# RECENT DEVELOPMENTS IN FUSION ENERGY RESEARCH

## HEARING

BEFORE THE

U.S. CONGRESS, HOUSE COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES

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# RECENT DEVELOPMENTS IN FUSION ENERGY RESEARCH

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WEDNESDAY, APRIL 26, 1989

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
*Washington, D.C.*

The Committee met, pursuant to call, at 9:45 a.m., in room 2318, Rayburn House Office Building, Hon. Robert A. Roe [Chairman of the Committee] presiding.

## OPENING STATEMENT OF HON. ROBERT A. ROE, CHAIRMAN, COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

The CHAIRMAN. The Committee will come to order. We want to welcome everyone to this hearing this morning.

As the first order of business, without objection, permission is granted for coverage of this meeting by television, radio, and still photography. If there is no objection, so ordered.

Good morning, ladies and gentlemen.

In recent weeks, an atmosphere of high excitement and anticipation has permeated the scientific community as startling possibilities for sustained nuclear fusion reactions at room temperature have emerged. The potential implications of a scientific breakthrough that can produce cold fusion are, at the least, in our judgment, spectacular.

At the heart of this excitement is a collaborative experiment conducted by Professor Stanley Pons of the University of Utah and Professor Martin Fleischmann of the University of Southampton in England. Experimental work took place on the Utah campus, and the announcement of results first came on March 23rd. This announcement preceded the traditional submission to a scientific journal where the article would be reviewed by other researchers in the field. Since March 23rd, researchers around the world have attempted to duplicate the experiments of Dr. Pons and Dr. Fleischmann with conflicting results, at least as reported in the press.

Our objective in holding this hearing today is to examine the various developments to date to allow an interchange among experts with differing views and to help Members of the Science, Space, and Technology Committee assess the significance of the current information.

The harnessing of fusion energy for eventual commercial use has been an illusive dream for decades, as we all know. The United States as well as other industrial nations have spent millions of dollars to fund various experimental approaches to generating sustained fusion energy. All of the efforts to date have required both very expensive machines and extraordinary temperature levels. The possibility of creating fusion energy at room temperature was wishful thinking only a few short months ago. Today, I believe we all have regained new hope.

The hope of producing commercial fusion energy is the hope of an energy-hungry world and the need of an energy-hungry world. Energy, as we all know, is the life's blood of mankind's technological society. The Middle East has over one-half of the world's known oil resources and one-quarter of global natural resources—reserves, that is. The United States has a quarter of the world coal reserves and 4 percent of global oil reserves and 6 percent of world natural gases. What is perhaps most wondrous is that those facts may be superseded by man's intellectual inventiveness and dogged curiosity.

Today, we may be poised on the threshold of a new era. It is possible that we may be witnessing the cold fusion revolution, so as to speak. If so, man will be unshackled from his dependence on finite energy resources.

We are extremely pleased we have assembled here the two professors who may have discovered cold fusion and certainly have brought great excitement to the scientific community and to the world. Additionally, we have with us today several recognized experts in the fields of fusion energy research and materials research from across the country, and I want to thank all of them for adjusting their demanding schedules to be able to appear before us on such a very short notice.

Without further comment from me, I would at this time recognize our distinguished colleague and good friend and ranking Member, the distinguished gentleman from Pennsylvania, Mr. Robert Walker.

## OPENING STATEMENT OF HON. ROBERT WALKER, RANKING MINORITY MEMBER, COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

Mr. WALKER. Thank you, Mr. Chairman.

In a period when our news seems to be filled with items telling us about drugs, budget deficits, the decline in America's economic position and environmental problems, the news of the possible discovery of cold fusion in Utah, even with its accompanying controversy, was wonderful news.

If this discovery is fully proven, it will show once again the importance of supporting a vigorous small science enterprise in this period of large engineering and science projects. The possibility that cold fusion can make energy with little or no radioactive byproducts makes the prospects of this discovery even more exciting.

If the initial results are verified, it is essential that we do everything we can to develop the promise of cold fusion. In the process, we must ensure that this Nation does not lose out in reaping the potential economic benefits.

That is why I was pleased that the Committee's Energy Research and Development Subcommittee accepted my amendment during its April 6 markup authorizing that $5 million be redirected from the Magnetic Fusion Program into the Basic Energy Sciences activity specifically for room temperature fusion. It is my understanding since that $25 million may be a more realistic kind of figure for this Committee to be committing to. I am fully supportive of that and hope the Committee will move in that direction. With this time and amount put together with monies from the State of Utah, from industry, and other places, I think we can make a real commitment to the effort, and that is the very least we should be doing in a time when great promise is being shown.

The discoveries at the University of Utah, if verified, will take a giant step toward realizing a major national goal at greatly reduced cost and with much less difficulty. I wish to congratulate Dr. Pons and Dr. Fleischmann for their hard work, and I will look forward to hearing more about their efforts.

Thank you, Mr. Chairman.

The CHAIRMAN. The Chair thanks the distinguished gentleman from Pennsylvania and recognizes the distinguished gentlelady from Tennessee, the Chairman of the Energy Research and Development Subcommittee, the Honorable Mrs. Lloyd.

## OPENING STATEMENT OF HON. MARILYN LLOYD, CHAIRMAN, SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Mrs. LLOYD. Thank you very much, Mr. Chairman.

I, too, am pleased to participate in this very important hearing today. My Subcommittee on Energy Research and Development often deals with issues on a reactive nature, such as how to keep disasters like Chernobyl and Three Mile Island from occurring, how to clean up nuclear waste, and how to deal with acid rain and the greenhouse effect. So I can tell you that it is indeed a pleasure to be here today to receive testimony on cold fusion. Your presence here today proves that good news can make headlines.

We are looking forward to advances in this exciting new field of fusion energy research, and I am sure it comes as no surprise to our witnesses that the race to confirm their claim is highly competitive. Scientists all over the world are now trying to duplicate the power of the Sun because of your experiments. Hopes of developing this power that runs on limitless sea water and leaves virtually no dangerous radioactive waste have put research projects all over the country on hold.

I, myself, am particularly interested in the efforts of a group of seven chemical technologists and eight nuclear engineers in Oak Ridge, Tennessee. The scientists at the Oak Ridge National Laboratory have been in the forefront of nuclear fusion development by more traditional methods for many years now, but they are thrilled, and we all are, by the scientific ramifications of your experiment.

Gentlemen, the world awaits the crucial details of your amazing claim. The amount of energy that your experiment produced is larger than any chemical reaction that we know can justify, and the production of so much heat energy without a corresponding number of neutrons is novel, to say the least. We all want this to work.

Energy is the lifeblood of our Nation, and fusion energy would be an enormous step toward the goal of energy independence. You can certainly be sure that my Subcommittee is supportive of new initiatives along with the rest of the Nation, and I am hopeful that today's revelations will move us toward a new era in fusion temperature.

Thank you, Mr. Chairman.

The CHAIRMAN. I thank the distinguished gentlelady.

The chair recognizes the distinguished gentleman from New York, Mr. Scheuer.

Mr. SCHEUER. Mr. Chairman, we have an enormous showing of Members today, a baker's dozen at the least. If each of us takes five minutes, it will be over an hour before we get to the distinguished witnesses. So I would ask unanimous consent that Members from here on in be restricted to two minutes.

The CHAIRMAN. Is there any objection to the unanimous consent request? None is heard. The unanimous consent is agreed to.

The chair recognizes the gentleman from Pennsylvania, the distinguished Mr. Ritter.

## OPENING STATEMENT OF HON. DON RITTER

Mr. RITTER. Mr. Chairman, I think I object.

The CHAIRMAN. Too late. You've got two minutes.

Mr. RITTER. Thank you, Mr. Chairman, for holding this important hearing.

A simple tabletop experiment with heavy water and electrical current has caused excitement to ripple through the American scientific community. The discovery of cold fusion may turn out to be historic, and "fusion in a flask" may, in the longer term, prove to be a cheap, meaningful source of energy.

I want to emphasize the fact that the money that went into supporting this research is minuscule in comparison to the kind of large-scale mega-projects we have been dealing with over the years here.

A scientist at Oak Ridge said on Monday, "We don't know whether we're doing the right experiment, but we're willing to come in at midnight and do it." What that tells me is, there's a lot of energy being harnessed in pursuit of cold fusion and a lot of energy is out there already. This is the dogged persistence of some of America's best and brightest scientists.

If cold fusion works, we would be remiss if we didn't do everything within our means to encourage its long-term development, but we need to know more. Is it indeed a nuclear reaction that's occurring? Does the process produce enough energy to make it viable in a scaled-up version? And then there's the would-be questions about our existing nuclear fusion program. There are so many other questions, but, in fact, it may be too early to ask them.

"The most beautiful thing we can experience is the mysterious," Albert Einstein said in 1930. But bringing these mysteries into the home, into the marketplace, and into the production line demands long-term commitment, not just fanfare at the moment. We shouldn't forget that we sat here not too long ago and felt the intensity generated by another revolutionary breakthrough: high-temperature superconductivity. But the Gomery report, which tells us how to follow through on the initial excitement over the long term, is still sitting on the shelf awaiting implementation.

I've used the phrase "long-range" and "long-term" several times, Mr. Chairman, because it is only with patience, commitment, and investment that our scientific discoveries become part and parcel of our lives, not just fascinations and Nobel Prize-winning efforts for the scientific community.

In recent years, we have been more successful in providing the world with scientific breakthroughs, but our competitors have been often more creative in putting that science to large-scale commercial use.

Mr. Chairman, I commend our witnesses for participating in this exciting quest. I welcome them, and I look forward to their testimony.

Thank you.

The CHAIRMAN. I thank the gentleman from Pennsylvania.

The chair recognizes the distinguished gentleman from New York, Mr. Scheuer.

## OPENING STATEMENT OF HON. JAMES SCHEUER

Mr. SCHEUER. Thank you, Mr. Chairman.

Mr. Chairman, the prospect of having a world in which we have an unlimited new, cheap, and clean source of energy is almost unbearably exciting. I think we are all transported here with the potential of not having to worry about the polluting effects of burning fossil fuels.

Mr. Chairman, we have had scientists appear before this Committee and, because of acid rain, global warming, the greenhouse effect, et cetera, et cetera, they have told us that the world has got to stop burning fossil fuel. Now that may not be a very practical prospect, but it shows how serious are the environmental problems which plague us.

If we can find this new, clean source of energy, it would be a godsend of unimaginable proportions. But I must say, Mr. Chairman, the process so far by which we have learned about this has been more confusion than cold fusion, and there seems to be a feeling about that the process has been more driven by a wish to protect future potential profits than it has been adherence to normal peer review processes, and I hope that we'll dispel that this morning, and I hope that we'll get to prove the scientific peer review process that will be liberated to assure us that this is actually real, which we all hope, and pray, and dream that it is.

Thank you, Mr. Chairman.

The CHAIRMAN. The chair recognizes the distinguished gentleman from Washington, Mr. Morrison.

## OPENING STATEMENT OF HON. SID MORRISON, RANKING MINORITY MEMBER, SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

Mr. MORRISON. Thank you, Mr. Chairman.

I think what Mrs. Lloyd, our distinguished Chairman of the Energy Research and Development Subcommittee, had in mind was sort of a friendly fireside chat with these distinguished gentlemen, and we are proud to share this with, Mr. Chairman, not only the Full Committee but, obviously, a tremendous level of attention that is virtually world-wide.

I would suggest that maybe our interest on this committee this morning will be, first of all, how to verify; second, how to multiply; third, how to apply, these findings to the problems we face in this Committee.

Thank you, Mr. Chairman.

The CHAIRMAN. I thank the distinguished gentleman.

The chair recognizes the distinguished gentleman from California, Mr. Packard.

## OPENING STATEMENT OF HON. RON PACKARD

Mr. PACKARD. Thank you, Mr. Chairman.

Today, our national energy sources are inadequate at best, and our dependence on imported oil is not acceptable. Domestic oil exploration is fraught with many problems, and the burning of coal and natural gas causes world-wide environmental concerns. Cold fusion, a virtually inexhaustible fuel source, could certainly be the answer to these concerns.

This cold fusion process also holds the promise for ending our nuclear waste problem. We are told that we could be on the threshold of a discovery that would potentially revolutionize power generation as we know it today. We also realize that further experimentation and scientific proof is necessary to validate this discovery of cold fusion. Yet, despite the uncertainties, we Members of the Science Committee share the enthusiasm and the hope of the scientific community for the success of this apparent breakthrough as we deal with environmental and energy issues which threaten our Nation and the world.

Congratulations are certainly due to both of our distinguished witnesses and all of our witnesses and to you, Mr. Chairman; I certainly thank you and commend you for holding this important hearing.

The CHAIRMAN. I thank the distinguished gentleman from California.

The chair now recognizes the distinguished gentleman from New Mexico, Mr. Schiff.

## OPENING STATEMENT OF HON. STEVEN SCHIFF

Mr. SCHIFF. Thank you, Mr. Chairman.

Mr. Chairman, I am the newest Member of the Energy Research and Development Subcommittee, having just been elected to the Congress, and I've been impressed by the large amounts in terms of dollars that the Committee authorizes for fusion research, in inertial fusion, and in magnetic fusion. It therefore came as quite a surprise to me of news reports of fusion occurring in a tabletop experiment. I want to observe, however, that I am sure many of the greatest scientists in this country and the world were able to achieve breakthroughs without large Federal grants in the past, and therefore I'm looking forward to hearing from these witnesses and from the other panelists today.

The CHAIRMAN. I thank the distinguished gentleman.

The chair now wants to recognize two of our distinguished representatives from Utah who are accompanying our first panel, and first I would recognize our colleague from the Second District in Utah, the Honorable Wayne Owens, for some opening comments and to introduce his witnesses.

## STATEMENT OF HON. WAYNE OWENS, THE REPRESENTATIVE IN CONGRESS FROM THE SECOND DISTRICT OF THE STATE OF UTAH

Mr. OWENS. Thank you, Mr. Chairman.

I would like to hold my comments until after the second panel and simply introduce, if I could, the panel that is here this morning.

The CHAIRMAN. Well, before you do that, do you want to hear Mr. Nielson first?

The chair then would recognize Mr. Nielson, also from the great State of Utah, the Third District.

## STATEMENT OF HON. HOWARD C. NIELSON, THE REPRESENTATIVE IN CONGRESS FROM THE THIRD DISTRICT OF THE STATE OF UTAH

Mr. NIELSON. Thank you, Mr. Chairman.

All three Members of the House from Utah are graduates of the University of Utah and very proud of the University of Utah and for the efforts they have made. I also taught at BYU for 25 years, and I'll be introducing Dr. Steven Jones from Brigham Young University, who has been doing a lot of work in the fusion area, later.

My interest in the commercial aspects has already been mentioned pretty much by the Committee, and I'll not take time to read my statement, I'll submit it for the record, but I am happy to be here in support of the cold fusion research in Utah both at BYU and the University of Utah and wherever else we can make a good effort there.

The CHAIRMAN. If there's no objection, the gentleman's full statement will appear in the record at this point.

[The prepared statement of Mr. Nielson follows:]

STATEMENT OF THE HONORABLE HOWARD C. NIELSON

BEFORE THE SUBCOMMITTEE ON ENERGY RESEARCH AND DEVELOPMENT

WEDNESDAY APRIL 26, 1989

MR. CHAIRMAN:

I APPRECIATE THE OPPORTUNITY TO PARTICIPATE IN THIS COMMITTEE'S HEARING TODAY REGARDING THE UNIVERSITY OF UTAH'S RECENT CLAIM OF SUCCESSFULLY SUSTAINING A NUCLEAR FUSION REACTION AT ROOM TEMPERATURE.

WHEN DOCTORS PONS AND FLEISHMANN MADE THEIR ANNOUNCEMENT JUST A FEW WEEKS AGO, THE WORLDWIDE SCIENTIFIC COMMUNITY BECAME EXCITED ABOUT THE HOPES AND DOUBTS OF SUCH A DISCOVERY.

OF COURSE, AS A UTAHN, I HAVE BEEN PARTICULARLY INTERESTED IN THE UNIVERSITY OF UTAH'S EXPERIMENT, AS WELL AS THE RESEARCH OF DR. STEVEN JONES OF BRIGHAM YOUNG UNIVERSITY, WHICH IS IN THE HEART OF MY DISTRICT AND WHERE I TAUGHT FOR 25 YEARS. DR. JONES HAS BEEN A LEADER IN NUCLEAR FUSION RESEARCH FOR A NUMBER OF YEARS AND HAS ALSO HAD SUCCESS WITH A DIFFERENT, YET SIMILAR EXPERIMENT.

