18 Newtonianism, Reductionism and the Art of Congressional Testimony

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My talk this afternoon will be about the philosophy of science, rather than about science itself. This is somewhat uncharacteristic for me, and, I suppose, for working scientists in general. I've heard the remark (although I forget the source) that the philosophy of science is just about as useful to scientists as ornithology is to birds.

However, at just this time a question has arisen in the United States that will affect the direction of physics research until well into the twenty-first century, and that I think hinges very largely on a philosophical issue. On 30 January of this year the present administration in Washington announced that it had decided to go ahead with the construction of a large new accelerator for elementary particle physics, the Superconducting Supercollider, or SSC for short. "Large" in this case means that its circumference would be about 53 miles. The circumference is determined by the necessity of accelerating protons to energies of 20 TeV (2×10^{13} electron volt). Within this ring there would travel two counter-rotating beams of protons, that would slam into each other at a number of intersection regions. The intensity of the beams is designed to be such that one would have a collision rate of about one per second for typical processes (with a cross-section of a nanobarn). All of these design parameters lead to a bottom line parameter: the cost in 1986 dollars is estimated to be 4,400 million dollars.

The chief reason for wanting to go ahead with this accelerator is that it would open up a new realm of high energy which we have not yet been able to study. Just as when astronomers start to study the sky at a new wavelength, or when solid state physicists go down another factor of ten in temperature, every time particle accelerators go up a factor of ten in energy we discover exciting new physics. This has generally been the rationale for new accelerators. Occasionally, one can also point to specific discoveries that can be anticipated from a particular new accelerator. One example is provided by the accelerator built in Berkeley over 30 years ago, the Bevatron, which for the first time was capable of producing particles with masses of 1 GeV. (In those days American physicists talked about BeV instead of GeV.) The Bevatron was designed to be able to produce antiprotons, and indeed it did so shortly after it went on the air. That was not the only exciting thing done at that accelerator. Quite unexpected was the discovery of a vast forest of new mesonic and baryonic states, that led to a change in our conception of what we mean by an elementary particle. But in planning the Bevatron, it was nice to know in advance that at least one important discovery could be counted on.

The same is true now of the SSC. The SSC is so designed so that it will discover the particle known as the Higgs boson, provided that the Higgs boson is not too heavy. If the Higgs boson is too heavy, then the SSC will discover something else equally interesting.

Let me explain these remarks further. As many people may have heard, there has been a certain measure of unification among the forces of nature. This unification entails the idea that the symmetry among the forces, specifically the weak nuclear force and the electromagnetic force, is spontaneously broken. It can't be spontaneously broken by the forces we know about, that is, the ordinary strong and weak nuclear forces and the electromagnetic force; therefore there must be a new force in nature which is responsible for the symmetry breaking, like the phonon exchange force in a superconductor. We don't know exactly what that force is. The simplest picture is that it has to do with the existence of a new kind of elementary scalar particle. The members of the multiplet of elementary scalar particles that would be observable as physical particles are called Higgs bosons.

Now, we are not sure that that is actually the correct picture of the mechanism for electroweak symmetry breaking, and we certainly do not know the mass of the Higgs boson. The SSC would be able to discover the Higgs boson if its mass is not greater than about 850 GeV, and, of course, if it exists. However, the SSC (to borrow a phrase from M. Chanowitz[1]) is a no-lose proposition, because if the Higgs boson does not exist, or is heavier than 850 GeV, there would have to be strong interactions among longitudinally polarized W particles, which the SSC could also discover. These strong interactions would reveal the nature of the spontaneous symmetry breaking between the weak and the electromagnetic interactions.

