ATOMS FOR PEACE + 50
Nuclear Energy & Science
for the 21st Century

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Dr. T. James Symons, Director, Nuclear Science Division, Lawrence Berkeley National Laboratory

Panel Chairman:
Dr. Raymond L. Orbach, Director, Office of Science, Department of Energy

ORBACH: The first area we're going to investigate is the nuclear physics world and we are very pleased to have Dr. James Symons, Senior Physicist and Director of the Nuclear Science Division at the Berkeley National Laboratory, LBNL, Lawrence Berkeley National Laboratory. He received his BA degree in Physics in 1972 and Doctor of Philosophy from Oxford University four years later. He's been a member of the LBNL staff since 1977 and his interests are in experimental nuclear physics, including nuclear structure and relatavistic heavy ion collisions. He's presently a member of the STAR Collaboration, engaged at RHIC, a remarkable detector at Brookhaven National Laboratory and he's a fellow of the American Physical Society.

He's served on numerous national and international panels including the DOE-NSF Nuclear Science Advisory, NSAC, our advisory committee, which he chaired for two years from the year 2000 to 2002. During his term as NSAC Chair, he led the development of a new, national long-range plan for nuclear science, "Opportunities in Nuclear Science Long-range for the Next Decade." And his talk is entitled, "Nature's Recipe for Nuclear Matter." Dr. Symons.

SYMONS: Thank you very much, Ray. Good afternoon. I'm like many of the speakers this afternoon, I read the speech. I wasn't familiar with the "Atoms for Peace" speech of President Eisenhower. And, of course, both from reading the speech itself and from the talks I've heard today, I learned a tremendous amount about his vision and leadership at that time. Now, when I was preparing this talk I looked at it and there was a sentence near the beginning that I liked. He wrote, "At the same time that I appreciate the distinction of addressing you, I also have a sense of exhilaration as I look upon this assembly."

I noticed in the Xerox that they put in our folders this morning that, actually, we heard that Eisenhower was a great writer and that he actually changed at the last minute. He changed excitement to exhilaration. Now I will comment on that because excitement is what you feel when you're sitting in the stands watching the Yankees and the Boston Red Sox and New York is knocking the Red Sox out of the World Series again. Exhilaration is what you feel when you are rounding the bases after hitting a home run. And that is what Aaron Boom(?) was feeling last Thursday when he hit that homerun.

And as I stand here facing you, I share President Eisenhower's feeling of honor of being able to address you and appreciate the distinction of that honor, but I don't feel a lot of exhilaration but I do want to communicate to you that, I'm a little nervous actually, that within the field of nuclear physics, even though the field is an old one, we're talking about a field where the nucleus was discovered almost a century ago by Rutherford, where 50 years ago it was a hot field.

There is still a lot going on, there is a lot of excitement, actually a lot of exhilaration with the field. And as Ray said, I've been an active participant for 25 years now and I feel that the level of exhilaration based on a few home runs that have been hit is as high today as it has been any time during my career.

So, 15 minutes and I've probably used three of them so I have got to get through a lot in a short period of time. So I am going to cover a different topics, talk a little bit about neutrinos. On this day I should certainly say something about neutrinos, astrophysics and so on. So, in terms of how things have evolved since 1953, obviously the list that I put up here would not have been the list in 1953. There have been some changes.

Nuclear physicists in 1953 predominantly, or nuclear scientists studied finite atomic nuclei. Nowadays we study all kinds of complicated systems, nuclear matter, quark-gluon plasmas and the like. The field has broadened in that sense but, there is still interest and surprise in nuclear physics, nuclear structure, so it stays on the list.

Anyway, the first thing I want to talk to you about was neutrinos. And this is a story that has legs. This is a story that's been going for 70 years. It's a story that is associated with some of the greatest figures of 20th century physics. Down at the bottom corner of this slide here, there is a picture of the beta-decay spectrum, a beta-decay spectrum. This is a problem in that unlike other radioactive decays, rather than there being a discrete energy coming out of the system, it was a continuum spectrum and it was a problem because we believed that energy should be conserved and so ...(inaudible) to go.