MY INTEREST IN THE COMMERCIAL PROSPECTS FOR NUCLEAR FUSION ARE HEIGHTENED AS A RESULT OF MY ASSIGNMENT ON THE HOUSE ENERGY AND COMMERCE COMMITTEE. RECENTLY THERE HAS BEEN A GREAT DEAL OF COMMITTEE ATTENTION AND INTERNATIONAL ATTENTION FOCUSED ON ENVIRONMENTAL PROBLEMS SUCH AS ACID RAIN AND GLOBAL WARMING THAT ARE ASSOCIATED WITH THE USE OF FOSSIL FUELS. NOT TO MENTION FEARS OVER THE DISASTROUS ALASKAN OIL SPILL, OUR NATION'S INCREASING ENERGY CONSUMPTION AND THE RISKS OF OVER DEPENDENCE ON FOREIGN OIL.

COMMERCIAL APPLICATION OF NUCLEAR FUSION WOULD CERTAINLY PROVIDE A PANACEA FOR SO MANY OF OUR ENVIRONMENTAL

PROBLEMS WHILE MEETING OUR ENERGY NEEDS WITH A VIRTUALLY UNLIMITED, SAFE ENERGY SOURCE. WE ALL HAVE REASON TO HOPE THAT THESE RECENT EXPERIMENTS PROVE COMMERCIALLY FEASIBLE.

WHILE I UNDERSTAND THAT STANFORD UNIVERSITY HAS REPLICATED THE UNIVERSITY OF UTAH'S EXPERIMENT, I ALSO UNDERSTAND THAT OTHER RESEARCHERS HAVE NOT BEEN ABLE TO ACHIEVE THE SAME RESULTS AND THE SCIENTIFIC JURY IS STILL OUT ON THE SUCCESS OF THE EXPERIMENT AND IF IT WORKS, WHY IT WORKS.

I LOOK FORWARD TO TODAY'S TESTIMONY AND I THANK YOU AGAIN FOR THE OPPORTUNITY TO BE WITH YOU HERE TODAY.

The CHAIRMAN. The chair again recognizes Mr. Owens, our colleague from Utah.

Mr. OWENS. Mr. Chairman, Members of the Committee, we have before us an extraordinary event and, I think, an extraordinary opportunity. The event, the possible achievement of solid state fusion, or the so-called cold fusion, is nothing less than a miracle with all the elements of a miracle—surprise, exhaltation, disbelief, and skepticism. The opportunity could also present itself as a miracle of accomplishment, a chance to preserve in America an American-born technology that will change the face of the earth.

I would like you to hear the story first-hand from the people who created the story, two distinguished scholars, Dr. Stanley Pons and Dr. Martin Fleischmann. They will each have a written submission, Mr. Chairman, which we would ask that you would include in the record.

The CHAIRMAN. If there is no objection, it is so ordered.

The chair recognizes the distinguished gentleman from the great State of Utah, Dr. Pons—Dr. Stanley Pons.

Welcome.

## STATEMENT OF STANLEY PONS, PROFESSOR, DEPARTMENT OF CHEMISTRY, UNIVERSITY OF UTAH, SALT LAKE CITY, UTAH

Mr. PONS. Chairman Roe, first I would like to thank you—

The CHAIRMAN. Now, of all the miracles of our time, the one thing about those microphones is, they are not that good, so you have to pull them closer.

Mr. PONS. Okay. Thank you very much.

Chairman, first we would like to thank you and the Committee for the opportunity to testify here today.

I might start by saying, while discussing new research problems with Martin Fleischmann in 1984, as we usually do, the problem of high-energy or high-pressure electrochemical phenomena was considered. We knew that measurements of hydrogen—the concentration behavior of hydrogen which had been placed in two certain metal lattices by electrochemical means indicated that if one were to try to duplicate these processes by hydrostatic means—in other words, hydrostatic pressures—then it was clear that enormous, almost astronomical, pressures would have to be applied.

So this indicated to us the possibility of many new areas of research, such as hydrogen storage implications or new synthetic methods, new chemical synthetic methods. The most intriguing implication was the possibility that under such high energy conditions it indeed might be possible to fuse light atomic nucleii, a very unlikely situation, but certainly the science seemed to be there to implicate that.

A simple experiment was then designed and started, and a few months later a result was obtained in Utah that convinced us that we might, indeed, have demonstrated a nuclear reaction, and one of the present experimental devices that we used is here in front of you, and if I might use some slides I would like to show you a schematic of this diagram.

The CHAIRMAN. Yes, of course.

[Slides shown.]

Mr. PONS. The cell itself, as we are now using them, is a glass cylinder, a test tube, if you like, except this particular one shows a double wall so that we may control the amount of heat, the rate at which heat is transported in and out of the device.

This square block in the center represents a palladium rod of metal at which the reaction occurs.

Other devices in the cell are this item here, which is a resistive heater so we can—so that we can place known amounts of heat—inject known amounts of heat into the device; this is for calibration purposes. We also have a thermister, which is a device which measures temperature inside the cell. We would have a similar device outside the cell so we can measure the temperature in the surrounding bath, and we go to great care to keep the outside of the cell at a very constant temperature.

Other devices are a reference electrode which may be placed in the cell and—let's see, I think I've mentioned all of the other—and an outlet to put materials in and out.

Oh, yes, and of course we need two electrodes to run an electrochemical reaction, and so this—we just lost our light—no. These wires running across these represent a platinum anode, which is the other electrode in the reaction, so that current passes between that electrode and the center palladium electrode.

If we could have the next slide.

The reaction that is considered is the reduction of heavy water at this palladium electrode. This line represents the metal palladium electrode, so we can imagine that this is the metal on the right-hand side. There's a short in this device here. So the reaction—this sequence here represents the supposed mechanism for the reduction of water at a palladium electrode, and B2O represents heavy water. An electron is pumped into this water, across the electrode solution interface, and the first product that is formed is quite well known, is atoms of deuterium that then are attached to the outside surface of the palladium metal, and you also release the base, the hydroxide ion in this case, the deuteroxide ion.

The next process which is known to occur—and this is a strange process, and it has been known for many years—is the dissolution or the diffusion of this absorbed atom of deuterium inside the metal lattice. So this equation here represents the deuterium—absorbed deuterium that are moving into the metal from the—from the surface.

Now there is a competing reaction, or a series of competing reactions, and that is that these surface atoms may also either recombine or further react through another electron transfer reaction to give you deuterium gas. This is the hydrogen evolution reaction—again, a well studied and quite well known reaction.

So we have a competition between these two reactions, and the activity or the concentration of the deuterium which ends up inside the lattice is a function of the relative rates of the first two reactions. If you can decrease the rate of this last reaction as opposed to the first, then, of course, you would increase—or you would, indeed, intend to increase the concentration of the deuterium, or the activity of the deuterium, inside that lattice.

The next slide, please.

So this, again, is another cartoon showing that process. We have the atom then moving inside to this metal lattice.

Now upon absorption it is well known that indeed these atoms diffuse in. The strange thing that happens: there is convincing evidence that the single electron that is associated with this absorbed atom is given up to the metal lattice—that is that the deuterium atom on the surface moves into this metal lattice as an ion. In other words, we end up having a low temperature plasma of deuterium inside the metal instead of atoms or molecules of deuterium.

The next slide.

A further measurement that has been made many times and by many different, or by several different methods, is that you can measure the potential or the activity, the chemical potential, if you like, of the species inside and outside of the metal lattice, and, like I said, this has been done by a variety of techniques; and you measure the potential of the species inside and outside of the chemical—the difference in the chemical potential as about .8 volts. While this is not a very large voltage—if you think in terms of a battery, for instance, that would not be a very large voltage, but it has very strong implications, or the implications of this measurement become enormous if we think what we would have to do to recreate the same situation in a chemical sense.

On the next slide, we point out that if, indeed, you would try to—if you were to try to obtain that same voltage by the compression of hydrogen gas to get that same chemical potential of .8 volts, you would have to exert a hydrostatic pressure of a billion, billion, billion atmospheres, tremendously high pressure.

And, further, we see—or the point here is that also these pressures—or certainly these pressures, absolute hydrostatic pressures, are not attained inside the metal lattice. The dissolution of this material, these atoms going to these ions inside the lattice, represents a very high energy process, and it is not very well understood.

We further note that we never observed deuterium gas in the lattice, which means that there is little driving force to form that material. That indicates that indeed the deuterons, these ions inside the lattice, are quite well shielded by the electrons in the lattice, and this has certain other implications which we will discuss a little bit later.

The CHAIRMAN. Doctor.

Mr. PONS. Yes.

The CHAIRMAN. We will have to suspend at this point. They are on the second roll call to vote. We will vote and return immediately.

[Recess.]

The CHAIRMAN. When we recessed, we had to go vote, and Dr. Pons was in the middle of his explanation. Suppose we get our charts back up. Back up one chart, and go on from there.

The chair recognizes Dr. Pons.

Somebody just pulled the plug.

Mr. PONS. Professor Fleischmann just remarked that we don't—that this may happen in the twenty-first century if we are not careful.

The CHAIRMAN. We've got to be careful of this.

Dr. Pons, go ahead.

Mr. PONS. Okay. Well, just to summarize what I've been saying, it appears as though—that inside this metal lattice the presence of this deuterium plasma inside this metal lattice gives rise to a condition of very high compression by the energetics, high mobility due to the shielding, the possibility of many collisions because of the high number of the deuterons inside this lattice, and very long confinement times—we calculate on the order of 600 years. So we have then the possibility under those conditions—the possibility of nuclear fusion.

The next slide, please.

Now the normal reaction which one would consider then would be the fusion of deuterium deuterons inside the lattice, and, as is well known in physics, this normally proceeds at high temperatures and high-temperature plasmas to give a tritium atom and a proton plus energy release or, by another possible branch approximately equal to the first one, helium-3 and a neutron, again with the emission of large amounts of energy.

Now our evidence for these reactions, for this normal branching reaction, have been the measurement of very low levels of neutron flux. We had some masspectroscopic data which did not pan out to be very pertinent to the experiment, although we're now using it for other investigations of other materials that may be involved; the gamma ray radiation associated with this neutron coming out of the material and reacting with water, or heavy—I'm sorry—light water in the surrounding water bath; tritium measurements of this—tritium measurements of the accumulation of this atom in the phase outside as it exchanges with deuterium on the surface to give DTO, an isotope of water; and, lastly, the calorometric measurements, which I'll—Martin and I will discuss in just a moment.

The next slide shows the evidence for the tritium. This is a beta ray spectrum of the solution. We extract some of the solution after it has been in contact with the electrode for some time, and we find that this spectrum appears. This spectrum represents a fingerprint, if you like, of the presence of increasing amounts of tritium in the heavy water solution adjacent to the electrode. We also notice that this grows in time to a limiting value, and from that we can make judgments as to how much and how fast the nuclear reaction is taking place.

The next slide shows the gamma ray spectrum which is expected from the reaction of neutrons coming out of that metal lattice reacting with water in the surrounding water bath. The neutron reacts giving up—with the water, giving up a gamma ray, which is measured in that surrounding bath.

Also, using dosimeters, we have measured a neutron flux which corresponds to about 10 to the fourth neutrons per second.

The next slide, please.

This table—while we do measure very low levels of these nuclear reaction products, we make a much more significant measurement, and this is our calorometric data. The table shows several important results.

First of all, excess heat is liberated in the reaction. We measure more heat coming out of the electrode, more heat coming out of the cell, than is injected from the outside by the power supplies, by the current that is passed into the electrochemical cell.

Number two, the heat arises from a process inside the electrode. These data here, for instance, show that the reaction, the quantity of heat that comes out, takes place inside the electrode and not on the surface of the electrode. That is evidenced by the fact that the quantity of the heat depends on the volume of the electrode and not the surface area of the electrode.

And, number three, the values that we attain, for instance, in this last column, or these last columns over here, the intensity of this heat, the excess heat liberated, is of such a magnitude that it cannot be explained by any chemical reaction. The heat generation continues indefinitely until the cell is turned off, and it is a constant excess heat under the conditions measured here.

And before I let—before Martin Fleischmann continues with this presentation, if you would allow us, I would like to say that if we try to explain the magnitude of the heat by the conventional deuterium deuterium reaction, which I showed a couple of slides ago, we find that we have 10 to the ninth times more energy from these thermal measurements than that represented by this neutron and tritium that we observe.

So apparently there is another nuclear reaction or another branch to the deuterium deuterium fusion reaction that heretofore has not been considered, and it is that that we propose is, indeed, the mechanism of the excess heat generation.

If I could, I would like to let Martin Fleischmann continue at this point.

The CHAIRMAN. The chair recognizes Dr. Fleischmann.

## STATEMENT OF PROFESSOR MARTIN FLEISCHMANN, UNIVERSITY OF SOUTHAMPTON, ENGLAND

Mr. FLEISCHMANN. Mr. Chairman, Professor Pons has given you the essential experimental details. Let me just point out—summarize that again and then carry on.

The investigation really falls into two parts: the measurements of the heat and the measurements of the expected products of the nuclear reaction in the tritium and the neutrons; helium-3, the evidence is ambiguous; but we do see also the gamma rays which we expect from the neutrons.

Now the measurement of these products of the nuclear reactions are extremely interesting in themselves, and many people are preoccupied with that particular problem on its own. It's really a problem in physics.

From our point of view, though, we have been more interested in the heat release, and it is that quantification of the heat release and the establishment of the conditions for the heat release which is really the social side of our research, and I will tell the Members of the Committee that the social considerations have, of course, been very much in our mind.

Now the experiment which Professor Pons has described to you is superficially simple but is actually quite difficult to carry out, because you have to go through a process of optimizing the experiment such that you will make a significant observation.

He has really given you the essential observations, and what I would just like to do in the few moments I have here before you is to carry out some speculation about the nature of the results and to try and project these results into the future and make some point of comparison with the more conventional approaches to nuclear fusion which have been researched so well and so far.

Now the experiment design which you see in front of you here and which you also saw in the slide has really been developed to measure the heat release. It is not an experiment designed to optimize the energy production, the yield of energy, with respect to the energy input.

So if I may just speculate for one moment and show you the next slide, if you were to try and do this experiment in a more sensible way—the next slide, please, and lower the lights, please—you would—you might do it in some fashion such as that, that you had some type of palladium electrode in which you carry out the compression, which Professor Pons has been speaking about. The gas would be taken away from here, pumped into something like a fuel cell anode, where the deuterium generated in the closed system would again be ionized, so that there is no production of oxygen in the system.

This would be a much more energy-efficient device and underlies one of the results which I'll show you—one of the sets of calculations which I'll show you on the next slide and is really a hypothetical energy release—I'd stress that—because it involves the recalculation of our data to project them to the condition where we can use a closed system rather than evolving the gas here and generating oxygen at the positive electrode.

Well, let us look at the next slide, please, and this table of figures now contains three sets of figures. The first is the excess energy which we are able to generate as a function of the size, the diameter of the electrode going in factors of 2 here; these are rod electrodes, 1 millimeter, 2 millimeter, 4 millimeter in diameter, 10 centieeters long, polarized at different current densities; that's the current per unit area, 8, 64, 512—there are special reasons why we have chosen such odd numbers—and here is the excess energy generated.

As ygu see, it actually—it increases markedly with electrode diameter, and this is for the condition where oxygen—where we would actually continuously decompose the heavy water.

The second set of figures here relate to the condition where the energy is expressed as a percentage of the total energy supplied to the cell, and the third set of figures relates, in fact, to the condition of our speculative hypothetical cell in which we do not generate oxygen at the anode.

You will see that as we increase the diameter, we can, in fact, get factors of 3, 4, and 8 under this limiting condition—let's just focus on one set of conditions—about half an ampere per square centimeter. A 4-millimeter electrode giving about eight times as much energy out—would hypothetically give about eight times as much energy out as we put in. But let me stress, that is a projection of our figures, but in fusion research there has been so far—naturally, you have to project to a viable technology. This is part and parcel—has been part and parcel of fusion research so far, so I think we are justified in making such a projection ourselves.