Now it remains for Congress to decide whether or not to authorize construction of the accelerator and to appropriate the money. Two committees of the two houses of the Congress, the Committee on Space, Science, and Technology of the House of Representatives and the Subcommittee on Energy Research and Development of the Senate Committee on Energy and Natural Resources, announced hearings on the SSC, both to begin on 7 April of this year. In March, about a month before these hearings, I was asked to testify at them. I may admit that I found this more frightening than inviting. I had been active for some time in working for the building of the SSC, and all this time it had been a nightmare of mine that I would be called up before some tribunal, and asked in a stern voice why it is worth 4.4 billion dollars to find the Higgs boson. Also, I had testified in Congress only once before, and I did not consider myself a master of the art of congressional testimony.

The particle physicists of the United States are in fact quite united behind the idea that this is the right accelerator to build next. (As I said, its purpose is not limited to finding the Higgs boson, which is just one target, but, rather, it is to open up a new range of energies.) But there has been substantial opposition to the SSC from other physicists in the United States. I have read that this is perhaps the most divisive issue that has ever faced American physicists.[2] I believe that in Britain there is a similar debate—not about building an SSC but about whether Britain should remain in CERN, an issue on which I gather not all British scientists agree.

Heavyweights

I knew at the hearings in Washington there would be two heavyweights who would be testifying vigorously against going ahead with the SSC. One would be Philip Anderson, known to everyone as among the leading condensed matter physicists in the world. Anderson has over many years opposed the large sums that are spent on high energy physics. Another to testify would be James Krumhansl, also a distinguished solid state physicist. He, as it happens, taught me physics when I was a freshman at Cornell, but in addition, and this I suspect counts for more, he is slated the year after next to be the president of the American Physical Society.

Both Anderson and Krumhansl I knew would oppose the SSC, and they would be making arguments with which I really couldn't disagree. In particular, I expected that they would argue that money spent on elementary particle physics, high energy physics, whatever you want to call it, is not as sure to yield immediate technological advances as the same money spent on condensed matter physics, and some other fields. I would have to agree with that (though I would put more emphasis on the benefits of unpredictable discoveries and spin-offs). I expected that they would also argue that elementary particle physics is not more intellectually profound than other areas of physics like, say, condensed matter physics. I would also agree with that. In fact, we've seen in the last few decades a continual trading back and forth of ideas between elementary particle physics and condensed matter physics. We learned about broken symmetry from them, they learned about the renormalization group from us. And now we're all talking about conformal quantum field theories in two dimensions (I don't know who learned that from whom). But it is clear that there's no lack of mathematical profundity in condensed matter physics as compared with elementary particle physics.

The case for spending large sums of money on elementary particle physics has to be made in a different way. It has to be at least in part based on the idea that particle physics (and here, parenthetically, I should say that under "particle physics" I include quantum field theory, general relativity, and related areas of astrophysics and cosmology) is in *some* sense more fundamental than other areas of physics. This was denied more or less explicitly by Anderson and Krumhansl in their testimony and also by most of the opponents of the SSC. I didn't see how I could avoid this issue in making a case for the SSC. But it's a dangerous argument. It tends to irritate one's friends in other areas of science. Let me give an example, and here I will quote from myself because then I want to quote some comments on my own remarks.

In 1974, shortly after the standard model was put into its final form with the success of quantum chromodynamics, I wrote an article[3] for *Scientific American* called "Unified Theories of Elementary Particle Interactions." Just to get the article started I began it with some platitudes, as follows: "One of man's enduring hopes has been to find a few simple general laws that would explain why nature with all its seeming complexity and variety is the way it is. At the present moment the closest we can come to a unified view of nature is a description in terms of elementary particles and their mutual interactions." I really didn't intend to make any important point by this; it was just the sort of thing one says (as, for instance, Einstein: "The supreme test of the physicist is to arrive at those universal elementary laws from which the cosmos can be built up by pure deduction"). Then a decade later I was asked by the MIT Press to review a proposed book, a collection of articles by various scientists. In the manuscript I found an article[4] by a friend of mine at Harvard, Ernst Mayr, who is one of the most eminent evolutionary biologists of our times. I found that Mayr cited the remarks in the *Scientific American* article as "a horrible example of the way physicists think." He called me "an uncompromising reductionist."