And so to solve this problem, Wolfgang Pauli suggested that maybe something else was getting emitted, back in 1932. And Enrico Fermi, who enjoyed the central position on this slide, as he should on any slide related to weak interaction, suggested they call it the neutrino. Now, it remained for 15 or 20 years and abstraction, if you like, a way out of a difficult problem. But towards the end of World War II, Fred Riens(?) who I believed worked on the Manhattan Project started dreaming of maybe one could detect the neutrino.

Now it was expected that the cross section would be really, really low. But it was known that it would be copiously produced in nuclear reactors. And they set up to do that and finally, not long after the "Atoms for Peace" speech, they were able to see neutrinos using from the Savannah River nuclear reactor.

Interestingly, the technique that was developed for that, the ...(inaudible) technique, is still in use today in reactor neutrino experiments. So this was a nice piece of progress in the fifties. And also at that time Madame Woo(?) and her collaborators discover parity validation(?) and various things were going on, but it was noted the nuclear fission reactors were not the only copious source of neutrinos around us. We have a powerful fusion reactor up above and that should produce neutrinos too. And, in fact, the rates are calculable.

And back in 1964, John Bacall, a young theoretical astrophysicist, and Raymond Davis who was a chemist at the Brookhaven National Laboratory proposed an experiment to look for neutrinos from the sun. And the reasoning was two-fold. First of all, let's verify that we really do understand what's going on in the sun in the energy production. And second of all, who knows, if you do an experiment like this, maybe you hit a homerun.

So, over the next two decades, Ray pursued this experiment for which he received the Nobel Prize last year, founding this field and, of course, he is being honored with the Fermi Award this evening. But the experiment went well. They detected solar neutrinos but only at one-third of the expected rate. This created what was known as the solar neutrino problem, "was," because this problem is now solved.

And John Bacall has written, this is of course interesting because there are two ways out of this, either there was something wrong with the sun or there was something wrong with the neutrinos. Actually, there was a third way out of it, which was something wrong with the experiment. And there were not a few people 20 years ago who believed that maybe it was the third option. And for that reason, one really needs to go after this in a serious way and do other experiments.

So, also, in parallel with this in high energy physics, John might talk about but I think he will be doing other things, there were other neutrino experiments going on. And so, about 10 years ago, we knew at the time that there were three neutrino species and not more than that. Their matters were small. Nuclear physicists had measured that in beta decay. And we had this solar neutrino problem. And so over the last decade, a lot of very large, serious experiments have been done to try and resolve the situation.

And I should say that this whole question of mass will be discussed again by Mike Turner. But it is really important because in the standard model a neutrino is mass-less. So, if that mass is non-zero, not only does it break the standard model, or indicate ...(inaudible) beyond it, but also it can be a significant contribution to the mass of the universe. So, basically, people have been looking for the neutrino from all directions, from the sun, from atmospheric neutrinos, from nuclear reactors and since there are many representatives of the nuclear power industry in the audience here, I thought I might remind you of a couple of ways you helped us here.

This is actually a picture of the Sudbury(?) Neutrino Observatory solar neutrino detector. It's a picture looking up from below at a big ball, acrylic ball, which actually at the moment is loaded with a thousand tons of heavy water. And an experiment using a thousand tons of heavy water would have been inconceivable if the Canadian Nuclear Power Program had not used heavy water, the CANDO Program. So there just happened to be a thousand tons available on loan to the experiment from the Canadian government and the value is, I don't know, a billion dollars or something like that. We will be very careful not to mix it with a thousand tons of ordinary water that is surrounding it.

Another experiment, a reactor experiment, has being done in Japan. This is called KamLAND. And, again, I just added this slide today because I thought you might be interested in it. This is an experiment that is sitting under a mountain, the detector, in the center of Japan and it basically integrates the flux from a whole bunch of Japanese nuclear reactors. In fact they had to-- In order to be able to do the experiment to measure the rate, they have to know how much energy each reactor is producing at any given time so they have to sign MDAs to get that information from operators at the plant.