Well, let me just contrast this with—take the next slide and just contrast this with existing fusion research.

I think this figure will be familiar to Members—some Members of the Committee. It is what is known as a confinement parameter diagram where existing fusion—high-temperature fusion research—incidentally, let me correct one statement which has been made here today that room temperature fusion is not confined to the experiment which we have carried out. It is, of course, well known also in the field of muon catalyzed fusion, which perhaps Professor Steven Jones may be able to tell you something about this afternoon. But I'm confining my—making my comparisons with high-temperature magnetic confinement—results from magnetic confinement, not with inertial laser confinement.

Well, in the high-temperature research, the plasma, the aim, the objective, is to raise the energy of the particles in the plasma to the order of 10 to 100 kilo electron volts. One electron volts is the energy which is attained by a particle when it drops to a potential gradient of one volt. If you like, you can convert that into a temperature. We are talking about temperatures of 100 to 1,000 million degrees centigrade because we need that energy in the plasma to overcome the repulsive energy of the positively charged particles, and it is know that we have to get into this corner here, and the other axis here is the confinement parameter, which is really the particle density, particles per unit volume multiplied by the time, and notice, please, here that the objective here is to get to about 10 to the 14, 10 to the 15. Here are the results for the Joint European Taurus, which is probably about as close as people have got with this particular type of fusion research.

On here is a loop that is break-even. At that point, the system would be giving out as much heat as is put in but neglecting the energy required to drive the ancillary equipment, and inside is another loop, which is called ignition, and that is the point where the system would be generating heat even if you disconnect it from external energy supplies.

So please note the confinement parameter here, 10 to 14, 10 to the 15, and this enormous energy scale which is of the order 10 to 100—well, one is really talking about energies of the order of one million electron volts, which is the province of physics.

Let's look at the next slide.

Our experiment is really radically different from that. First of all, the energy scale is not measured in kilo electron volts but in electron volts. This is actually expressed as kilo electron volts, but a thousandth of a kilo electron volt is, of course, one electron volt, and here is—we are, therefore, at much lower energies, of the order of one electron volt, which is the province of a chemist. If you like, it is high-energy chemistry. The characteristic temperature is about 10,000 degrees Centigrade.

On the other hand, the confinement parameter here is 10 to the 36, is an astronomical magnitude. In the conventional fusion experiment, it's, if you like—it approaches a billion billion, but here it's a billion billion billion billion, this particle—parameter is really vastly greater, and if I could just backtrack—could you backtrack one slide?—there was one point I should have pointed out to you.

These diagrams here are always projected towards not the utilization of the deuterium deuterium reactions, which are safe to use in this type of large apparatus, but are projected towards fusing deuterium and tritium in these type of Tokamak devices. Now our research has been somewhat guided by that previous research.

May I go back to my slide—go forward again one slide.

There is, incidentally, another axis on here which measures the dimension of the electrode, because we have shown you already that the results are sensitive to the size of the system. But our results here are actually expressed so the deuterium plus deuterium reaction—in other words, our measurements—are based on heavy water, not water enriched with tritium, and if you observe this, here's the break-even line for that reaction. These are the measurements which we have done; these are the measurements which currently concern us, including this last one here, which is demonstrated in the vessel which we have brought here today, and we feel confident that with the systems we are investigating now we would, in fact, be about the break-even point. In other words, we would definitely be generating more heat out than we put in. But we do project to the use of deuterium plus tritium mixtures. In which case, we feel all our systems would be above the break-even point, and instead of generating of the order of 100 watts per cubic centimeter, we might be generating 10 to 100 kilowatts per cubic centimeter.

Now that is, in fact, of course—how do I switch this off so that I don't blind someone with it? Press the button again? It's off.

Well, that is, in fact, the speculation. As I said, we have been—our research has been guided by the conventional approach to nuclear fusion, but it is quite clear that we would not need to be bound by that. There are other options available for us.

So if I could just spend one more minute on how we feel where we are, it is quite clear to us that a vast amount of new research is required. Our own view is that we want to extend the science base of the investigation and, in that extension of the science base, look for the appropriate theoretical description.

Our work was not just a shot into the dark, as people believe. We were guided by reasonable theoretical formulations of what might be taking place. But of course those theoretical formulations must now be refined, and those theoretical formulations, in turn, will throw up many new suggestions for research, and that research will clearly have to be done in the whole scientific community, and at the same time we do feel that—confident about our results, sufficiently confident that we feel we would like to start a parallel investigation—set of investigations which really go down a critical path towards the development of a bench-top demonstration, something like that maybe, different to this, of course—a bench-top demonstration of a device which gives out very much more energy than you put into this.

Now what we are here today—part of our objective here today is to point this out to you, and those of our colleagues who follow us will give you the scenario for this, the reasoning, to illustrate to you that this would be an opportunity where science and technology—technological applications could be investigated at an early stage in parallel rather than sequentially, as has been the practice so far.

Thank you.

DISCUSSION

The CHAIRMAN. Wayne, did you want to make a further observation?

Mr. OWENS. No. I would like to wait until after the second panel, Mr. Chairman.

The CHAIRMAN. All right.

I want to thank you both for your presentation. The time is now for questions by Members of the Committee, and the Chair recognizes the distinguished gentleman from California, Mr. Brown.

Mr. BROWN. Gentlemen, you seem to be well aware of all of the implications of what you are doing and have given thought, obviously, to, as you indicated, Dr. Fleischmann, proceeding somewhat in parallel with looking at the developmental aspects of this while the science base is broadening.

Have you given some thought to the amount of effort measured in terms of dollars or whatever other figure you might think is reasonable for the extent of both of these kinds of things—the additional research developments that might be needed as well as the parallel examination of the technological aspects—the developmental aspects?

Mr. FLEISCHMANN. It is a difficult matter to quantify these figures at an early stage, but we are well aware that it is necessary to investigate the parameters which control the phenomenon. These experiments take quite a long time. They are not—of their very nature, they require months and not days to carry out each individual experiment. We have to cover the materials problems, and the total effort involved on the science base alone is very large indeed.

There is no doubt that we should have—we will have the same quantitative relationship in this as in other fields of research that a scale—production of a scaled up device will cost 10 times as much as—this is the rough figure you come to—10 times as much as the basic research. An actual working large-scale prototype will certainly cost 10 times as much.

Again, I think we are talking in millions of dollars for the next phase of the work, and I would really like to pass that topic over eventually because the President of the University, Dr. Chase Peterson, will be talking to you about that, and I trust that he will be willing to quantify that in his presentation. Yes, he will.

Mr. BROWN. We are looking at something that appears to be a very low budget kind of an item. You know what you spent on this experiment—I doubt if it's more than a few tens or hundreds of thousands of dollars—and you contrast that with the half a billion a year that we are spending on other kinds of fusion research, and it represents quite a marked disparity. It would indicate, obviously, that we could proceed rather rapidly with this if it has the promise that you seem to indicate.

Also, I suspect that research will be funded in various parts of the world, not just in Utah, and we can expect results coming from Europe, and from Japan, and other places of that sort.

Let me just ask one additional question. There has been considerable criticism of your procedure here with regard to the research results. I'd like to have you briefly defend the process that you have used here—that is, the public release of the information prior to the publication in a journal and the fact that the lack of data seems to have inhibited the replication of your results in most cases where it has been tried. I'm sure you've wrestled with this in your own mind, and I'd like to hear your public explanation.

Mr. FLEISCHMANN. Well, the first—I'd like to take this in inverse order. In scientific—in the process of scientific publication, it is common practice to release a preliminary publication before you write a full paper. Now there are journals which are devoted to the receipt of papers of this kind, and it was our judgment that it was appropriate to inform people at this stage about the work.

We thought that we had given sufficient data in that preliminary publication that a cool and collected look at the paper would enable other people to replicate the experiment. We admit that there were not the experimental details there, but in a preliminary note there never are these experimental details, and we do now have fax machines and telephones which would allow people to request that information from us, and those that have, we have given them that advice. I reject that particular criticism.

Now as regards the particular pattern of the release of the information, I think I would like Professor Pons perhaps to comment on that rather than myself.

Mr. PONS. In chemistry, it is generally the situation that when you have submitted a paper and the paper is accepted, which was the case in our case, then it is okay to make an announcement. I think that anyone can pick up a recent issue of the Chemical and Engineering News and they will find announcements where someone has announced the following discovery or the following important piece of research, and it is to appear in the Journal of the American Chemical Society in May. I mean this is a typical, very typical, thing in chemistry.

I think there has been a lot of confusion because the problem we have had is that physicists don't do things exactly the same way. They, in general, will send out a publication to many of their peers and have it informally reviewed before the paper is submitted for publication. This is simply a different system that chemists do not use. In the case of chemistry, or in chemistry, we leave that duty up to the editor of the journal. It is up to the editor to seek the proper peers to review that paper and then judge whether it is suitable for publication. So I think that is the situation that we found ourselves in.

Mr. BROWN. Mr. Chairman, may I follow up for just one brief additional questions?

The CHAIRMAN. By all means.

Mr. BROWN. I understand that in the case of your submission to Nature, which is very reputable, of course, that you felt that you could not provide the additional information that the reviewers asked for probably because of the thrust of the publicity that has been focused on you and so forth. Could you explain that just briefly?

Mr. FLEISCHMANN. I shall take that comment, Congressman.

The substance of the paper which has appeared was really more extensive than our preliminary paper to Nature. That was even more restricted. Now we were given reviewers' comments by Nature, and, incidentally, we steadfastly refused to tell the media that we had submitted a paper to Nature. We have been criticized in Nature for revealing that information. We did not do so, and I emphatically say that again and again and again. We refused to name the journals to which we had submitted the papers.

We got the reviewers' comments. We replied to them. Again, it is stated in Nature, I think this week, that we did not reply to the reviewers. We replied to the reviewers in detail in something like a 19-page document. But, nevertheless, we felt that we had reached the stage where there was no point in writing a short paper on this subject, that we really need to write an extensive paper—extensive sets of papers, on the different parts of our work and that this has to be submitted to the appropriate journals, and Nature would not be the appropriate place to submit this work in the form of a full paper; they don't publish full papers. So we decided the best thing to do was to say, let's leave it.

Mr. BROWN. Nature is probably going to be unhappy with this decision, I imagine.

Mr. FLEISCHMANN. Well, that is up to the editor. We don't wish to in any way influence—it would be totally wrong for people who seek to publish their work to seek to influence editorial policy. That is absolutely within the gift of the editor and the particular reviewers whom he chooses.

Mr. BROWN. We feel the same way in Congress. We don't like to influence editorial policies either.

The CHAIRMAN. And most of the time we don't.

The Chair recognizes the distinguished lady from Tennessee, our Chairman of our Energy Subcommittee, Mrs. Lloyd.

Mrs. LLOYD. Thank you very much, Mr. Chairman.

I think that you can see by the audience here for our hearing today that you have generated not only the excitement of the scientific community but the great populace of the country we live in and, indeed, the international community.

Two of the questions that I am asked most frequently when we discuss your experiment: What is the possibility of demonstrating the net output of heat at somewhat higher temperatures where there would be increased thermal efficiencies in terms of useful energy generation? The question is, hey, can we have enough useful energy generation that this will be meaningful in our electrical output?

Mr. FLEISCHMANN. Well, Congresswoman LLoyd, we actually have had a cell boiling. We have had a cell driven up to boiling point. We are quite happy to tell you that here today. But, of course, to go beyond boiling, to generate low pressure initially—low pressure, higher temperature steam will require a special effort in technology and raises many new problems. We consider it will be feasible, but that is part of the question of the ongoing costs of this research. You certainly cannot make a high-pressure steam generator with a device such as this. You have to go into the materials problems and the machining and so on. So we think it is entirely feasible to do that, that research can be initiated. There are many options open for that particular work, but it will require a very large effort.

I would not like the Members of the Committee to think that just because we have made an initial stab at this for about $100,000 that the ongoing research will be always in units of $100,000. A high-pressure steam generator, we might guess just that bit of the program, just the demonstration of that, will cost one to ten million dollars.

Mrs. LLOYD. Of course, you know that the interest of this community is in the commercial applications of technology at all times. That's the nature of our work.

The other question I'm asked as Chairman of this Subcommittee is: What led you to pursue this particular approach to experiments? Why these particular materials, Dr. Fleischmann?

Mr. FLEISCHMANN. That is really guided by our theoretical understanding.

Mrs. LLOYD. In other words, why did you use palladium?

Mr. FLEISCHMANN. Because we understand the properties of hydrogen and deuterium dissolved in palladium. The posing of the question, why can you dissolve such large quantities of deuterium in palladium under such extreme conditions without getting recombination of the deuterium in the lattice to form bubbles of D2 gas? that, in itself, is, if you like, the nutshell underlying that. The theoretical aspects of that underlying that question is what guided us initially to say it would be—if it is at all possible—and we realized that there were many difficult questions there—if it was at all possible, then palladium would be a very obvious first choice of material.

Now it may be that it proves to be the best choice or the only choice, but of course the materials aspects of this particular problem have to be fully researched again. There are other possible choices, other possible strategies.

Mrs. LLOYD. As you know, many of our national laboratories across the country have been unable to reproduce your results. If other laboratories and universities cannot reproduce your results, will this detract from your experiments?

Mr. FLEISCHMANN. Well, I've expressed the view repeatedly that any scientific process requires independent verification. Those groups who are not able to reproduce the results must, in our view, publish details of their experimental procedure in full, just as those who are able to reproduce the results and extend them must publish that work.

I have said in my presentation that it is not easy to do this work. The dimensioning of the apparatus is critically important, and I have been given access to some people's experimentation who have not been able to find the heat, and which has been totally unsurprising to me, because they would never have been able to find it using the apparatus they have used. But we are very happy to tell people how we have done it, to demonstrate our results to them, and what would worry me if they couldn't reproduce our results in our apparatus, that would worry me.

Mrs. LLOYD. Again, I want to congratulate you, but I would urge you to have Los Alamos or some other one of our national laboratories to go to Utah and verify your findings.

Mr. PONS. If I might comment on that, I've been to Los Alamos; I've seen, again, their apparatus; they've been up to my laboratory once, and they are—we have set up an experiment at Utah that they will take away in operational form as soon as it's charged up.

Mrs. LLOYD. Thank you very much.

Thank you, Mr. Chairman.

The CHAIRMAN. I thank the gentlelady.

The chair recognizes the distinguished gentleman from Pennsylvania, Mr. Ritter.

Mr. RITTER. Mr. Chairman, having just returned from serving as ranking Member on another subcommittee, I would like to retain my position after Mr. Morrison goes. But, you know, it is indicative, I was at a Subcommittee hearing on drug and alcohol rehabilitation and insurance, and I think it shows you some of the conflicting priorities. As a scientist/congressman, I am just very, very, very enthusiastic about what's going on here, but the drug problem is a major one.

Mr. Chairman, I would like to retain my position after Mr. Morrison asks his questions and the next round goes.

The CHAIRMAN. The chair recognizes the distinguished gentleman from Washington, Mr. Morrison.

Mr. MORRISON. Thank you, Mr. Chairman.

Gentlemen, first, I appreciate the visit to my office yesterday. It gave me an extra advantage in understanding and appreciating the work that you've done.

We all recognize that there's a worldwide scramble on now to duplicate, to repeat, your efforts. I was wondering, perhaps a way to provide a very quick answer to this would be if you would be willing to do the work again and let some very knowledgeable critics observe the efforts under your settings. Everyone is hanging on every word that has been published and trying to use that as the basis for duplicating your efforts. What if we did it again?

Mr. PONS. As I mentioned just a moment ago, we are doing precisely that. We have 19 new experiments being set up now. One of those is a demonstration of a previously run experiment for Los Alamos. They will come up, make the measurements they want to make on our own system, bring their electrochemists, and let the electrochemists go through our method of measuring the thermal output, and when they are satisfied with what they see, then they will take that experiment away.

I might mention that there have been other groups in the lab last week who have looked at the heat, looked at the data, and have indeed been satisfied. So we are indeed doing precisely that.

Mr. MORRISON. I think that will be very, very helpful.

Also, one of the things you shared with me yesterday in response to my question was your suggesting that we retain and maintain our investment in, let's use the term "hot fusion."