Agreement

Now, I strongly suspect that there is no real disagreement between Ernst Mayr and myself, and that in fact we are simply talking past each other, and we should try to understand how we agree rather than fight over this. I don't consider myself an uncompromising reductionist. I consider myself a compromising reductionist. I would like to try to formulate in what way elementary particle physics is more fundamental than other areas of physics, trying to narrow this down in such a way that we can all agree on it.

Let me first take up some of the things I don't mean. And here it is useful to look back at some more of Ernst Mayr's writing, because he is in fact the leading opponent of the reductionist tendency within biology, as well as in science in general. He wrote a book[5] in 1982, *The Growth of Biological Thought*, that contains a well-known attack on reductionism, and so I looked at it to see what Mayr thought reductionism was, and whether or not I consider myself, in his terms, a reductionist.

The first kind of reductionism that Mayr opposes is called by him "theory reductionism." As far as I can understand it, it's the notion that the other sciences will eventually lose their autonomy and all be absorbed into elementary particle physics; they will all be seen as just branches of elementary particle physics.

Now I certainly don't believe that. Even within physics itself, leaving aside biology, we certainly don't look forward to the extinction of thermodynamics and hydrodynamics as separate sciences; we don't even imagine that they are going to be reduced to molecular physics, much less to elementary particle physics. After all, even if you knew everything about water molecules and you had a computer good enough to follow how every molecule in a glass of water moved in space, all you would have would be a mountain of computer tape. How in that mountain of computer tape would you ever recognize the properties that interest you about the water, properties like vorticity, turbulence, entropy and temperature?

There is in the philosophical literature a term, emergence, that is used to describe how, as one goes to higher and higher levels of organization, new concepts emerge that are needed to understand the behaviour at that level. Anderson summarized this neatly in the title of an interesting article[6] in *Science* in 1972: "More is Different."

Another kind of reductionism is called by Mayr "explanatory reductionism." As I understand it, it is the idea that progress at the smallest level, say the level of elementary particle physics, is needed to make progress in other sciences, like hydrodynamics, condensed matter physics and so on.

I don't believe that either. I think we probably know all we need to know about elementary particle physics for the purposes of the solid state physicist, for instance, and the biologist. Mayr in his book makes a point that surprised me (but I suppose it's true; he knows a lot more about this than I do), that even the discovery of DNA was not really of much value in the science of transmission genetics. Mayr writes, "To be sure the chemical nature of a number of black boxes in the classical genetic theory were filled in by the discovery of DNA, RNA, and others, but this did not affect in any way the nature of transmission genetics."

I don't disagree with any of this, but it seems to me that in their attacks on reductionism, Mayr and also physicists like Anderson, Krumhansl and others, are missing the point. In fact, we all do have a sense that there are different levels of fundamentalness. For instance, even Anderson[7] calls DNA the "secret of life." We do have a feeling that DNA is fundamental to biology. It's not that it's needed to explain transmission genetics, and it's certainly not needed to explain human behaviour, but DNA is fundamental nonetheless. What is it then about the discovery of DNA that was fundamental to biology? And what is it about particle physics that is fundamental to everything?

Having spoken at length about what I don't mean, now I want to say what I do mean. But I'm not trying here to say anything new, that you don't all already know. What I'm trying to do is precisely the opposite: to identify what we can all agree on.

In all branches of science we try to discover generalizations about nature, and having discovered them we always ask why are they true. I don't mean why we believe they are true, but why they *are* true. Why is nature that way? When we answer this question the answer is always found partly in contingencies, that is, partly in just the nature of