But, anyway, it integrates the signals from Kasuazaki(?) and Takahama(?) and Ohi(?), and all these other reactors. The bottom line is that what happened, the nice thing about this Sudbury experiment, the solar neutrino experiment, is that it has two ways of detecting the neutrinos, one that was just sensitive to electron neutrino as the Davis experiment had and one that could measure all the neutrinos. When they did that they found out that the total sum was exactly right, right bang on with the standard solar model and they agreed with the Davis results on the electron neutrinos.

So, indeed, the right number of neutrinos is being produced, but only a third of them are detectable in our--

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SYMONS: --are changing their flavor. The first evidence of this actually came from that atmospheric neutron experiment, the neutrino experiment in Japan. It is now being confirmed by Snow and by the KamLAND experiment. So, in a sense, the solar neutrino problem is solved. That's a home run for our field. These experiments are sensitive to mass differences between the different kinds of neutrinos. But we still would like to know the absolute mass of the neutrino. So, as we look at the future, there is going to be another generation of experiments.

It's really, really difficult to measure this absolute mass but one hopes to do it with double beta decay experiment, double beta decays, neutrino double beta decays. There's a proposal on the table for us to build a ...(inaudible) to do this and I should comment on why that seems relevant. Because as I pointed out, the pioneering experiments in this field, Ryan Cowan's experiment to measure the reactor(?) neutrinos, Ray Davis' experiment in Home(?) Stake(?), these are all done in the United States. A generation of experiments ...(inaudible) solve this neutrino problem, finding the neutrino mass were all done elsewhere in the world, Canada or in Japan.

When we're looking to the future, we feel that there are great opportunities in this field, but if we want the United States to be a major player, we need a laboratory where we can do this work. You probably read in the newspapers about efforts to create special laboratory in the Home Stake Mine. It is tough. It is filling up with water at the moment. But somewhere or other I hope that in the United States and maybe I'll find a home for such a detector.

By the way, in the bottom left hand corner, this looks just like the advanced nuclear reactor that we were shown this morning, but actually it's a low energy solar neutrino reactor(?), liquid(?) helium(?) ...(inaudible). So, solar neutrinos or the standard model of the sun is the connection between nuclear physics and astrophysics, but it is just one of many places where nuclear physics and astrophysics come together.

This picture is showing you a timeline of various events in the universe. One difference is from 1953, is that in 1953 just drawing such a time line would have been controversial because remember in 1953, you're pre-black body radiation, pre-acceptance of the Big Bang Cosmology, which, I think, we all now completely accept in the universe. But nuclear physics is relevant in all kinds of places from the very birth of the universe to the formation where we believe that there was a quark-gluon plasma to the formation of helium in the early universe, the formation of stars, the burning of stars, explosive events like super Novi, where the heavy elements are formed.

So nuclear physics is needed in many places to understand what's going on in the universe. But sometimes astronomy returns the favor. And one of the biggest discoveries in nuclear physics, I would say in the last 50 years, was the discovery from astronomy, which was fed back into our field. And this was the observation back in the 1960s that is you looked at super Nova remnants-- Like this is a picture. On the top left is the Crab Nebula. You look at them with radio telescopes and in a few cases, somewhere in the remnants you will find a very active periodic pulsing radio source. These are called pulsars.

What they are, or we believe they are, is spinning neutron stars, enormous atomic nuclei. Now to say they were discovered in the 1960s, it is, of course, true that they were experimentally discovered but it is not true to say this is the first time that such a possibility is considered. In fact, way back in the thirties, there had been discussions of possibility that gravity would stabilize very large nuclei.

Experimentally, this forced us to confront this and really introduce the study of nuclear matter into nuclear physics. Because what we are talking about now is the nucleus is not going to have 20 protons and neutrons or 92 or whatever. Here is a nucleus that has ten to the-- I forget. I know the angle of momentum is 1079 units. But I forget-- But the nucleus is basically a kilometer across. It is a giant object.