Mr. FLEISCHMANN. I have been on record throughout as saying that existing programs should not be affected by the discovery of some new phenomenon. The existing programs are well founded in theory, well founded in terms of the experimental results which have been obtained.

Stan Pons and I share the view that we shall need fusion, the generation of fusion power, in the coming centuries, probably already in the next century, and it may well be that devices based on the research which has been carried out so far will prove to be optimal for certain types of application. If our research turns out to be successful, it could be that it turns out to be suitable for the same application or a different range of applications.

I think it would be a mistake to narrow the options on the research fund. I think there will come a point in time when it is a question of trying to realize that as a demonstration unit and, in fact, to put it into commercial practice. At that point, there has to be clearly a decision taken on which line to pursue. But I would be very unhappy if the existing lines of research were affected by what we have demonstrated so far.

Mr. MORRISON. There probably would be some other people who would be unhappy, too, Doctor, so we appreciate that.

Thank you, Mr. Chairman.

The CHAIRMAN. The chair recognizes the distinguished gentleman from California, Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman.

There are some discrepancies and differences between what Dr. Jones—Professor Jones is doing at BYU and what you have been doing. How do you account for those differences, and what is being done to reconcile those?

Mr. PONS. I think the experiments are entirely different. I think that the experimental apparatus which he is using to demonstrate the cold fusion process is, indeed, somewhat different, or greatly different than what we have done. We have designed our experiments to look for heat during this entire period, whereas I do not believe that that has been the intent of his experiments.

Mr. PACKARD. Okay. Have you done anything in terms of patent protection?

Mr. PONS. The University has filed for patent protection, yes.

Mr. PACKARD. That has been done.

Also, you were offered and refused grants from DOE. What was the reason, and what is your intent in future grant opportunities?

Mr. PONS. We made application to the Department of Energy for research funds early last fall. There were considerable delays in the review process which resulted—well, and, after all, these delays—we were notified that the funding would be approved. In March, I still had no idea as to when those funds would actually arrive.

In the meantime, we had continued our investigations, continued funding these investigations, had asked the University of Utah for funding, which they had given, and we felt that, in the beginning of this month, that we had essentially accomplished most of the initial work which we had put into that proposal and that it would not be right for us at that point to take the money to do work that had already been done, and withdrew the proposal, submitted another proposal for other research to the Office of Naval Research, and that funding was granted.

Mr. PACKARD. How long do you think it would be if things went according to your hopes before commercialization of this process could become a reality? Are we looking at decades? Are we looking at a few years?

Mr. FLEISCHMANN. Congressman, I think that the normal time scale one thinks of in terms of a commercial development is 10 to 20 years. This experiment is more akin to chemistry than physics.

So the time scale, I think, might be shorter than if one were dealing with a high energy physics experiment by it's very nature, because the operation is more simple. So I think we are taking the type of time scale one thinks of for a chemical process, which I am sure you can get independent corroboration of this, would be—the thinking would be 10 to 20 years.

Nevertheless, as I have said, and others I am sure will indicate to you as well this morning, I think it is possible to envisage already one critical path to a benchtop realization, and I think while keeping in mind and not making any promises for less than 10 to 20 years, the time is ripe to start immediately on that route, because others will certainly do so.

Mr. PACKARD. Will the infusion of money enhance or shorten that period of time?

Dr. FLEISCHMANN. Absolutely. It is totally—any commercial development, any technological—any development of scale-up is totally limited by the cash flow. You can do scientific experiments sometimes on the cheap, but you cannot do scale-up, you cannot do engineering on the cheap. That is, it would be a waste of money to do so.

Mr. PACKARD. One last question, if I may, Mr. Chairman.

In your experiment, were you able to detect or determine any undesirable by-products? Were there any, for example, radioactive waste materials or other by-products that give you concern?

Dr. PONS. Other than the small amounts of tritium that we have detected, we have seen no other harmful products. We had neutrons at very low levels and tritium at very low levels. We have not detected any other radioactive material.

Mr. PACKARD. You mentioned the neutron increase. Someone has mentioned—and I'm sorry that I don't have it on the tip of my tongue—that that may have been sufficient to have actually killed the experimenter. Would you explain that, please?

Dr. PONS. Yes. We tried to point out the number of neutrons that we measured is extremely low, certainly very low, harmless levels. The amount of heat which we measure, however, is quite large. Now, if you try to explain that heat generation by the conventional deuterium fusion reaction, then yes, you would have 10 to the 9th times more neutrons, which would certainly be lethal. But we do not observe these neutrons. We do observe the heat, however, and we therefore propose a heretofore uninvestigated or unknown nuclear process that does not yield the neutrons.

Dr. FLEISCHMANN. May I just comment on that?

Mr. PACKARD. Please, Dr. Fleischmann.

Dr. FLEISCHMANN. Of course, the experiment was designed to be safe. It was designed taking into account the possibility of a high neutron flux. And when we obtained our first quantified heat release, we, in fact, discontinued that line of experiments for quite some time because we were still concerned that we might have a high neutron flux.

But as we went along with our measurements, it became apparent that there was an enormous, billion—a factor of a billion disparity between the heat release and the neutron flux.

Mr. PACKARD. So you now have no explanation as to what happens to that excess neutrons in terms of where it goes and where it's at during the experiment?

Dr. FLEISCHMANN. We have some idea of what is going on, but that requires further research. I don't think we would be too happy to talk about this at the present time. But equally, your comment about the search for other products, that is another aspect, of course, of the whole research which has to be carried out by us and by others. So far we have found nothing. There is no guarantee, of course, that someone, we or other people, will not find something under other conditions.

Mr. PACKARD. Thank you very much.

Dr. FLEISCHMANN. We cannot put our hand on our hearts and say no one will ever find anything harmful.

Mr. PACKARD. Thank you.

Thank you, Mr. Chairman.

The CHAIRMAN. The Chair recognizes the distinguished gentleman from Pennsylvania, if he's prepared.

Mr. RITTER. Thank you, Mr. Chairman. I am at this point.

I would be interested in knowing, was this research initially, or any part of it, was it funded by the Federal Government? I understand about the $322,000 DOE grant. But is this a University of Utah initiative, or was the Federal Government involved in any way?

Dr. PONS. Not at all.

Mr. RITTER. Not at all.

Dr. PONS. During the entire term, we funded it ourselves.

Mr. RITTER. This is incredible. I mean—

Dr. PONS. I might clarify that we did use university facilities, so to the extent that we used the utilities and the spaces at the University of Utah.

Mr. RITTER. Mr. Chairman, you're witnessing an example here where not all of the sweetness and light and new discoveries is going to come from the top five research universities in America. There's quite a bit out there in Utah.

What is it about Utah that—

[Laughter.]

There's Howard Nielson and there's Mr. Owens.

Dr. FLEISCHMANN. Shall I answer that, Mr. Chairman?

There were certain special conditions—

Mr. RITTER. I would be interested in hearing about that.

Dr. FLEISCHMANN. Yes. First, in the mid-1970s, I—well, late in the 1970s, I was due to become Chairman of the Department of Chemistry in Southampton again, and having had a previous spell of this, I decided to resign.

Now, that meant that I had free time. We had actually thought about this experiment before, but in order to do strange experiments, you must have some free time yourself. And so I had free time. I had a little money. My colleague here also had some free money. We thought that this experiment was so strange that it would—that we were, in the first place, extremely unlikely to get any financial support, and secondly, that it was almost incorrect to ask for financial support for a project which had a low probability of success.

Mr. RITTER. In other words, the truly creative, innovative work might not have been able to be supported by the Federal Government, is that it?

Dr. FLEISCHMANN. Well, we've had excellent support from the Office of Naval Research for strange experiments of other kinds. That is the organization, if I may—if you will excuse me and sing their praises—which preeminently has fathered and fostered innovative research in the United States.

Mr. RITTER. That's probably because they have given researchers some freedom—

Dr. FLEISCHMANN. Huh?

Mr. RITTER. They've given researchers some freedom, not just to be glancemen and to be—

Dr. FLEISCHMANN. In the small science area, in the small science area.

Now, we appreciate that, but there is a sort of limit beyond which we did not even want to drive our friends to the limits of credibility.

Mr. RITTER. There were some experiments early in the century that had something to do with palladium and some attempts to induce results such as yours. What differs between what you have done and what was done earlier?

Dr. FLEISCHMANN. Well, that refers to the work of Professor Parnett, who was a genius, an innovative genius of the earlier part of the century.

Mr. RITTER. He probably wasn't federally funded, either.

Dr. FLEISCHMANN. Probably not.

And while he worked initially in Germany and then in the United Kingdom. But that paper was subsequently withdrawn and the results were shown due to spurious accumulation of helium in the system. So he withdrew that paper—

Mr. RITTER. Without—

Dr. FLEISCHMANN. I hope that that is not a scenario for our own work. But I don't think it is. Of course, at some stage one will have to look very carefully into Parnett's results, because sometimes work is discredited and subsequently found perhaps that it was discredited incorrectly.

Mr. RITTER. Is the helium spurious?

Dr. FLEISCHMANN. His helium was probably spurious. I don't want to discuss helium too much at this stage.

Mr. RITTER. The Stanford results are trying to explain your experimental results on the basis of Helium-4. Is this going to be a "no comment" response still?

Dr. PONS. We certainly are investigating—this product would, indeed, indicate one possible further path that this fusion reaction might take, and that is certainly one that we are investigating.

Mr. RITTER. One last question. Are you patenting your process? Is it patentable? I assume it is. Washington State seems to be patenting a theory.

Dr. PONS. Oh, no. The State has taken out a number of patents on the process, and the entire research effort.

Mr. RITTER. My time is up. I want to commend you, I want to commend the University, and I want to commend the great State of Utah for being first.

Thank you, Mr. Chairman.

The CHAIRMAN. I thank the distinguished gentleman.

The Chair recognizes the distinguished gentleman from California, Mr. Rohrabacher.

Mr. ROHRABACHER. Thank you, Mr. Chairman. I didn't know I was going to be next.

First of all, congratulations to both of you for maintaining your composure at what must be a tumultuous time in both of your lives, especially in front of a hearing like this. Sometimes it gets a little difficult to express yourselves, and you've done very well today and I appreciate it. I'm sure the rest of us appreciate it as well.

First of all, we just heard a question about Stanford University. Have the findings from Stanford University tended to verify your findings?

Dr. PONS. The experiments were quite similar. I have not yet seen all the experimental details, but yes, I think that could be considered a pretty—Yes. I think there will be testimony on that, as a matter of fact, later today.

Mr. ROHRABACHER. Okay.

Your support for existing programs notwithstanding, we all know that you've created a lot of heat, not only in the beaker but outside the beaker as well. Do you think that some of this heat is being generated by the fact that there are a lot of people in the scientific community who are dependent on hundreds and millions of dollars worth of Government grants that may not be open minded towards the type of change you're suggesting is possible?

Dr. PONS. The only comment I would make there is that I think it's always dangerous to point at incorrect experimental data being based on theory. I think theory must be used to explain experimental data, not to criticize experimental data. I mean, if it's a well-established theory, then certainly you can raise questions. But I think that you need to consider first that the experimental data must be duplicated and explained, and then a theory put forth, rather than just saying your data must be wrong because the theory doesn't predict that.

Dr. FLEISCHMANN. I think Professor Pons is alluding to the nature of the criticism which has been leveled by people who are working in those areas of research. I don't really see that our work impacts too much on that work. It's another line to pursue and should be seen as that.

Mr. ROHRABACHER. But you're going to put some of these people out of business, aren't you, if you're successful?

Dr. FLEISCHMANN. Well, no. I think we will put them out of—if we are successful in demonstrating the science space, and if we go to the point of technology, then the Members of this Committee and the scientific community at large will start to make a choice about whether to develop this technology. But that technology has to be developed not only in competition with fusion, other fusion technology, but in competition with fission technology, solar energy, bioenergy, all other options as well.

So I think then we are not really going to be comparing this device with other fusion devices. We are going to compare a source of—a conceivable source of energy and a conceivable development of a technology with all the other technologies at our disposal. That's going to be a different judgment, in my view, than a judgment strictly within the area of fusion technology.

Mr. ROHRABACHER. But contrary to public opinion or perception, isn't it true that most new, major scientific breakthroughs have not been—I shouldn't say most, but many major scientific breakthroughs in human history have not been greeted by the professionals of the day with open arms and—

Dr. FLEISCHMANN. How can you expect it? I think that a strange piece of research will strike people as being strange. You have to get used to it. You have to live with it. It's like an old bicycle. You have to grow old with it.

Mr. ROHRABACHER. And perhaps the fact that so many people in the scientific community are now dependent on Government grants, that perhaps are heading in totally the opposite direction to achieve the same results, might actually make this problem even worse.

Dr. FLEISCHMANN. I hope not. I think that in the end all the people working in this area will come to see this as just another arm of the research, one they will wish to be involved in, rather than one they wish to stand aside from. I think if we are correct, if we are opening up this gray area between physics and chemistry, where there is this strong overlap, then the people who have got the big experience in the high energy physics end will have an absolutely vital contribution to make. I think they will come to see that very shortly.

Mr. ROHRABACHER. I hope you're right. I would like to note that Jonas Salk in my own time was not greeted with open arms, and was vilified for a certain period of time in his life, and there was a lot of confusion about that. I think he probably saved a lot of young people's lives.

One last question. We've heard some qualifiers from you today, and they're justifiable. But are you still absolutely confident that you have discovered a new fusion process?

Dr. PONS. Well, for five-and-a-half years I think we were our most severe critics, and we are still as sure as sure can be. We produce our data and we believe what we are seeing. So I'm sure.

Dr. FLEISCHMANN. I do not know how to interpret our results in any other way than that we have observed a fusion phenomenon. So I'm still totally convinced about our own work. But naturally, we shall have to look at everybody else's work as well, including all the unsuccessful experiments, and only time will show whether we are correct or not.

Mr. ROHRABACHER. Mr. Chairman, if I could have one more question, if this is, indeed, the opening of a new door, what do you think mankind is going to see as we walk through that door? Just a very brief summary of the new potential that this may unleash.

Dr. FLEISCHMANN. Well, of course, as I said when I made my presentation, our motivation was social. If this is correct, then we have a source of energy which is clean, which avoids the pitfalls of generating carbon dioxide and sulphur dioxide. However, let's not again have too rosy a view. It will have a destabilizing effect initially as it is put into practice. Hopefully, eventually it will have a stabilizing effect on world economies. But the adoption of such an energy scenario would not be without difficulties for the developed and the developing countries of the world. I think those raise very profound political questions, which I'm sure this Committee and other Committees of Congress will wish to address.

The CHAIRMAN. The Chair recognizes the distinguished gentleman from New Mexico, Mr. Schiff.

Mr. SCHIFF. Thank you, Mr. Chairman. Welcome, gentlemen.

I would just like to say first I appreciate the kind remarks that have passed back and forth between the witnesses and some Members of the Committee about Los Alamos National Laboratory. We're very proud of it in New Mexico, as well as Sandia Laboratory and other such institutions.

Gentlemen, I am privileged to meet you in person. The first knowledge I had of your experiment was in reading the local newspaper's reprint of it. I must confess to you, that is the first skepticism that was presented to me as a Member of this Committee back home, is that it appeared that the first release of this information was in the form of a press release. We politicians are notorious for doing that, but I was told in the scientific community things are done differently. But I heard you refer earlier to presenting the material in a sort of abbreviated form in scientific journals.

So I would just like to clarify, what was the first release of the information? Was it to a scientific journal, even in abbreviated form, or was it in a, if you will, commercial press release?

Dr. PONS. The first release of the information was to the Journal of Electroanalytical Chemistry on March the 11th, 1989.

Mr. SCHIFF. Thank you.

Gentlemen, since—if I understood correctly—your experiment produced heat but not at least the expected number of neutrons as a by-product, may I ask how you conclude that you have witnessed fusion and not a chemical reaction that produced the heat?

Dr. FLEISCHMANN. Congressman, the point, really, which demonstrates this is that you have heat release of the order of 10 watts per cubic centimeter of electrode material for periods of the order of 100 hours. We have actually run experiments longer than that. In that time, you typically release what is quantified as 5 megajoules of energy per cubic centimeter, which is about a factor of a hundred larger than that for any conceivable chemical reaction in the system. I have no doubt that if we ran it for a thousand hours—but now we come into a cost problem here, because you now are trying to run these experiments for a longer time—I have no doubt that if you run these things for a thousand hours, we will have 50 megajoules per cubic centimeter. So it would be a factor of a thousand times higher in that chemical process.