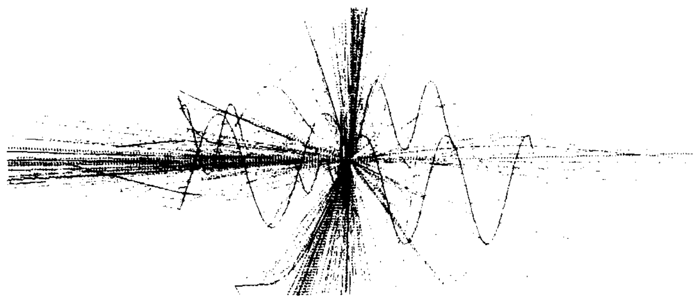

Figure 18.1
Hunting the Higgs—computer simulation of a proton-proton collision in the SSC. The picture is taken from "To the Heart of Matter," issued by the Universities Research Association.

the problem that we pose, but partly in other generalizations. And so there is a sense of direction in science, that some generalizations are "explained" by others.

To take an example relative to the tercentenary celebration of the *Principia*: Kepler made generalizations about planetary motion, Newton made generalizations about the force of gravity and the laws of mechanics. There is no doubt that historically Kepler came first and that Newton, and also Halley and Wren and others, derived the inverse square law of gravity from Kepler's laws. In formal logic, since Kepler's laws and Newton's laws are both true, either one can be said to imply the other. (After all, in formal logic the statement "A implies B" just means that it never happens that A is true and B isn't, but if A and B are both true then you can say that A implies B and B implies A.)

Intuition

Nevertheless, quite apart from formal logic, and quite apart from history, we intuitively understand that Newton's laws of motion and law of gravity are more fundamental than Kepler's laws of planetary motion. I don't know exactly what I mean by that; presumably it has something to do with the greater generality of Newton's laws, but about this also it's hard to be precise. But we all know what we mean when we say that Newton's laws "explain" Kepler's. We probably could use help from professional philosophers in formulating exactly what that statement means, but I do want to be clear that it is a statement about the way the Universe is, not about the way physicists behave. In the same way, even though new concepts "emerge" when we deal with fluids or many-body systems, we understand perfectly well that hydrodynamics and thermodynamics are what they are because of the principles of microscopic physics. No one thinks that the phenomena of phase transitions and chaos (to take two examples

quoted by Krumhansl) could have been understood on the basis of atomic physics without creative new scientific ideas, but does anyone doubt that real materials exhibit these phenomena because of the properties of the particles of which the materials are composed?

Another complication in trying to pin down the elusive concept of "explanation" is that very often the "explanations" are only in principle. If you know Newton's laws of motion and the inverse square law of gravity you can deduce Kepler's laws—that's not so hard. On the other hand, we also would say that chemical behaviour, the way molecules behave chemically, is explained by quantum mechanics and Coulomb's law, but we don't really deduce chemical behaviour for very complex molecules that way. We can for simple molecules; we can explain the way two hydrogen atoms interact to form a hydrogen molecule by solving Schrödinger's equation, and these methods can be extended to fairly large molecules, but we can't work out the chemical behaviour of DNA by solving Schrödinger's equation. In this case we can at least fall back on the remark that although we don't in fact calculate the chemical behaviour of such complicated molecules from quantum mechanics and Coulomb's law, we could if we wanted to. We have an algorithm, the variational principle, which is capable of allowing us to calculate anything in chemistry as long as we had a big enough computer and were willing to wait long enough.

The meaning of "explanation" is even less clear in the case of nuclear behaviour. No one knows how to calculate the spectrum of the iron nucleus, or the way the uranium nucleus behaves when fissioning, from quantum chromodynamics. We don't even have an algorithm; even with the biggest computer imaginable and all the computer time you wanted, we would not today know how to do such calculations. Nevertheless, most of us are convinced that quantum chromodynamics does explain the way nuclei behave. We say it explains it "in principle," but I am not really sure of what we mean by that.

Still, relying on this intuitive idea that different scientific generalizations explain others, we have a sense of direction in science. There are arrows of scientific explanation, that thread through the space of all scientific generalizations. Having discovered many of these arrows, we can now look at the pattern that has emerged, and we notice a remarkable thing: perhaps the greatest scientific discovery of all. These arrows seem to converge to a common source! Start anywhere in science and, like an unpleasant child, keep asking "Why?" You will eventually get down to the level of the very small.