And if you start thinking about the structure of it, the nucleus is incredibly rich. The pressure gradient goes from zero on the surface to 1014, the density grade is 14 orders magnitude between the core and the outside. There may be phase transitions in there. There may be different species of barium and so on. It has a complicated magnetic structure. So this is a whole new field that is being created inside nuclear physics based on that discovery.

Now when you-- So we know that we're there. We know they have a period. We can get limits on the range of their masses. We can see glitches, things like this-- But the experimental access to neutron stars is limited. It's unfortunate. So it has forced physicists over the last few decades to think there might be other ways to access the properties of nuclear matter in the laboratory.

So this is now moving rapidly to my next topic, which is a diagram of a relativistic heavy ion collision. This is where you are colliding two heavy nuclei in an accelerator, the idea being that in that collision, you would, although it is not going to sit there but for a short, brief moment of time ,you would have highly dense nuclear matter.

We have now an accelerator at Brookhaven National Labs, the relatavistic heavy ion collider built to do this job. It is working very well. There are fantastic results. The bottom right hand corner is a picture, an image from a detector, which is become iconic of our field now, it is the STAR Detector showing the very large number of particles that is produced in such a collision. What we can say at this point is that we certainly have the conditions, we able(?) to meet some of the conditions of temperature and pressure an so on for dense matter, but whether or not we are going into a new phase remains to be seen.

But, at least when we have finite neutrons and neutron stars, these collisions, we can start to speculate on what a phase diagram might look for nuclear matter. We know for H20 the phase diagram has phases, like water, steam, ice, the third phase. What would we expect the phase diagram to look like for nuclear matter? Well, the strong theoretical prejudice or belief that if you go to a high enough density or high enough temperature, the quarks and gluons that are normally locked inside the proton, the sub-structure of the proton, will actually be released. You will have a de-confined phase of matter.

I already mentioned it earlier in my talk when I discussed the state of matter in the very earliest universe. So if you like, there is an arrow coming down on the left, which is a high temperature and low density. But in an accelerator like RHIC, we can make an excursion into that phase and, hopefully, learn something about it.

Another way to look at confinement, is to look at all the different objects you might make with quarks and gluons inside them. This is another main theme in our field. And we have another, almost new, facility, the continuous electron beam facility at Jackson Labs, which does just this and looks for exotic states of quarks and gluons. So we have two enormously powerful facilities that are just in their prime of life.

I’m now getting close to the end of my 15-minute tour here and I come back to nuclear structure, which in some sense you might have felt should have been the first topic on the list. After all, this is classical nuclear physics and in 1953 most people in the field would have been working on this field. And during the last 50 years have been at tremendous amount of progress or interest. We’ve had models of nuclear structures have been now well accepted. There is some connection between these models and more fundamental theories like QCD. We find exotic objects, hyper-deformed nuclei, nuclei with halos of neutrons around them, nuclei-- Everywhere we look there is something interesting going on.

We’ve also made a lot of new nuclei. This is-- You probably see these charts. They come from Westinghouse in the old days or whatever it is, which shows you at any given time, the number of isotopes and elements that have been made. This has three colors on it. The black and white boxes, which are a line moving up from the bottom to the top right are the naturally abundant isotopes. The gray ones are ones that were known in 1953. And the red ones are the ones that have been discovered since that time. Obviously, we have discovered a lot of nuclei and so what?

Well, so what, two things. First thing is when you go and look at those nuclei there is interesting physics to study, new modes of nuclear motion and so on. What, two, is that these are the nuclei that are actually involved in the explosive nuclear synthesis I mentioned that makes the heavy element that makes the material that we're made of today. Fusion reactions and stars only make elements up to iron. If you want to make elements beyond iron, you have to use something else. And the super nova is believed the way to do it, that these are made. These are very neutron rich environments. So basically there is a nuclear reactions that are taking place with these red isotopes or many of them.

So, C-Bath(?) and RHIC are two flagship facilities at the present time. Be able to show you a photograph. For the rare isotope accelerator, I can show you a photograph; I can just show you a diagram because it is something that we would like to build. This is an accelerator that will accelerate these rare isotope beams. This is showing you another nuclear chart but instead of now showing you what the isotopes that have been discovered, this is showing you the ones that will be accelerated or could be accelerated by this accelerator and, also, some of the astrophysical pathways like the R-process and the RP-process.