I have seen the calculations which people have put forward to try to explain that we would have a chemical phenomenon. They just don't hold up. We shall reply to that in other publications. That is just not possible. We have a moment of the chemical phenomena in our system which might possibly affect our results, and they are of a very, very minor scale and can't explain the results which we have observed.

Mr. SCHIFF. In other words, Dr. Fleischmann, you're saying the amount of heat produced in your experiment was greater than could be explained through a chemical reaction?

Dr. FLEISCHMANN. Absolutely.

Mr. SCHIFF. In terms of heat, in another study I have done as a member of the Energy Research Subcommittee of this Committee, the existence of heat to produce a fusion reaction has been almost axiomatic, at least in the testimony I've heard. In your opinion, gentlemen, what is there in your cold fusion experiment that substitutes for heat? In other words, the existing facilities produce heat to try to gain fusion; yours does not. So how do you circumvent that to get the results of fusion?

Dr. PONS. The critical parameter is to attain this minimum confinement time, which physicists have stated is 10 to the 3—is well known as 3 times 10 to the 14th. This was one of the axis on one of our diagrams. Our confinement parameter is 10 to the 36th. So this also is a critical parameter.

Additionally, it is also well known that fusion reactions can easily take place at room temperature—indeed, much lower than room temperature. This has been demonstrated by muon catalyzed fusion. So there are other things that must occur and can substitute for high temperature. One of those is holding these nuclei close enough for long enough, and you attain that with a confinement parameter. Essentially, in muon catalyzed fusion, you attain a very high confinement parameter as well.

Dr. FLEISCHMANN. May I just back that and say our original hypothesis, which we still adhere to, to some extent, is that you would get clusters of neutrons in the lattice—in appropriate lattice spaces. It is those clusters of neutrons with associated change in the screening of repulsion between the neutrons which would allow significant fusion events to take place.

It's a hypothesis. I think it would be a very difficult task to prove this theoretically. You could qualitatively prove this theoretically, but it is a difficult task. But that was our working hypothesis, and I think it's, in the end, we believe it is so because we have observed the fusion phenomenon.

Mr. SCHIFF. Mr. Chairman, I would like to ask one more question with your permission.

The CHAIRMAN. The gentleman is recognized.

Mr. SCHIFF. Thank you.

Gentlemen, the Energy, Research and Development Subcommittee has authorized a great amount of funding for fusion research, not only because of scientific achievement but because, as you have heard discussed here, the goal of energy—clean, cheap energy in the future that we know we're going to need.

Do you have any opinion at this time that you would like to offer in terms of looking at an ultimate commercialization, a power plant, if you will, to generate electricity for cities? Would you offer any comparison of cold fusion that you have experimented with with high temperature fusion, in terms of which might be the more commercially practical means in the future?

Dr. FLEISCHMANN. That is one of those sort of bottom line questions you are posing me, Congressman. I think it will be apparent to you that the design we are working on is related to something which could be put into practice using existing technology, without too much modification, using existing technology, nuclear technology.

Now, if we are correct, that we can make a range of devices scaled to different dimensions, then one point I think which this would raise very early on is the decentralized generation of electric power. That would be desirable in a developed economy, but even more desirable in a third world economy, because, of course, their costs of distributing power are the major part of the whole operation.

Now, I view the existing efforts in fusion research as being orientated at the large scale generation of power. It might be that this approach would be suitable for that, it might not. So one's initial task would be to assess the relative needs of the large scale generation of power in centralized facilities versus the decentralized generation and consumption of power in local facilities. We are not competent to make such an assessment, and this would be a study I think which the Committee would wish to have initiated by people who are competent in techno-economics.

Mr. SCHIFF. Thank you, gentlemen.

Thank you, Mr. Chairman.

The CHAIRMAN. The gentleman from Connecticut, Mr. Shays.

Mr. SHAYS. Thank you.

I would like to thank you both for being here. I have to say that I'm a new Member and I am extraordinarily excited to be on this Committee and to hear your presentation and to hear my colleagues ask their questions and your responses.

If your discovery is verified, what you have done is obviously changed the course of mankind. I would just like to parenthetically ask you, there must have been a moment when, my god, you said "We may have changed the course of mankind." Did that happen? Was there a moment like that?

Dr. PONS. No.

[Laughter.]

It sure changed our lives, I'll tell you that.

Mr. SHAYS. Let me just say to you that you don't have the advantage I do. I see a lot of smiling faces behind you, and some of your responses.

I know you've talked in general terms. I'm not a scientist. I think I'm like many Americans. I had no science class in high school and only one in college, so that's the kind of person you've having to communicate with. But my understanding is we're talking about cheaper energy, we're talking, in essence, about unlimited energy, and energy that may be less destabilizing to our environment; is that accurate?

Dr. FLEISCHMANN. That is accurate, Congressman, yes.

Mr. SHAYS. You touched—and we can imagine the positive contribution this can make. What potentially are the negative, not the destabilizing, but what potentially can be—does this have use in terms of weaponry that could be very regretful?

Dr. PONS. We have not considered any weapons applications whatsoever. I imagine there could be social problems as a new technology begins, but we have not considered any.

Mr. SHAYS. We are told by some of the experts that come before this Committee that we are a science-creating machine in our country, but we're not a science-consuming machine. I couldn't help but wonder with that.

Are you aware of any attempts by the Europeans or the Japanese scientists to develop viable cold fusion?

Dr. FLEISCHMANN. Congressman, I think the people who speak after us are more able to put you in the picture about that, but it is certainly true that this is being researched around the world, and I've had confirmation of our results from far afield, very far afield. You can guess where that might be.

Mr. SHAYS. Obviously, one of our interests, as has been pointed out by the distinguished woman Chairman of the Energy Subcommittee, that we're interested in large measure with the commercial aspects. But our Committee gets involved with the funding of so much research. Are you going to be—and obviously, we want it spent well, and we want to make sure we're putting it in the right areas.

Are you going to be making recommendations, specific recommendations, on what Government policy should flow as a result of your work, or will there be someone who follows that will do that?

Dr. FLEISCHMANN. Congressman, that will be presented by Chase Peterson, the President of our University.

Mr. SHAYS. Okay.

I would just like to thank you and this Committee for having this hearing. Thank you.

The CHAIRMAN. I thank the distinguished gentleman.

The gentleman from California has another question.

Mr. PACKARD. I do, Mr. Chairman. This is, I think, prime two witnesses and I hate to lose them without this question.

What is the difference between fusion of using what we call "heavy" water versus "light" water, and are there similar possibilities with light water?

Dr. FLEISCHMANN. That's another bottom line question. As someone once said to me, "you're not standing on my toes; you're standing on my feet." I think we would prefer not to—there are certainly possibilities of carrying out fusion reactions involving light water, or mixtures of light and heavy water. But we really do not wish to be drawn into this particular discussion at this time, Congressman.

Mr. PACKARD. Thank you.

The CHAIRMAN. The Chair recognizes the distinguished Ranking Member, the gentleman from Pennsylvania, Mr. Walker.

Mr. WALKER. Thank you, Mr. Chairman.

I'm sorry I wasn't here for all of your testimony, but I gather the one question we probably need to ask at this point of you, and then probably of witnesses to follow, is what should this Committee be doing at this point to help you?

Dr. PONS. I think to hear the rest of the testimony and then make a decision on whether the establishment of a center or the establishment of ongoing research should be made. As far as personally, I think we are going to continue with our research, irregardless.

Mr. WALKER. But the question for this Committee is whether or not we want to go ahead, use the base of research that you have now provided to establish a center, to take a look at this, and then look at applications and all of that. I mean, that's where you see this process going and where this Committee could be most helpful?

Dr. FLEISCHMANN. Yes, Congressman. What we are looking to is the resources to widen the science base and the theory base, and to try to short-circuit the consecutive development of this project and to attempt, for once, to initiate a parallel technological development at an early stage. That is really the substance of what we are looking to the Committee for.

Mr. WALKER. You mentioned a couple of minutes ago the fact that if you could run the experiment for a thousand hours it would give you more proof and so on, that you would say that gets into the question of cost. If we, for instance, were able to allocate $25 million out of this Committee for your work, would that allow you to do that kind of experiment?

Dr. FLEISCHMANN. Surely, Congressman. I mean, the point about running a long-term experiment is—I don't know what the policy is here, whether you run a two-shift system and thus the verification, or a three-shift system. You do have to triplicate on the staff, on the staffing, you have to put the resources in, you have to monitor. The first question which any industrial organization will ask is what is your thousand hour performance and what is your 7,000 hour performance? What is your one-year performance, your half-year performance?

Now, that requires funding. That is absolutely—that is beyond a private person's capability, and it is totally beyond the normal funding level which can be secured in the university or even from the conventional science-funding organizations in any country. That needs a special initiative.

Mr. WALKER. But as has been mentioned here, this Committee in the past has been willing to put fairly large amounts of money into fusion research, because we think that it's extremely important for the Nation's future to move in that direction.

The question is, we want to make certain at this point, that if this is an exciting new opportunity, as we believe it to be, are we willing then to give you sufficient resources. I am trying to get some idea of what you would regard as sufficient resources to assure that each of the goals that you want to attain gets done.

Dr. FLEISCHMANN. Congressman, I really do think that the president of our university is more capable of giving you that figure.

Mr. WALKER. That's what I understand, and so my main question—

Dr. FLEISCHMANN. But it is—we are talking about units of tens of millions of dollars. That is quite clear.

Mr. WALKER. Thank you, Mr. Chairman.

The CHAIRMAN. Let me ask just one. We've been dancing around that maypole, and I know the next two panels are going to get into that issue.

I think the question that ought to go on the record, at least as far as these two distinguished scientists are concerned, and at this point, do you feel that your work is of credibility and of substance enough—and that's the purpose of this hearing, fundamentally, today—to continue intensive research in the field of cold fusion? I think basically that is the question.

And then it would seem to me that a logical follow-on question would be, as Dr. Fleischmann was pointing out, that to get the optimum yield or the time of getting the yield out of the process could run somewhere from 10 to 20 years.

Now, assuming those two premises are correct, which we accept in your presentation this morning, we either limp along, I would assume, with limited resources to make our experimentation, which exacerbates the time frame—time is a combination of time and money for me, and time and space—or we say we make a reasonable, whatever that is, kind of contribution, some kind of help, jointly, severally, whatever the case may be, to shorten as much as possible, within reason, the time for additional experimentation, for peer review, and for all the other—what do you call it, scientists and folks doing this kind of research throughout the world. Isn't that reasonably where we're at at this point so far?

Dr. PONS. Yes, I think that's precisely the question. I think that if, indeed, it works, there will be other efforts elsewhere in the world to do the same thing, and maybe with a different philosophy than we normally do.

The CHAIRMAN. If the gentleman can yield, but my father taught me something in my life which I've never forgotten. He said to me, "Remember one thing, that half of nothing is nothing." So the question is the degree and the credibility and the desire for the Nation to move ahead, basically. Isn't that correct?

Dr. PONS. That's—

Dr. FLEISCHMANN. Mr. Chairman, may I just add to that. I think I may have expressed it to other Members of the Committee yesterday, and I would like to reiterate that. In a society with high interest rates and inflationary pressure, it is essential to shorten the commercialization of any new idea. That is a high risk strategy and we must be willing to say, if it doesn't work, oops, curtail it. Cut it off. But I think the worst scenario for the development of a new idea is to believe that we can continue with the sequential development of the science and technology. We must move towards the simultaneous development of science and technology in this area, as indeed in every other area of endeavor.

The CHAIRMAN. You see, if the Committee will indulge me one more moment, because this is sort of a little bit of a summary before you folks finish, there is another huge issue involved that has not been touched upon, in my judgment, because of the enthusiasm and the genuine excitement of what we are looking at today. But it seems to be very axiomatic to me when we start talking about the battle of the budget—and my good friend, Bob Walker, and I will have to be testifying—not testifying—we will be fighting on the floor today to protect the Science, Space, and Technology research programming. We've got to come back and say to the Congress and to the American people that if you don't develop a less offensive, call it that, or less waste product type of energy source, that you are exacerbating a whole series of issues, such as the ozone situation, and on and on. I'm thinking about the acid rain and whatever.

So that maybe when people would start to think a minute, that if we can get into the new technologies that begin to show promising results, as you testified to here today, that that can help us, where billions of dollars are involved and resources to solve these other problems—in fact, even the destruction of mankind, I don't think that's too dramatic to say today. So I think that we have to put into perspective, it isn't just the idea of clean, inexpensive energy; what does it mean to mankind and the people. That's what the average citizen has to understand. I think you have done the job and we hope to be able to draw that out a little bit further.

I see the gentleman from California wishes to make a further comment.

Mr. Brown. The Chairman has been emphasizing some points that I wanted to make, and you have done it very well, and I won't belabor it, Mr. Chairman.

The Chairman. All right. Are there any further questions or comments from the distinguished Members of the Committee? The gentleman from New York?

We want to thank you very much for your time and your presentation. We think it's been excellent. There's a lot of work to do and we'll get on with it and decide what we're going to do from here.

Dr. Pons. Thank you, Mr. Chairman.

Dr. Fleischmann. Thank you, Mr. Chairman, very much.

[The material submitted by Drs. Pons and Fleischmann follow:]

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MF/KJW

6 June 1989

Dear Congresswoman Lloyd,

First may I tell you that I am disconcerted in not knowing the correct way in which to address you. I believe I made a faux pas in addressing you at our meeting in Washington.

Secondly, I must apologise for being as tardy in replying to your letter of the 4th May and you will see that I am at present in Southampton. It proved quite impossible for me to deal with my correspondence while I was visiting Salt Lake City recently and I do hope that my late reply will not cause you too many difficulties. I am addressing my replies to Kathryn Holmes and I trust that she will forward this letter to you.

Professor Pons and I certainly appreciated being able to meet the members of the Committee and to outline the work which we have been doing. May I tell you that our current research fully confirms our earlier findings and that we look forward to publishing full accounts of our work during the Summer. Stan Pons and I have also found the sociological aspects of the recent controversy most interesting - I think it will prove to be the first example of scientific hysteria induced by electronumail and Fax machines. We both feel that there might well have to be new laws of libel with regard to publications in such media.

With kind regards,

Yours sincerely,

Martin Fleischmann

Martin Fleischmann

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MF/KJW

6 June 1989

Dear Ms. Holmes,

I am sure you will recall that Mrs Lloyd addressed three questions to me following our meeting in Washington and she has asked me to send my replies to you. May I ask you to hand the enclosed letter to her. In that letter I have apologised for being so late with my reply: it proved quite impossible for me to deal with my correspondence during my recent visit to Salt Lake City and I can only hope that the delay will not have caused you any difficulties.

<u>Question 1</u>

In our own work we have made numerous "blank" determinations. At this stage I can only tell you those that were concerned with heavy water. Thus for example there is no generation of excess heat when using platinum cathodes or when using inactive palladium cathodes.

As I have implied I am not able at this stage to comment about work in ordinary water but I will certainly follow up this letter when I am able to do so. It is, however, well known that others have shown that there is no generation of excess heat when using light water.

As I have only now returned to the U.K., I have had no opportunity to review the work with my colleagues in Harwell. However, again to the best of my knowledge, nobody has observed any neutrons or gamma rays above background when making measurements in ordinary water.

Our own experiments on the detection of gamma rays and neutrons have been carried out almost exclusively to check on the safety of our observations. We have only observed such radiation from the largest electrodes which we have used when these were polarised at the highest current densities and when these electrodes were generating large excesses of heat (using heavy water). In this work we have used a Harwell Neutron Dose Equivalent Rate Monitor, Type 95/0949-5 and a Nuclear Data ND-6 High Energy Spectrum Analyser.

It is important to realise that neutrons are produced in bursts and it appears to us that tritium production also takes place discontinuously.

/continued.....

Question 2

Our main experiments concerning the production of tritium have dealt with comparison of the generation on platinum and palladium electrodes in heavy water. There is a very small increase in the background level when using platinum and a very much larger increase when using palladium. The results which we have reported showed an increase of about 10-20 disintegrations per minute (dpm) for a 1 ml sample taken from a cell containing a Pt electrode, the initial concentration being 41 dpm. The steady state concentration for a Pd electrode of the same size was 141 dpm. The current density was 64 mA $cm^{-2}$ and the total current was 200 mA. These tritium levels are low compared to the observations which have been made by other research workers.

