of scientists to manipulate the flow of currents through evacuated tubes was at the root of the development of the industry of electronics. If JJ Thomson had set out in the 1890s to do work that would have direct practical importance, he might have worked on steam boilers, and not on the discharge of electric currents through evacuated tubes. Industry benefited greatly from his pursuit of knowledge for its own sake and not for the sake of industry.

Now we didn't win the argument. We didn't win the argument about the Superconducting Super Collider. It was cancelled in 1993 by a vote in the House of Representatives. The Senate voted to continue funding, but the house of Representatives voted against the project. In the conference committee the House of Representatives was so adamant about killing the project, that the Senate was not able to prevail. The project was killed, after several billion dollars had already been spent, two thirds of the tunnel dug under the ground, and hundreds of farmers had been displaced from their land to make way for the buildings that would house the scientists and engineers running the Super Collider. It was a great mistake and great tragedy.

Why did this happen? I think it is instructive to think of various reasons. One reason was that it was very difficult to communicate to the public why this was important. It was even more difficult to give the public a sense of excitement about the kind of results that might be learned. We communicate to the public through journalists. Some of the journalists covering science are very talented and very perceptive, but they work in an institutional framework which inevitably drives science journalism in the direction of great shallowness. I don't know how many times I been in a physics conference back in the 1960s and 1970s during that golden age of great progress in elementary practical physics when new exciting results were described and instead of trying to explain the results to the public, journalists would always fall back on the narrative that the physicist were recreating conditions in the early universe. I have seen this again and again now with regard to results emerging from the Large Hadron Collider. Results are always explained in terms of recreating conditions in the early universe, because the journalists or the people that employ them feel that public does have a certain fascination with the origin of the universe but couldn't care less

about particles and forces. While understandable, one has to try to explain science in its own terms, in terms what its actually trying to accomplish, rather than some mythical goal of recreating the conditions of the early universe which is not our aim.

In fact, the collisions of the Large Hadron Collider cannot recreate the conditions in the early universe because when 2 protons collide you have a lot of quarks and relatively few anti-quarks. In the early universe the number of quarks and anti-quarks are nearly identical. So whatever energy and intensity this accelerator is reaching, it will not be recreated the early universe. We will be discovering things about the laws of nature that are directly relevant to what was going on in the early universe. These are things that we need to know in order to improve our understanding of the early history of the universe – but we cannot recreate the conditions of the early universe.

It is a deep problem, in my view, that we value private goods too highly and public goods not highly enough. I of course emphasize the public good of scientific research as this is the sort of thing I do, but many of the problems and in fact I would say most of the problems confronting society have to do with public need for various public goods such as education, healthcare, border security, national security, and medical research. These are all things that we pay for with tax dollars. Now many people seem to feel that the public sector of the economy is somehow parasitic on the private sector. In this view, if you want to stimulate the economy you must put money in hands of consumers, who will buy private goods – automobiles, consumer electronics, or food – rather in hands of government who will buy public goods such as national defense medical care, public house, border security etc.

Dollars spent on public goods circulates through the economy essentially same way that the dollars spent on private goods circulates. It's just a question of what kind of goods society wants. Do we want better consumer electronics or better medical research? I remember for example, during the time George W Bush was governor of Texas – taxes at that time were running in surplus in its state budget. The university of Texas, which been starved for a decades by legislature, was at last beginning to hope that some of the damage that has been done to the higher education in Texas, would be reversed by the ability of the state government at last to spend money without having even to raise taxes. Instead the governor decided to take that surplus and return to the voters with few 100 dollars per voter in the form of a tax rebate. I think all the scientists like myself can do about is to talk about and try to urge other people like you, the audience I am talking to now, to talk about this and do what we can to shift the balance the of spending more in the direction of public goods, as difficult that may be politically.

I talk about the various benefits to the society, but some fundamental research is not aimed at directly benefiting society. It is rather aimed at improving our knowledge of nature and its most fundamental level. I have talked about technological spinoffs, intellectual spinoffs and human spinoffs. If there is another spinoff, which in my point of views is the most important spinoff of all, it is a civilizing spinoff. One of the greatest elements of our civilization is that we have learned a great deal about what makes the world the way it is. We learned about the particles of which matter is composed, and how forces act upon these particles. We have learned about how living organisms become what they are through the process of evolution, given direction by natural selection. We have learned great deal about the human mind, about the nature of universe as a whole. All of these lessons, I think, are the glorious parts of our civilization. As we learn more and more about the world, we become less and less subject to superstition. The historian Hooper has pointed out was the advanced of science in 17th century particularly with the work of Isaac Newton that led in 18th century to the gradual end of the practice of accusing and burning witches.

I would include among the superstitions which science tends to drive out is religious certainty. Now I am not saying that scientists can't be religious. But the kind of certainty, for example, that drives young men to fly an airplanes into office buildings and plant bombs in cities in America or Europe or the Middle East – that kind of religious certainty, which is so opposite in its spirit of scientific research, I do regards as superstition. It is this sort of certainty, I think, that the advance of science is gradually weakening. I think, in the long run, the advance of science will help to remove from the human spirit superstition over centuries as this happens, it will in the end be the greatest contribution of scientific research to humanity.

Thank You.