All I want to demonstrate here is that these lines sit on the nuclei and so while we are making the relevant nuclei with the accelerator and accelerating them and measuring their properties, we hope to tie this all in with a comprehensive theory of super nova formation and nuclear synthesis.

So, I organized this talk in a particularly, an arbitrary way to try to build a bridge from each section to the next. I could have organized it in different ways. This is a picture that organizes the different aspects of nuclear physics that are currently studied as a function of size, of scale. It is a series of islands. We have within our community, sub-fields. We have people who study nuclear structure, their interest in metabody systems, the effective nuclear, nucleon force, people who study fu-nucleon(?) systems at C-Bath, people who want to look at complex systems of quarks and gluons who use RHIC.

And at the top I have listed the four facilities. You can see in blue we have RHIC and C-Bath. These are tremendously productive facilities that run now. We would love to have an underground science lab to do the next generation of neutrino experiments in this country and other experiments also. We'd love to have a rare isotope accelerator to do the next generation experiment in nuclear structure and nuclear astrophysics.

So, actually, I didn't mean to show that one just yet. In closing, I think I would like to say three things. The first thing is, which has nothing to do with the slides I talked about-- The first one is that, we've heard many talks today where nuclear physics is important in the technology we've heard about, whether it's nuclear medicine, national security issues, nuclear reactors and so on. We're clearly going to need nuclear physicists and nuclear chemists in the future.

Now, you can learn a lot of nuclear physics that you need from books. But many of us believe that the way to still train best nuclear scientists is by doing forefront experiments. So we feel it's important to maintain the field just for that reason alone. That's the best way to bring new people into the field. The second point I want to make is the point about applications in the field because sometimes I'm asked about what is the benefit of nuclear physics or other sciences to the common good.

So I made up this slide to make a point here, which is, back in 1952 the year before the "Atoms for Peace" speech, the Nobel Prize in physics was won by Felix Block and Purcell for their studies on nuclear magnetic resonance in solids and liquids. NMR had been discovered previously and Rabi had won the Nobel Prize for it. He did experiments using atomic beams but, if you like, the underlying physics that led to the MRI scanner was the work of Block and Purcell done in the forties. This year the Nobel Prize in medicine was won, and I've forgotten their names, of course, for their discovery concerning magnetic resonance images, essentially turning this physics into a device. It took 51 years between those two Nobel Prizes.

And, of course, the work that was done in developing the scanner was done in the seventies and there were many advances doe there too. But it shows that the time scale to be long and you have to be patient. But there can be a long-term benefit from basic research leading into application. And just to let you know that this particular topic is not over, right now to do nuclear physics experiments at C-Bath, we like to have polarized targets, make polarized helium or xenon, breath it into your lungs, you can make a superb image of the lungs. So that is the little picture down at the bottom there. So there maybe ten years from now you will be going to the hospital and they will be giving you a xenon or a helium bag to breathe in to check the condition of your lungs.

And the third point, the final point, other than just trying to communicate a little bit the excitement in the field is to express our appreciation for the stewardship that the Office of Science and the National Science Foundation give to our field. We've been enormously fortunate, I think, in the enthusiasm of the DOE and the NSF supporting basic science. I hope it goes on. If you want to learn more about the topics I've covered, we wrote a report last year, "Opportunities in Nuclear Science." I'm sure you will be able to get copies from the DOE or you can download it from their website. Thank you very much.

[applause]

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Questions and Answers:
ORBACH: The floor is now open for questions.

QUISH: My name is Alan Quish. I'm a physics professor at the University of Michigan. One important benefit of the Atoms for Peace Proposal was the start of exchange visits between Russian and American scientists and students, especially in high energy and nuclear physics. This seemed an excellent and successful example of what Susan Eisenhower this morning mentioned as her father's hope that the Atoms for Peace Program would build good relations between scientists and students who would later become scientific leaders and help this, to have better relations between the two sides of the Iron Curtain. I think this had worked.