To the best of my knowledge comparisons have not yet been done on the accumulation of tritium in heavy and light water and this is a blank determination which must certainly be put in hand.

The quantification of the production of helium is extremely difficult and Professor Pons and I would not wish to commit ourselves on this topic at this time. Indeed, we are presently arranging for a blind test to be carried out which will involve a variety of samples as well as a number of laboratories who have offered to help with the mass spectrometric analyses.

Question 3

This can be ruled out completely and we do, in fact, have a video which demonstrates the extremely rapid radial mixing in our cells, as well as the somewhat slower axial mixing. As heat is injected along the axis, the latter is not important and we have, in fact, confirmed that there are no temperature gradients in our cells under the conditions which we use. In our system there is vigorous gas evolution and at low current densities we additionally bubble gas through the cell.

I believe I understand the origin of this question: at least one vocal commentator on our work copied the cell design which we demonstrated at the Committee meeting. We have not as yet used cells of this size which have been designed for work with very large electrodes. As they are the largest cells we have made, we thought it most convenient to use these as a visual aid. However, it turns out that others have used electrodes smaller than our smallest electrodes in cells of such enormous size and have been polarised at the currents which are lower than our lowest currents. It is totally unsurprising, therefore, that these workers have observed temperature gradients in their cells.

Yours sincerely,

M. Fleischmann

Martin Fleischmann

THE UNIVERSITY OF UTAH

May 3, 1989

Ms. Marilyn Lloyd, Chairman
Subcommittee on Energy
Research and Development
U.S. House of Representatives
Committee on Science, Space
and Technology
Suite 2321 Rayburn House Office Bldg.
Washington, DC 20515

Dear Ms. Lloyd:

Enclosed are copies of the slides we used as part of our presentation to the Committee. If I can be of further help do not hesitate to call.

Sincerely,

Stan Pons mjl

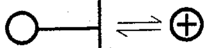
Stan Pons
Professor of Chemistry

:mjl

Department of Chemistry
Henry Eyring Building
Salt Lake City, Utah 84112

$$D_2O + e^- \rightleftharpoons D_{ads} + OD^-$$

$$D_{ads} \rightleftharpoons D_{lattice}$$



$$D_{ads} + D_2O + e^- \rightleftharpoons D_2 + OD^-$$

$$D_{ads} + D_{ads} \rightleftharpoons D_2$$

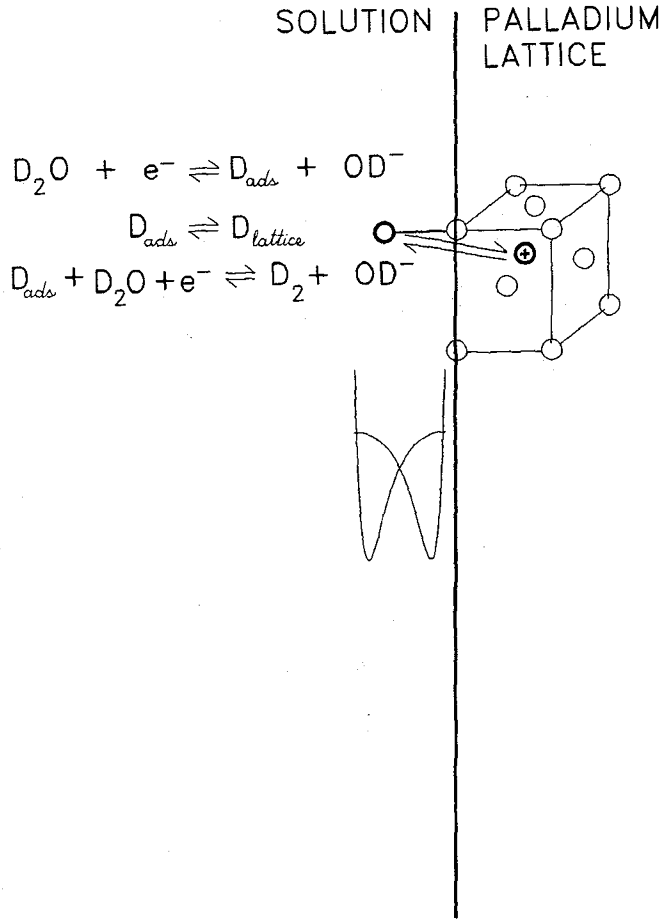
$$\mu_g^o + \frac{RT}{2} \ln P = \mu_\ell$$

$$P \cong 10^{27} ats$$

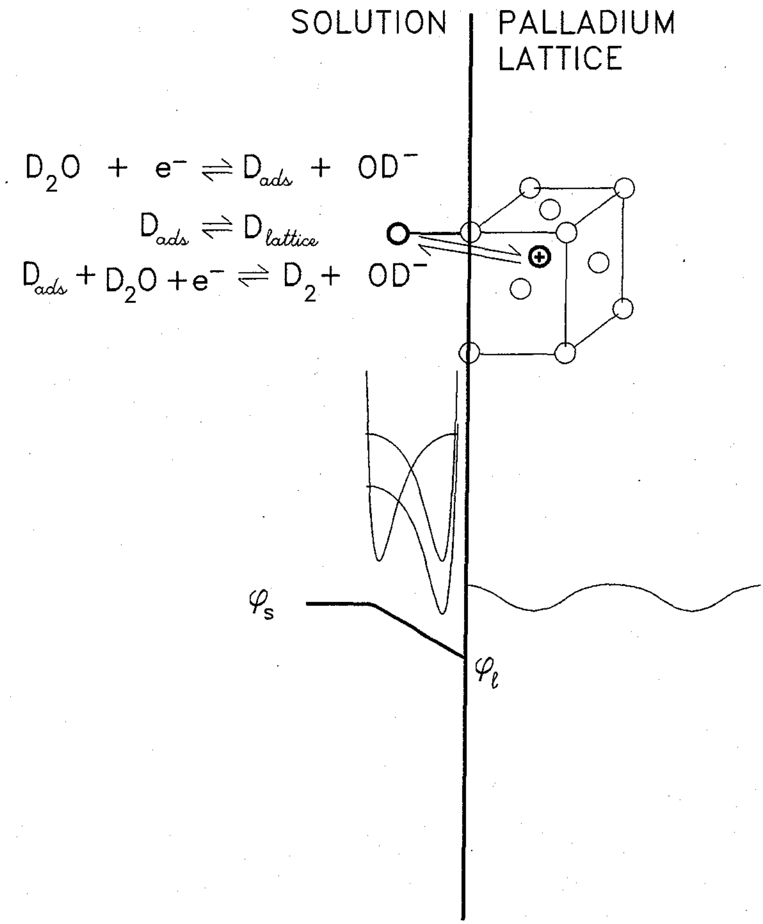
$$D_2O + e^- \rightleftharpoons D_{ads} + OD^-$$

$$D_{ads} \rightleftharpoons D_{lattice}$$

$$D_{ads} + D_2O + e^- \rightleftharpoons D_2 + OD^-$$



SOLUTION
PALLADIUM
LATTICE
$D_2O + e^- \rightleftharpoons D_{ads} + OD^-$
$D_{ads} \rightleftharpoons D_{lattice}$
$D_{ads} + D_2O + e^- \rightleftharpoons D_2 + OD^-$


SOLUTION
PALLADIUM LATTICE
$D_2O + e^- \rightleftharpoons D_{ads} + OD^-$
$D_{ads} \rightleftharpoons D_{lattice}$
$D_{ads} + D_2O + e^- \rightleftharpoons D_2 + OD^-$
$\varphi_s$
$\varphi_l$

$$D_2O + e^- \rightleftharpoons D_{ads} + OD^-$$

$$D_{ads} \rightleftharpoons D_{lattice} \qquad \rightleftharpoons \oplus$$

$$D_{ads} + D_2O + e^- \rightleftharpoons D_2 + OD^-$$

$$\bar{\mu}_s = \bar{\mu}_{lattice}$$

$$\mu_s + F\varphi_s = \mu_\ell + F\varphi_\ell$$

$$\mu_s + \underbrace{F(\varphi_s - \varphi_\ell)}_{0.8\ eV} = \mu_\ell$$

$$D_2O + e^- \rightleftharpoons D_{ads} + OD^-$$

$$D_{ads} \rightleftharpoons D_{lattice}$$

$$\rightleftharpoons \oplus$$

$$D_{ads} + D_2O + e^- \rightleftharpoons D_2 + OD^-$$

$$^2D + {}^2D \longrightarrow {}^3T + {}^1H + 4.03\ MeV$$

$$^2D + {}^2D \longrightarrow {}^3He + n + 3.27\ MeV$$

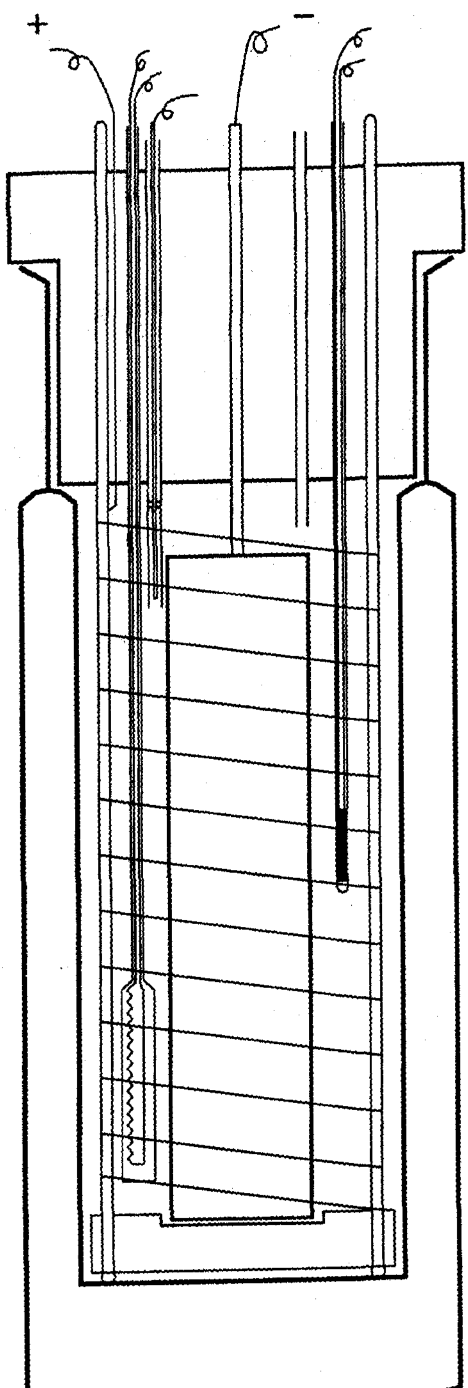
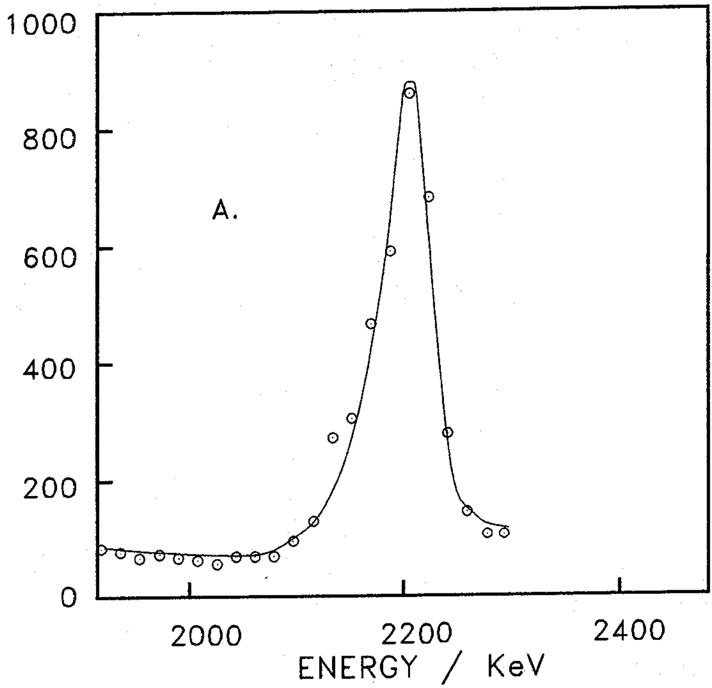


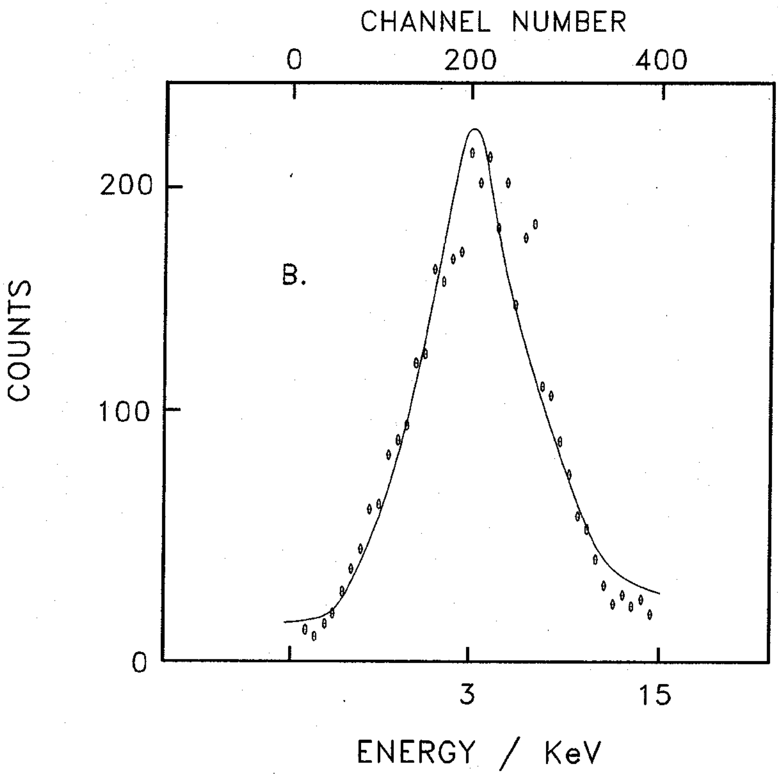
TABLE 1. Generation of excess enthalpy in Pd−cathodes as a function of current density and electrode size.

| electrode type | dimensions | current density /mA cm−2 | excess rate of heating/watt | excess specific rate of heating/watt cm−3 | current density /mA cm−2 | excess rate of heating/watt | excess specific rate of heating/watt cm−3 | current density * /mA cm−2 | excess rate of * heating/watt | excess specific rate of heating/watt cm−3 |
|---|---|---|---|---|---|---|---|---|---|---|
| Rods | 0.1x10cm | 8 | .0075 | .095 | 64 | .079 | 1.01 | 512 | .654 | 8.33 |
| | 0.2x10cm | 8 | .036 | .115 | 64 | .493 | 1.57 | 512 | 3.02 | 9.61 |
| | 0.4x10cm | 8 | .153 | .122 | 64 | 1.751 | 1.39 | 512 | 26.8 | 21.4 |
| Sheet | 0.2x8x8cm | 0.8 | 0 | 0 | 1.2 | .027 | .0021 | 1.6 | .079 | .0061 |
| Cube | 1x1x1cm | 125 | ← WARNING! IGNITION? see text* | | 250 | | | | | |

*Measured on electrodes of length 1.25cm and rescaled to 10cm.