By the mid-1920s, the arrows of explanation had been traced down to the level of the quantum mechanics of electrons, photons, atomic nuclei and, standing somewhat off in the corner, the classical theory of gravity. By the 1970s we had reached a deeper level—a quantum field theory of quarks, leptons and gauge bosons known as the standard model, and with gravity still somewhat isolated, described by a not very satisfactory quantum field theory of gravitons. The next step, many of us think, is the theory of superstrings, still under development. I myself, although a late-comer to this field, confess my enthusiasm for it. I think it provides our best hope of making the next step beyond the standard model.

Objective Reductionism

Now reductionism, as I've described it in terms of the convergence of arrows of explanation, is not a fact about scientific programmes, but is a fact about nature. I suppose if I had to give a name for it, I could call it objective reductionism. It is very far from a truism. In particular, these arrows of explanation might have led to many different sources. I think it's important to emphasize that, until very recently, most scientists thought that that was the case; this discovery, that the arrows of explanation point down to a common source, is quite new. (In a comment on an earlier version of this talk, Ernst Mayr informs me that what I call "objective reductionism" is what he means by "theory reductionism." Maybe so, but I prefer to keep the separate terms, because I wish to emphasize that what I am talking about here is not the future organization of the human scientific enterprise, but an order inherent in nature itself.)

To underscore this point, I'd like to mention a few examples of the contrary view surviving until well into the twentieth century. The first is biological vitalism, the idea that the usual rules of physics and chemistry need to be modified when applied to living organisms. One might have thought that this idea would have been killed off by the rise of organic chemistry and evolutionary biology in the nineteenth century. However, Max Perutz in his talk at the Schrödinger centenary in London in April reminded us that both Niels Bohr and Erwin Schrödinger believed that the laws of physics as then understood in the 1920s and 1930s were inadequate for understanding life.[8] Perutz explains that the problem of the orderliness of life that bothered Schrödinger was cleared up by advances in the understanding of enzymatic catalysis. Ernst Mayr was careful in his book to disavow any lingering attachment to vitalism, as follows: "Every biologist is fully aware of the fact that molecular biology has demonstrated decisively that all processes in living organisms can be explained in terms of physics and chemistry." (Mayr, by the way, is using the word "explained" in exactly the same sense as I am here.)

A second example, Lord Kelvin, in a speech to the British Association for the Advancement of Science, around 1900, said,[9] "There is nothing new to be discovered in physics now. All that remains is more and more precise measurement." There is a similar remark of Michelson's that is often quoted.[10] These remarks of Kelvin's and Michelson's are usually cited as examples of scientific arrogance and blindness, but I think this is based on a wrong interpretation of what Kelvin and Michelson meant. The reason that Kelvin and Michelson made these remarkable statements is, I would

guess, that they had a very narrow idea of what physics was. According to their idea, the subject matter of physics is motion, electricity, magnetism, light and heat, but not much else. They felt that that kind of physics was coming to an end, and in a sense it really was. Kelvin could not possibly have thought in 1900 that physics had already explained chemical behaviour. He didn't think so, but he also didn't think that was a task for physics. He thought that physics and chemistry were sciences on the same level of fundamentalness. We don't think that way today, but it isn't long ago that physicists did think that way.

I said that these arrows of explanation could have led down to a number of separate sciences. They also could have gone around in a circle. This is still a possibility. There is an idea that's not quite dead among physicists and cosmologists, the "anthropic principle," according to which there are constants of nature whose value is inexplicable except through the observation that if the constants had values other than what they have the Universe would be so different that scientists would not be there to ask their questions. If the anthropic principle were true, there would be a kind of circularity built into nature, and one would then I suppose have to say that there is no one fundamental level—that the arrows of explanation go round in circles. I think most physicists would regard the anthropic principle as a disappointing last resort to fall back on only if we persistently fail to explain the constants of nature and the other properties of nature in a purely microscopic way. We'll just have to see.