I've been involved in this program since the late 1960s. Unfortunately, this mutually beneficial exchange was significantly reduced when the now expired Department of Energy "Min(?) Atom Peaceful Use of Atomic Energy, Memorandum of Cooperation" was not signed when it was expired in February 2002. Has any progress been made in getting this signed again so this can continue.

ORBACH: I think the question was addressed at me and not the panel. Would any members of the panel like to respond to that? (laughter) Yes there is progress made an, in fact, in today's newspaper you will see one of the reasons why that progress will now accelerate. Thank you.

Are there other questions addressed to the panel? Yes.

GERKY: Bob Gerky, INNEL, to one of the three panelists. We've heard a lot about, where did the water come from on earth. And I'd be curious as to what the latest thinking is. There are some who have said the water on earth has come from comets. Does that hold any water?

ORBACH: Michael?

TURNER: I'm looking for an astronomer or planetary side on my left side here but I don't see one. Well, it certainly came from the quarks (laughter) in the Big Bang and it went through the Big Bang nucleus synthesis. I cannot tell you what the best idea for where the water on earth came from. I can only tell you the early origin of water, which is extremely exciting.

__: Is there any reason that it should have come from any different than all the rest of the elements that we have here on earth?

TURNER: Well, chemistry plays an important role in the formation of the solar system because some of the elements are more volatile than other elements. So, here on earth we don't see the primordial mix. Most of the universe is-- Most of the atoms in the universe are hydrogen. We certainly don't see that here on earth. But the earth's gravity isn't strong enough to hold the hydrogen and the helium, whereas in the sun and in the giant planets it's possible. So, chemistry plays a very important role in what we see here.

ORBACH: Questions? As Chairman, then, I am going to take the liberty of asking my own, which has been driving me nuts for five years. I would like to ask the panel to speculate on just what this dark energy is.

TURNER: Well, I think my son's idea's pretty good.

ORBACH: Are we really that bereft of ideas?

TURNER: No, I think we are at the phase right now where we need a really crazy idea. One of the exciting things about science is that when you get the very toughest problems, they involve some creative break, thinking outside of the box and so I tell this to graduate students and undergraduates and I also put a footnote saying, "Not every crazy idea is a solution to a profound problem. Some of them are just crazy ideas."

The range of things that we're thinking about run from something as mundane as the energy of the quantum vacuum. The problem there is that Jonathan and his friends can't calculate how much the quantum vacuum weighs. When they try to calculate it they get an absurdly large number before they say it must be zero.

(Laughter) It could be-- I describe very, very briefly inflation, speed up in the early universe. Maybe this is a milder form of inflation. And an idea that I really, really like, because it seems crazy enough to be correct is that there is no dark energy; we just don't understand gravity. And that a theme, again, that Jonathan was talking about, was that this marriage between gravity and quantum mechanics will require a modification of general relativity. If you'd ask Jonathan and his colleagues five years ago where that modification would be, they would say, "Oh, it is going to be at very, very short distances. It's not going to affect the cosmos."

But you never know where the clues are going to come from and this could be the clue that tells us about how we have to modify general relativity. And so I think that the solution to this problem that we go back and look at Jonathan's paper and find in page five that there is a two that should have been a 1.5. I think it's that we find something out very profound about matter, space, time and energy.

BAGGER: Mike is completely right about that. I showed a graph, which showed quarks and leptons and so forth, but that is really just a schematic for a whole structure which allows you to do calculations that are tested at experiments to better than a tenth of a percent level. And that whole structure completely breaks down on the subject of dark energy. As Mike was saying, if you use that, basically, you calculate that the dark energy should be infinite and in particle theory if it is infinite, well, maybe it's zero; you missed something.

The fact that it's not zero and it's not infinite, is something that is just completely beyond anything that we can understand and so something brand new has to happen and we just don't have a clue what it is.

ORBACH: Are there other questions?