1000
800
600
400
200
0
A.
2000
2200
2400
ENERGY / KeV



CHANNEL NUMBER
0
200
400
200
B.
COUNTS
100
0
3
15
ENERGY / KeV

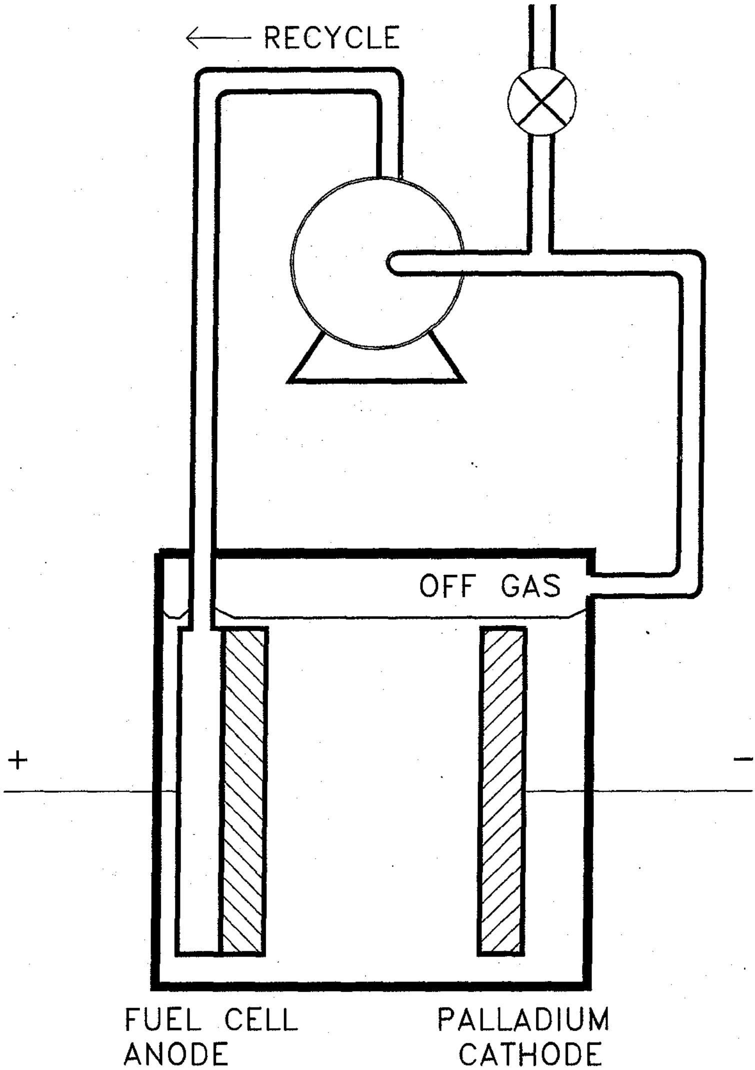

RECYCLE
OFF GAS
+
−
FUEL CELL
ANODE
PALLADIUM
CATHODE

TABLE 2. Generation of excess enthalpy in Pd rod cathodes expressed as a percentage of breakeven values.

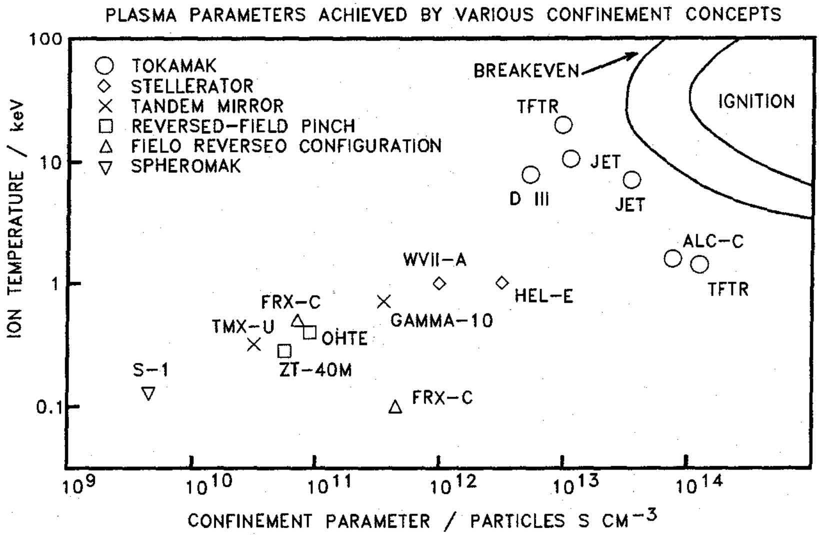
| electrode type | dimensions | current density /mA cm$^{-2}$ | excess heating * /% of breakeven | excess heating ** /% of breakeven | excess heating *** /% of breakeven | current density /mA cm$^{-2}$ | excess heating * /% of breakeven | excess heating ** /% of breakeven | excess heating *** /% of breakeven | current density /mA cm$^{-2}$ | excess heating * /% of breakeven | excess heating ** /% of breakeven | excess heating *** /% of breakeven |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Rods | 0.1x10cm | 8 | 23 | 12 | 60 | 64 | 19 | 11 | 79 | 512 | 5 | 5 | 81 |
| | 0.2x10cm | 8 | 62 | 27 | 143 | 64 | 46 | 29 | 247 | 512 | 14 | 11 | 189 |
| | 0.4x10cm | 8 | 111 | 53 | 308 | 64 | 66 | 45 | 438 | 512 | 59 | 48 | 839 |

* % of breakeven based on Joule heat supplied to cell and anode reaction $4OD^- \rightarrow 2D_2O + O_2 + 4e$

** % of breakeven based on total energy supplied to cell and anode reaction $4OD^- \rightarrow 2D_2O + O_2 + 4e$

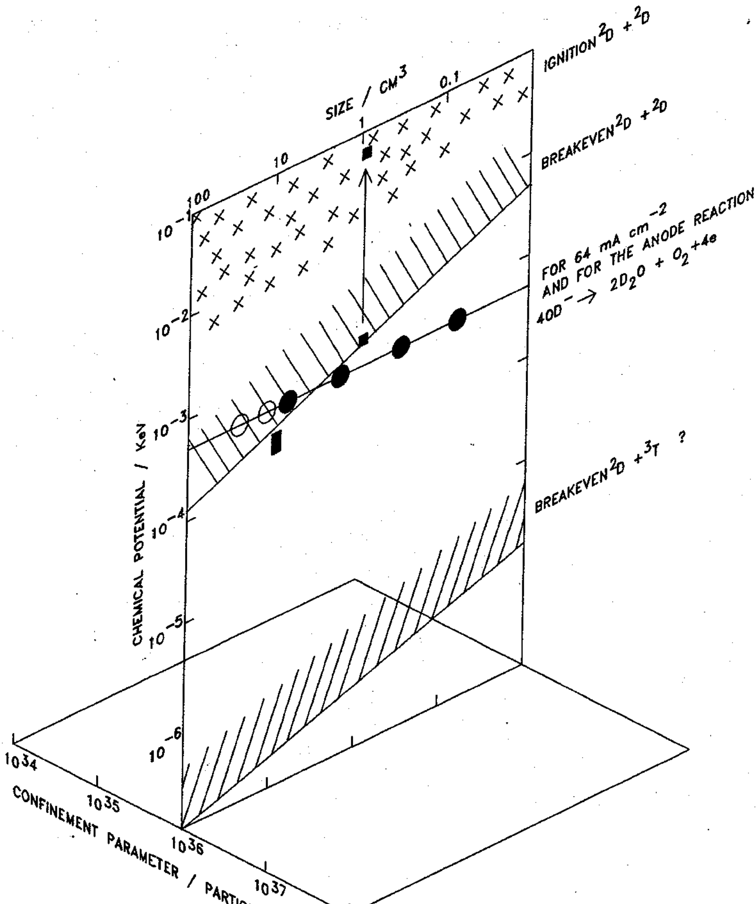
*** % of breakeven based on total energy supplied to cell and for an electrode reaction $D_2 + 2OD^- \rightarrow 2D_2O + 4e$ with a cell potential of 0.5V.

All %'s based on $^2D + ^2D$ reactions, i.e. no projection to $^2D + ^3T$ reactions.




S-1: Spheromak-1; Princeton
TMX-U: Tandem Mirror Experiment Upgrade; Lawrence Livermore
ZT-40M: Toroidal Z-Pinch; Los Alamos
FRX-C: Field Reversed Experiment; Los Alamos
OHTE: Ohmically Heated Toroid Experiment; GA Technologies; San Oiego
GAMMA-10: University of Tsukuba; Ibaraki
WVII-A: Wendelstein Experiment; Garching
HEL-E: Heliotron-E; University of Kyoto; Kyoto
D III: Doublet III; GA Technologies; San Oiego
JET: Joint European Torus; Abington
TFTR: Tokamak Fusion Test Reactor; Princeton
ALC-C: Alcaor-C Experiment; MIT

Source: Office of Technology Assessment, 1987.



IGNITION $^2D + ^2D$
SIZE / $CM^3$
100
10
1
0.1
BREAKEVEN $^2D + ^2D$
FOR 64 mA $cm^{-2}$
AND FOR THE ANODE REACTION
$4OD^- \rightarrow 2D_2O + O_2 + 4e$
BREAKEVEN $^2D + ^3T$ ?
CHEMICAL POTENTIAL / KeV
$10^{-1}$
$10^{-2}$
$10^{-3}$
$10^{-4}$
$10^{-5}$
$10^{-6}$
$10^{34}$
$10^{35}$
$10^{36}$
$10^{37}$
$10^{38}$
CONFINEMENT PARAMETER / PARTICLES S $CM^{-3}$

The CHAIRMAN. Our second panel for today is Dr. Chase Peterson, President of the University of Utah, accompanied by Mr. Ira Magaziner—is that the correct pronunciation? Pretty good, wasn't it? Magaziner, from Rhode Island.

The Committee will come to order. The Chair recognizes the distinguished—We'll wait just a minute, Dr. Peterson, until our guests...

The Chair wishes to acknowledge and welcome our distinguished Dr. Chase Peterson, President of the University of Utah, and his colleague, Mr. Magaziner. And, Mr. Owens, did you wish to comment again?

Mr. OWENS. I would only give this very brief background, Mr. Chairman. Dr. Peterson is an eminent physician and scholar and a former Vice President at Harvard University, and has been for six years President of the University of Utah.

He will be followed by Ira Magaziner, who is one of the world's renowned business consultants, particularly dealing on issues of competition, world competition. He has written several books, including "The Silent War", which it will be my pleasure to provide a copy of to each Member of the Committee.

The CHAIRMAN. The Chair wants to recognize Dr. Peterson.

## STATEMENT OF DR. CHASE N. PETERSON, M.D., PRESIDENT, UNIVERSITY OF UTAH, SALT LAKE CITY, UTAH

Dr. PETERSON. Chairman Roe, Members of the Committee—

The CHAIRMAN. You have to move that microphone closer.

Dr. PETERSON. Chairman Roe—Is that better?

The CHAIRMAN. Yes, that's much better.

Dr. PETERSON. —and Members of the Committee.

The CHAIRMAN. If you could just suspend a minute, Doctor.

In or out, ladies and gentlemen, one or the other. Close the doors.

You're recognized.

Dr. PETERSON. Fine.

It's an honor to be with you. With your permission, I would submit my written testimony for the record and speak very briefly from notes.

The CHAIRMAN. No objection. So ordered.

Dr. PETERSON. Congressman/Professor Ritter, I've learned of your background in the last few minutes. Let me comment on the question you asked of our investigators—why didn't they come to other sources for money. I asked the same question myself four months ago, and Professor Pons said "I have my pride. I would have been too embarrassed to ask my university to fund something that was as far-fetched as this." Well, I think it does say something about the individual you were talking about.

Mr. RITTER. If the gentleman would yield, it also says something that perhaps we need to encourage greater flexibility in our own procedures in dealing with scientists.

The CHAIRMAN. I also want to add a comment, Dr. Peterson, that we're 48 Members of this Committee, but there's only one professor, and it takes 47 of us to keep an eye on him.

[Laughter.]

Dr. PETERSON. Mr. Chairman, I perceive the caution in your remark to this former professor.

The comments that my colleague, Ira Magaziner, has to make about what essentially is a separate but related issue, and that is the public policy aspects of this, in my view, they are so important that I would like to turn to him and have him give his testimony right now, and I will stand with him to pick up my own testimony, if there are pieces that would usefully contribute later. If that's all right with you, Mr. Chairman, I would turn—

The CHAIRMAN. No objection whatsoever. The Chair recognizes the distinguished gentleman, Mr. Magaziner, from Rhode Island.

## STATEMENT OF IRA C. MAGAZINER, CONSULTANT TO THE UNIVERSITY OF UTAH, PRESIDENT, TELESIS, USA, INC.

Mr. MAGAZINER. Thank you very much, Mr. Chairman.

I'm not from Utah and I wouldn't recognize a piece of palladium or a fusion reaction even if I were staring right at it. In that regard, I assume I'm similar to most people in this room.

What I am concerned about—and it's a somewhat different issue than what we've been talking about—is what is American public policy going to be with respect to this invention and, in general, with respect to inventions that we make in this country.

From its early days, this Nation prospered in great measure because we were very good at taking science and being very practical in converting it into industry. Over the past 15 years, we have been losing that ability. I can go through a whole long list for you, and I'll give you part of it.

American scientists at Raytheon invented the microwave oven, but today it is Korean and Japanese companies who produce 90 percent of the world's microwave ovens. American scientists at RCA invented the color television, but today European and East Asian companies produce over 97 percent of the world's color televisions. American scientists at Ampex invented the VCR, but today Japanese, Korean, and European companies produce over 99 percent of world VCRs. American scientists, funded by DARPA, invented the computer numerically controlled machine tool, but today European and Japanese companies produce over 75 percent of the world's computer numerically controlled machine tools.

American scientists at AT&T Bell Labs and Texas Instruments invented the base technology that produced the world's first memory chip, but today over 80 percent of the memory chips produced in the world are produced in Japan. American scientists, backed by NASA, sent the first commercial communications satellites into space, but today, it is a European Company, Aerienne Espace, which has acquired well over half of the commercial space launching business.

American scientists at Control Data and Cray Corporation invented and perfected the first supercomputer, and we now trail, technologically, Japan's NEC in the production of supercomputers. American scientists at Bell Labs invented the first photovoltaic cell, and we're now seeing the Japanese and European companies produce over 70 percent of the world's photovoltaic cells. And more recently, three years ago, we made the breakthrough in superconductors, and a recent OTA report now assesses that the Japanese are ahead in commercializing superconductors.

I'm afraid I could go on with this list for a long time. I won't do it today. But we all know the result of it is, the result is that we still have a $135 billion negative trade balance despite devaluing our dollar by 48 percent the past couple of years. We should make no mistake about it. Over 50 percent of our negative trade balance is from countries who pay higher wages and higher benefits to their workers than we do. They don't beat us with cheap labor. They beat us with technology and skilled labor.

That is the fundamental reason why I have come today, to talk a little bit about why we're not being able to commercialize and reap the economic benefits of our scientific invention.

In former days, as Dr. Fleischmann said, we had a kind of chain of events that took many decades from basic research to getting products. Basic research was done in the universities, then company or Government laboratory scientists read the papers, produced and began to think about new technologies. Then company product divisions began to engineer specific product prototypes to take to their customers. Then the customers looked them over and suggested modifications. Then these products were introduced to the market. Then companies worked on ways to manufacture these new products more efficiently. It was all done sequentially, it all took decades.

Today, these steps don't move sequentially. They move in parallel. Even before basic science is proven, applied research begins, product developments are undertaken, market research is done, and manufacturing processes are working. That's the way the Japanese and Europeans are playing the game. We in America are not playing it that way.

In this country, usually we have companies competing with each other in these basic stages, whereas in Europe and Japan there are major programs of government-backed cooperation, not just for the basic research but also for the commercialization, the commercial research and development.

In Japan, billions are being spent through the agency for science, industrial science and technology, located within MITI, to fund a whole series of projects, about 50 different projects. The areas of emphasis range across the scientific spectrum.

In Europe, billions of dollars are being spent through programs called Eureka, Esprit, Brite and Race, dedicated to commercializing basic science.

As we speak now, in Japan there are now a number of company laboratories, as well as university laboratories, working in this cold fusion area. Beginning about a week after these experiments were announced, these were set up. And people at MITI are now formulating a plan for a joint government-industry-university task force, as they do in Japan, to look not just into expanding the basic science, but into commercializing, commercializing activities from this basic science.

What is called a "fusion fever" in the Japanese newspaper has gripped Japan's scientific and commercial communities, and as far as we can tell, there are over a hundred companies already beginning to think about ways that this can proceed.

Similarly, a project team is being formed at Eureka in Europe to do the same thing, not just fund the basic science but also look towards commercial research and development in the field.