Now although what I have called objective reductionism became part of the general scientific understanding only relatively recently (after the development of quantum mechanics in the 1920s), its roots can be traced back to Newton (who else?). Newton was the first to show the possibility of an understanding of nature that was both comprehensive and quantitative. Others before him, from Thales to Descartes, had tried to make comprehensive statements about nature, but none of them took up the challenge of explaining actual observations quantitatively in a comprehensive physical theory.

I don't know of any place where Newton lays out this reductionist programme explicitly. The closest I can come to it is a remark in the Preface to the first edition of the *Principia*, written in May 1686. Newton says, "I wish we could derive the rest of the phenomena of nature by the same kind of reasoning from mechanical principles [I suppose he means as in the *Principia*] for I am induced by many reasons to suspect that they may all depend on certain forces." I suppose that the most dramatic example of the opening up by Newton of the possibility of a comprehensive quantitative understanding of nature is in the third book of the *Principia* where Newton reasons that the moon is 60 times further away from the centre of the Earth than Cambridge is (either Cambridge) and therefore the acceleration of the Moon towards the Earth should be less than the acceleration of an apple in Cambridge by a factor of 60^2. With this argument Newton unites celestial mechanics and observations of falling fruits in a way that

I think captures for the first time the enormous power of mathematical reasoning to explain not only idealized systems like planets moving in their orbits, but ultimately everything.

A digression. Since I have been talking about Newton, and also talking about the SSC, a prime example of "big science," I can't resist remarking that Newton himself was involved in big science.[11] In 1710, as President of the Royal Society, Newton by royal command was given control of observations at the largest national laboratory for science then in existence in England, the Greenwich Observatory. He was also given the responsibility of overseeing the repair of scientific instruments by the Master of Ordnance, an interesting connection with the military. (This arrangement, incidentally, infuriated the then Astronomer Royal, Flamsteed.)

Gaps

There are many gaps, of course, and perhaps there always will be many gaps in what I have called the chains of explanation. The great moments in the history of science are when these gaps are filled in, as for example when Darwin and Wallace explained how living things, with all their adaptations to their environment, could develop without any continuing external intervention. But there are still gaps.

Also, sometimes it isn't so clear which way the arrows of explanation point. Here's one example, a small one, but one that has bothered me for many years. We know mathematically that as a consequence of Einstein's general theory of relativity gravitational waves should be waves of spin two, and therefore when quantized, the theory of gravity should have in it particles of mass zero and spin two. On the other hand, we also know that any particles of mass zero and spin two must behave as described by Einstein's general theory of relativity. The question is, which is the explanation of which? Which is more fundamental, general relativity or the existence of particles of mass zero and spin two? I've oscillated in my thinking about this for many years. At the present moment in string theory the fact that the graviton has mass zero and spin two appears as an immediate consequence of the symmetries of the string theory, and the fact that gravity is described by the formalism of Riemannian geometry and general relativity is a somewhat secondary fact, which arises in a way that is still rather mysterious. But I don't know if that is the final answer. I mention this example just to show that although we don't always know which truths are more fundamental, it's still a worthwhile question to ask, because it is a question about the logical order of nature.

I believe that objective reductionism, reductionism as a statement about the convergence of arrows of explanation in nature, is by now ingrained among scientists, not only among physicists but also among biologists like Ernst Mayr. Let me give an example. Here's a quote from the presidential address of Richard Owen to the British Association in 1858.[12] Owen was an anatomist, generally regarded as the foremost of his time, and a great adversary of Darwin. In his address, Owen says, "Perhaps the most important and significant result of palaeontological research has been the establishment of the axiom of the continuous operation of the ordained becoming of living things." I'm not too clear what precisely Owen means by this axiom. But my point is that today no biologist would make such a statement, even if he or she knew what the axiom meant, because no biologist today would be content with an axiom about biological behaviour that could not be imagined to have an explanation at a more fundamental level. That more fundamental level would have to be the level of physics and chemistry, and the contingency that the Earth is billions of years old. In this sense, we are all reductionists today.