__: Well, it is sort of a comment. I'm an experimenter so I don't really believe much of anything that can't be measured. There was an article in Scientific American within the last year and I'm bad with names so I forget the guy's name but he proposed a good solution would be to have a very small extra term to Newton's Law that deviated from it at large distances. As far as I know there is no direct experiment that shows that you can't have something which would only cause 1020 deviations from Newton's Law nearby.

And a bunch of my theory colleagues dumped on me and started explaining why that couldn't work but I think it is something to look at.

__: The problem is that the violence that you have to do to the theory has to be consistent with that suite of precisions measurements that you have made so far. And so that imposes constraints. You have to be consistent. Yet, I agree. The answer has got to be crazy. So there is not much wiggle room but there is a hole somewhere and we have to find it.

ORBACH: I think one has to be careful of empirical fits. I mean you can play games with the laws but why, and are the microscopics behind them was my response with I read the article. It was certainly a clever argument but it doesn't answer anything. In the same way that the cosmological constant can change the sign and it gives you expansion, it doesn't tell you where it comes from and that is what these gentlemen have been struggling with. But you actually said something, Michael, that was quite, and, again, Jonathan, quite extraordinary. You believe that the structure of general relativity may be inaccurate.

TURNER: Well, certainly in science we know that in any give point in time, our description of the natural world is just an approximation and what's exciting about the scientific process is that it is never over. Newton wasn't wrong; he just didn't get the whole story. Einstein's theory encompasses-- In science successive theories eat their predecessors whole if we are doing our job right. And so, Einstein just didn't get the whole story. He got a big, big, chunk that we're still trying to swallow. We're still trying to understand black holes and their meaning and we're still trying to understand the Big Bang.

But I think if you took a poll among physicists, I think most of us would say, Einstein didn't get it all. He did not have the last word on gravity and we have more to learn and we're looking for clues and maybe the cosmic speed-up is a clue to tell us which direction to go.

BAGGER: Also it's quite possible, that because ...(inaudible) constant is related to the extra dimension, because it's a question of how our four-dimensional world is embedded in this higher dimensional space, it could be just related to that as well. Again, we don't know.

ORBACH: In the spirit of experiment, can you give us some clues as to how these extra dimensions might actually be observed?

__: Well, in particle physics, everything is a particle. So, actually, if they are the right size, the could be seen as a set of new particles that-- New accelerators like the LHC, or they could be seen through deviations from Newton's Laws and table top experiments, depending exactly on what variety of new dimensions we are talking about or they might be so small that they are only seen indirectly here or there. We don't know.

The great advance in the last few years-- Previously, people thought that these extra dimensions had to be so small that, well, you basically can't see them. But recently theorists have figured out how they can be infinitely large and you still wouldn't know that they're there. And so the story is wide, wide open.

ORBACH: Further questions? Yes.

DOWNEY: Jim Downey, again, at Harvard. And I would have to say I'm not much of a string theorist except for what it involves in tying my shoes. My question is a follow-on to that. It seems from what I have read about string theory that one of the flaws is that it's heavily on the word theory and that experiments to validate it are extremely limited. I'm wondering if we have to find multiple universes or if we can be comfortable at some point with the fact that this may, in fact, be the only universe that ever was.

__: String theory is, at this point, so-- It's so early in the development of string theory that one can't even say, really, what it predicts. How it connects to experiments, we don't know. It's more of also a paradigm at this point, than an actual precise theory with predictions. We'll have to see. Time will tell how it fits in, how it is detected, if it's detected.

__: To comment on your multi-verse possibility, I think what intrigues people are the questions that string theory addresses and the mathematical beauty. Then if you marry string theory with this idea of inflation that I was talking about, you could have had multiple Big Bangs and the rules, what we call the laws of physics, the local bylaws of physics, could be different in the different inflationary events and so the universe could have a structure that is infinitely larger than we can imagine and, if this is so, this would be a breakthrough on the same level of Copernicus getting us out of the center of the universe and the idea that there a multi-verse structure would bring us back down to earth.

As you say, we can't test that yet. So it's this intriguing idea because the hallmark of science is testability. And so I think even you find the string theorists who are desperate to find little ways to test the theory because you have to test these ideas in science.

__: Is that idea giving up this universe-- There are billions of universes and this one is the way it is just because it is? Are we giving up to say that?

__: Well, you're talking about the anthropic principle, which I'm not a fan of--

__: Well, it's related to what you are saying.

__: If the universe has this multi-universe structure that you asked about, and we're very far from saying that, then it's a fact of nature that we have to accept. So, let's wait and see if we have to accept that fact.

ORBACH: There's a book by Martin Reese called The Six Numbers that Determine the Universe, which raises this question, pointing out that these six number, which are the cause for existence, are accurate to an incredible limit for us to exist. And, therefore, why those six, which forms the basis for the second book that he wrote, and I refer you to that.

Burt--

RHICHTER: Burt Richter from Stanford. The whole history of physics is the history of metaphysics turning into physics because of experiments. And right now what we are suffering from is a dearth of experiments because the experiments are getting more complicated, bigger, and more expensive. My poor theory colleagues don't have any data to anchor them and so they are floating in the ether multi-verses and strings and all the rest of that sort of thing.

But sitting up there is one person controlling the budget of the Department of Energy, another person controlling the budget in certain areas of the National Science Foundation and what we've got coming along now are not only things like the LHC, this great accelerator, but we're going to have new telescopes, we're going to have new X-Ray satellites. And I think in the next ten or 15 years, I wish it were faster, we're going to have some new facts and new facts are going to bring some of theory friends back from floating around in the ether to having to make contact, once again, with the real world and then we're going to start to move to a new concept.

__: I hate to dispute. I hate to disagree with Burt Richter. You're absolutely right. We have wonderful possibilities in front of us and we have a plate that is very, very full and we will have a hard time getting everything done. But I think what's very exciting in this connection between the quarks and the cosmos, is that this astronomical fact that the universe is speeding up is not just of interest to astronomers but it's of great interest, as you well know, to your theorists and it may be--

Maybe it's not the clue that they wanted. Maybe they wanted the Hink's particle first but science is always orderly and so we've go other clues coming in, the dark matter, the dark energy. But I take you point on the possibilities before us and finding a way to carry out the experiments and realize our dreams.

ORBACH: I'm going to have Rob Goldsten ask the last question.

GOLDSTEN: Since some areas of particle physics are deferring from making predictions yet and there are these 17 or 19, depending on how you want to count them, basic numbers that are behind the standard theory, we had Roger Penrose come to Princeton to take the occasion to go after string theory as being irrelevant. That was kind of an interesting experience at Princeton. But I asked him this question and his answer-- And I'm curious how your response is, what your answer is-- Since we can't have a prediction, how about a meta-prediction, so to speak, a prediction about the predictions. When will one of these numbers come out of string theory? The first one?

[pause]

ORBACH: Would you like me to predict when Jonathan will answer this question? (Laughter)

__: So to speak, a meta, meta-prediction.

BAGGER: To be honest, I'm not a string theorist. I'm backpedaling fast. I actually believe in effective field theory. I'm more like a condensed matter physicist, mucking around with my effective Hamiltonian of the standard model and I can look further and see that either great things coming in the next factor ten in energy. To get all the way to the string theory scale, is more than I can imagine. But I can use string theory for inspiration and to take the big picture of string theory and use it as a guide, but actually detailed calculations, it's like trying to drive, perhaps chemistry from first principles of quarks and leptons. There are many steps in between. It may be very hard.

ORBACH: As one of those guys who mucks around with materials, let me thank the members of the panel and all of you in the audience. I think Burt Richter's plea, that this will come faster than 15 years, is upon us. We have four to five more years of operation of Fermilab. These issues, these new particles, the experiments that have been called for may well emerge from that.

One has, as you saw the large hadron collider at Cern, the possibility of the linear collider in parallel. We have in front of us machines and theories that address the very fundamentals of our existence. It’s an exciting time to be alive and I thank you all for joining us this afternoon.

[applause]

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