Now, these reflections don't in themselves settle the question of whether the SSC is worth 4.4 billion dollars. In fact, this might be a difficult problem, if we were simply presented with a choice between 4.4 billion dollars spent on the SSC and 4.4 billion dollars spent on other areas of scientific research. However I don't think that that's likely to be the choice with which we are presented. There is evidence that spending on "big science" tends to increase spending on other science, rather than the reverse. We don't really know with what the SSC will compete for funds. In any case, I haven't tried here to settle the question of whether or not the SSC should be built for 4.4 billion dollars—it is a complicated question, with many side arguments. All I have intended to argue here is that when the various scientists present their credentials for public support, credentials like practical values, spinoff etc., there is one special credential of elementary particle physics that should be taken into account and treated with respect, and that is that it deals with nature on a level closer to the source of the arrows of explanation than other areas of physics. But how much do you weigh this? That's a matter of taste and judgement, and I'm not paid to make that final decision. However I would like to throw into the balance one more point in favour of the SSC.

I have remarked that the arrows of explanation seem to converge to a common source, and in our work on elementary particle physics we think we're approaching that source. There is one clue in today's elementary particle physics that we are not only at the deepest level we can get right now, but we are at a level which is in fact in absolute terms quite deep, perhaps close to the final source. And here again I would like to quote from myself, from my own testimony in Congress, because afterwards I am going to quote some comments on these remarks, and I want you to know what it is that the comments were about:

> There is reason to believe that in elementary particle physics we are learning something about the logical structure of the Universe at a very very deep level. The reason I say this is because as we have been going to higher and higher energies and as we have been studying structures that are smaller and smaller we have found that the laws, the physical principles, that describe what we learn become simpler and simpler. I am not saying that the mathematics gets easier, Lord knows

it doesn't. I am not saying that we always find fewer particles in our list of elementary particles. What I am saying is that the rules that we have discovered become increasingly coherent and universal. We are beginning to suspect that this isn't an accident, that it isn't just an accident of the particular problems that we have chosen to study at this moment in the history of physics but there is simplicity, a beauty, that we are finding in the rules that govern matter that mirrors something that is built into the logical structure of the Universe at a very deep level. I think that this kind of discovery is something that is going on in our present civilization at which future men and women and not just physicists will look back with respect.

After I made these remarks there were remarks by other witnesses, and then there were questions from members of the Committee on Space, Science, and Technology. I am going to quote from the remarks of two of them. The first is Harris W. Fawell, Republican congressman from Illinois. Fawell throughout his questioning had been generally favourable to the SSC. The second is representative Don Ritter, of Pennsylvania, also a Republican, who had been the congressman most opposed to the SSC throughout the morning. (I suppose you could regard this as a modern dialogue between Sagredo and Simplicio.) I quote here from the unedited transcript of the hearings.

Mr Fawell: Thank you very much. I appreciate the testimony of all of you. I think it was excellent. If ever I would want to explain to one and all the reasons why the SSC is needed I am sure I can go to your testimony. It would be very helpful. I wish sometimes we have some one word that could say it all and that is kind of impossible. I guess perhaps Dr. Weinberg you came a little close to it and I'm not sure but I took this down. You said you suspect that it isn't all an accident that there are rules which govern matter and I jotted down, will this make us find God? I'm sure you didn't make that claim, but it certainly will enable us to understand so much more about the universe?

Mr Ritter: Will the gentleman yield on that? [That's something congressmen say to each other.] If the gentleman would yield for a moment I would say...

Mr Fawell: I'm not sure I want to.

Mr Ritter: If this machine does that I am going to come round and support it.

Now while this dialogue was going on I thought of a number of marvellous observations that I could make to score points for the SSC. However, by the time Mr Ritter reached his final remark I had decided to keep my mouth shut. And that, my friends, is what I learned about the art of congressional testimony.

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