Now, what should we do to respond to this? Well, if we do what we did with high temperature superconductivity, we will work for a while to verify and test the science; then the Defense Department will sponsor some work on how it could be useful to them; a handful of our companies will each put a few people to work on it; some Utah bodies, assisted perhaps by State and Federal funds, will support the continuation on a modest level of research, and maybe even develop a national laboratory of some sort. Then what will happen is OTA will undertake an 18-month study, which will be completed in early 1991, and will report that the Japanese have blown past us again in commercialization of another new science.

There is an alternative, and that's what I would like to talk to you about today in the remaining few minutes. It's an alternative that says that America is prepared to fight to win this time. The alternative is to form a research institute around this new science, but one which will be adequately funded and flexibly run, and which will engage both in basic research and, very importantly, in commercial development work. The institute can be funded with money from universities, the State, corporations, and some from the Federal Government. Additional funds can be made available to fund on a matching basis corporate efforts to develop products, manufacturing processes, prototypes, and market demonstration projects.

While Federal grants can be used to fund the basic research, we would suggest that the assistance for applied research and commercialization can be provided in the form of conditionally reimbursable loans so that the taxpayers can realize some return as well from the commercialization of this new science.

The University of Utah is willing to raise the money from its supporters, and has already started to do so, and from private corporations to support the effort. The State of Utah has already committed five million dollars to support the effort, and now it's the Federal Government's turn to step up to the plate. This need not be, nor is it desirable, for it to be primarily a Federal Government based project. But to match the competition in Europe and Japan, there must be Federal support.

But wait a second, you say. The science isn't even proven. Reputable fusion physicists throughout the world are expressing profound skepticism about the experiments. We don't even know whether it's really fusion for sure, although I would say fairly convincing arguments have been made to say that it is, and to some extent, if we go charging ahead, as I'm suggesting, we could all wind up with egg on our face, because we will have this major effort getting going—and then suppose it turns out to be a dirty test tube? I'm sure it's not, but suppose it does.

Well, like most of you, I'm not a scientist. I can't comment on whether this is the most important invention of the century or whether it's nothing ultimately. What I do know is that some very serious and accomplished people think it's real, and I do know that if it is, the implications are dramatic for the world and in particular for the nation that pioneers the products based on it.

I am a business strategy consultant, and if you will indulge me for a second, I'll take you through a risk/return analysis to our two alternatives here. One alternative says we proceed as I've suggested and begin to fund the development of this center at Utah, and what begins to happen is that maybe a week from now or a month from now or a year from now somebody discovers that this was all wrong. If it's a week from now, we may wind up losing thousands; if it's a month from now, we may wind up losing hundreds of thousands; if it's a year from now, we may end up losing a couple of million. All that's not anything to laugh about. It's a lot of money. A lot of good public servants have gotten in trouble for losing track of lesser amounts of money.

But now let's suppose that the science is real and it does open up a new energy source in the next decade and becomes a multi-billion dollar or even hundred billion dollar industry in the next few decades. If we dawdle and wait until the science is proven, and if we wait for economists to hold symposia on whether Adam Smith would approve of putting public money into it, or if we more cautiously and invest only in basic research, or only in defense applications, and wait for the spinoff, we're going to be much slower off the blocks than our Japanese and European competitors, because they won't run the race that way. Whether we approve of the way they do it or not, that's what they do, and they move quickly to commercialize.

Competitive success is a leading position in a race. If we fall too far behind at the beginning, we may never catch up. The downside risk of that could well be hundreds of thousands of high-paying jobs for our children, billions of dollars of trade balance, and billions in wealth, which then will go to someone else. Ultimately, it's not a very hard business strategy question. The downside of not doing something is much greater than the downside of taking the risk of spending some money.

So now I hope you can understand why I came here, even though I'm not from Utah. I have an interest in America's future. I see this as an opportunity both for America to develop this science and to future American prosperity, and also, importantly, to develop a model for how America can regain world preeminence in commercializing other new sciences in the coming decade.

I have come here to ask you to prevent another TV or VCR or computerized machine tool or solar cell or superconductor story. I have come to ask you to lead so that we will not be the first in our Nation's ten generations to leave its children a country less prosperous than the one we inherited. I have come here to ask you for the sake of my children and all of America's next generation to have America do it right this time.

Thank you very much.

[The prepared statement of Ira C. Magaziner follows:]

**STATEMENT OF IRA C. MAGAZINER**

**PRESIDENT, TELESIS, USA, INC.**

**HOUSE COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY**

**APRIL 26, 1989**

Dr. PONS. Oh, no. The State has taken out a number of patents on the process, and the entire research effort.

Mr. RITTER. My time is up. I want to commend you, I want to commend the University, and I want to commend the great State of Utah for being first.

Thank you, Mr. Chairman.

The CHAIRMAN. I thank the distinguished gentleman.

The Chair recognizes the distinguished gentleman from California, Mr. Rohrabacher.

Mr. ROHRABACHER. Thank you, Mr. Chairman. I didn't know I was going to be next.

First of all, congratulations to both of you for maintaining your composure at what must be a tumultuous time in both of your lives, especially in front of a hearing like this. Sometimes it gets a little difficult to express yourselves, and you've done very well today and I appreciate it. I'm sure the rest of us appreciate it as well.

First of all, we just heard a question about Stanford University. Have the findings from Stanford University tended to verify your findings?

Dr. PONS. The experiments were quite similar. I have not yet seen all the experimental details, but yes, I think that could be considered a pretty—Yes. I think there will be testimony on that, as a matter of fact, later today.

Mr. ROHRABACHER. Okay.

Your support for existing programs notwithstanding, we all know that you've created a lot of heat, not only in the beaker but outside the beaker as well. Do you think that some of this heat is being generated by the fact that there are a lot of people in the scientific community who are dependent on hundreds and millions of dollars worth of Government grants that may not be open minded towards the type of change you're suggesting is possible?

Dr. PONS. The only comment I would make there is that I think it's always dangerous to point at incorrect experimental data being based on theory. I think theory must be used to explain experimental data, not to criticize experimental data. I mean, if it's a well-established theory, then certainly you can raise questions. But I think that you need to consider first that the experimental data must be duplicated and explained, and then a theory put forth, rather than just saying your data must be wrong because the theory doesn't predict that.

Dr. FLEISCHMANN. I think Professor Pons is alluding to the nature of the criticism which has been leveled by people who are working in those areas of research. I don't really see that our work impacts too much on that work. It's another line to pursue and should be seen as that.

Mr. ROHRABACHER. But you're going to put some of these people out of business, aren't you, if you're successful?

Dr. FLEISCHMANN. Well, no. I think we will put them out of—if we are successful in demonstrating the science space, and if we go to the point of technology, then the Members of this Committee and the scientific community at large will start to make a choice about whether to develop this technology. But that technology has to be developed not only in competition with fusion, other fusion

Telesis

I am not from Utah. Nor would I recognize a piece of palladium or a fusion reaction even if I were staring right at them.

I am here because I am concerned about my three children and the future prosperity of their generation in America. There are many things which will determine whether they prosper, but few more important than America's ability to lead the world in pioneering the technologies and products of the future.

From its early days, this nation has prospered in great measure because we have led the world in taking the scientific knowledge of the day and bringing forth commercial products which we made more efficiently and in greater abundance than anyone else. We have been a practical people. We didn't always pioneer the science, but more often than not we led the way in applying the science to benefit a large number of people. This ability made us the most prosperous nation in the history of this planet.

Over the past fifteen years, however, we have been losing this ability. To be sure, we have had more than our share of scientific inventions, but we have lost the knack of converting these into products to create jobs for our people. Too often, we have won the battle of the patents but lost the war of creating jobs, the profits and wealth to other nations.

American scientists at Raytheon invented the microwave oven but today it is Korean and Japanese companies who produce 90% of the world's microwave ovens including well over 2/3 of those bought by Americans. American scientists at RCA invented the color television, but today European and East Asian companies produce over 97% of the world's color televisions including 85% of those bought by Americans. American scientists at Ampex invented the VCR, but today Japanese, Korean and European companies produce over 99% of the world's VCRs including virtually all of those bought by Americans. American scientists funded by DARPA invented the computer numerically controlled machine tool, but today European and Japanese companies produce over 75% of these machines, including over 60% of those bought by American companies.

[redacted] American scientists at AT&T Bell Labs & Texas Instruments invented the base technology that produced the world's first memory chip, but today Japanese companies produce over 80% of the world's memory chips including over 50% of those bought by American companies. American scientists backed by NASA sent the first commercial communications satellites into space but today, it is a European company, Aerienne Espace, which has acquired well over half of the commercial space launching business.

Though American scientists at Control Data Corporation and Cray Corporation first invented and perfected the supercomputer, we now trail Japan's NEC Corporation in supercomputer technology. Though American scientists at Bell Laboratories first invented the solar cell to convert sunlight to electricity, today Japanese and European companies have well over 70% of the world market. While scientists in America first invented high temperature superconductors just three years ago, a recent U.S. Office of Technology Assessment study team concluded that the Japanese were already ahead in commeroializing products from this new technology.

I am afraid that I could keep us here for hours continuing this list. I could of course list exceptions, cases where America leads the world in commercializing products we invented, but the negative list is growing faster than the positive one.

What's the result of all this? A negative trade balance of $135 billion despite a 48% devaluation of the dollar over the past 4 years. This deficit forces us to borrow from our foreign competitors each year and to sell them our land, our buildings and even our productive companies to finance our current living standards. And let's be clear. Well over 50% of this trade deficit is with nations like Japan, Germany, Franoe, Sweden, Holland, Switzerland and Denmark who pay higher wages -- yes higher wages -- and higher benefits to their workers than we do to ours. They don't beat us with cheap labor, they beat us with technology and skilled labor.

There are many reasons for our negative trade balance, but the fact that foreign countries are able to convert science into commercial products quicker and better than we do is one of the crucial causes.

The reasons they can do this are not hard to understand: more investment, better cooperation among government, industry, universities and research institutes and superior planning to develop marketable products even before the science is proven.

In former days, basic research was done in universities. Then, company or government laboratory scientists read the papers produced and began to think of new technologies. Then company product divisions began to engineer specific product prototypes to take to their customers. Then the customers looked them over and suggested modifications. Then these products were introduced to the market. Then companies worked on ways to manufacture these new products more efficiently. The process from basic science to mass production took decades.

Today, these steps don't move sequentially, they move in parallel. Even before basic science is proven, applied research begins, product developments are undertaken, market research is done, and manufacturing processes are developed . . . and here is where we in America fall behind.

In America, these early steps are usually taken by companies working on their own, competing with each other and often duplicating each others work as they compete. In Europe and Japan today, these steps in what is called the precompetitive stage are taken in cooperation. Companies work with each other and with applied research institutes and universities, usually with government funding and support, to accelerate the process of turning science into marketable products. In America, this partnership approach is frowned upon as meddling with the free market. In Europe and Japan, it is only when the first generation of products is developed that competition is promoted -- and then companies compete fiercely with each other.

[redacted]

Increasingly, the early stage competition is among nations and the later stage among companies.

We may not philosophically approve of this government backing for industrial development but it is the reality in today's international marketplace and we cannot let our bias blind us to its effectiveness. Catching up requires many actions: changing our financial structure to encourage industrial companies to take a longer time horizon, for example. But no action is more fundamental than meeting the need for publicly supported commercial research and development to match the efforts now underway in Europe and Japan.

Today in Europe, billions of dollars are being spent each year through general programs such as Eureka, Esprit, Brite, and Race and through specific programs like Airbus and Aerienne on over 500 projects bringing together companies and research institutes to pioneer the products of the 1990s. The Europeans are determined. Over $17 billion dollars of government money went to finance the development of Airbus over 20 years so that it could come from nothing to 25% of the world's commercial jet aircraft market, surpassing Lockheed and McDonnell Douglas. The result is that today Europe has 50,000 high skilled jobs and $5 billion of positive trade balance instead of America.

In Japan, billions are being spent through the agency for Industrial Science and Technology located within the Ministry of International Trade and Industry on dozens of joint projects bringing together companies, government laboratories and universities to pioneer products for the 1990s. Areas of emphasis range from biotechnology to new high performance materials to new electronic devices.

And what do we have to match these efforts? A few hundred million funnelled through the Defense Department for a handful of projects such as Sematech.

[redacted]

And even when we do these, we go through soul wrenching debates about whether we are violating our free market principles. Recently DARPA has been considering awarding $30 to $60 million to fund high definition television development in the U.S. and the debate about whether this is correct policy has reached the covers of a half dozen major periodicals. From Europe and Japan where hundreds of millions of government dollars routinely have been going into funding this technology every year, our late philosophical debate over so little money seems bizarre.

* * * * *

What does all of this have to do with my friends from Utah. As I speak to you now, it is almost midnight in Japan. At this very moment, there are large teams of Japanese scientists in University laboratories trying to verify this new fusion science. Even more significantly, dozens of company engineering laboratories are now working on commercializing it, thinking of products which can be created if the science works. Perhaps most significantly, a half dozen MITI officials are working hard on a plan for a coordinated push into this new industry. These efforts began within a week of the Utah announcement. MITI is already in the process of forming a committee to implement its plan. A phenomenon the Japanese newspapers have already dubbed "fusion fever" has gripped Japan's scientific <u>and</u> commercial communities.

Similarly, a project team is being formed at the Eureka program in Europe, and a number of European Universities and companies are already at work to develop a European "cold fusion" capability.

So what should we do? Well, if we do as we did with high temperature superconductivity, we will work for a while to verify and test the science. Then the Defense Department will sponsor some work on how this could be useful to them. A handful of our companies will each put a few people to work in the area and we will hold a few conferences. Some Utah [redacted] bodies assisted perhaps by state and federal funds will support the continuation on a modest level of research in this area and may even develop a national laboratory to pursue the science. OTA will undertake an 18 month study to see how we are doing and early in 1991 they will report that the Japanese have blown past us again and are leading in the race to develop industries from this new science.

There is an alternative. It's an alternative that says that America is prepared to fight to win this time. The alternative is to form a research institute around this new science, but one which will be adequately funded and flexibly run and which will engage both in basic research and in commercial development work. The institute can be funded with money from the university, the state, corporations and the federal government. Additional funds can be made available to fund on a matching basis corporate efforts to develop products, manufacturing processes, prototypes and market demonstration projects.

While federal grants can be made available to fund the basic research portion of the institute, the assistance for applied research and commercialization can be provided in the form of conditionally reimbursable loans which are paid back with a high interest rate if projects succeed and not paid back if they don't, with a sliding scale in between. This will allow the taxpayers of America to receive a potential return on their investment.

The University of Utah is willing to raise money from its supporters and from private corporations to support this effort and has already begun to do so. The State of Utah is willing to raise money to support this effort and has already committed over $5 million to do so. Now it is the federal government's turn to step up to the plate. This need not be, nor is it desirable for it to be, primarily a federal government based project. But to match the competition in Europe and Japan, there must be federal support.

[redacted]

But wait a minute you say. This science isn't even proven. Reputable fusion physicists throughout the world have expressed profound skepticism about these experiments. We don't even know if this is really fusion or just some quirk. Wouldn't it be prudent to wait until we see whether there is really something of value here? We could all wind up with an extra large egg on our faces and waste the public's money in the process.

Well, like most of you, I am no scientist. I have no idea whether this is the most important invention of the century or whether it is nothing. I do know that some very serious and accomplished people think it is real, and I do know that if it is, the implications are dramatic for the world and in particular for the nation that pioneers products based on it.

I am a business strategy consultant. I hope you won't mind if I take you through a brief risk /return analysis to weigh our strategic options. Suppose this science is a blind alley. Suppose a week or a month or a year from now scientists find that there really isn't anything much to it. If we move aggressively ahead and invest as I suggest, we will lose a few thousand dollars if it is discredited next week, a few hundred thousand if it is discredited next month, and a few million dollars if it is discredited next year.

Well, a couple of million dollars or even a couple of hundred thousand or even a couple of thousand is serious business -- good public servants have gotten in trouble for losing track of lesser sums.

But now let's suppose that this science is real and it does open up a new energy source in the next decade and becomes a multi-billion dollar or even hundred billion dollar industry in the next few decades. If we dawdle and wait until the science is proven and if we wait for the economists to hold

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[redacted]

symposia on whether Adam Smith would approve of putting public money into it or if we move cautiously and invest only in basic research or only in defense applications and wait for the spinoff, we will be much slower off the blocks than our Japanese and European competitors, because they won't run the race that way.

Competitive success is a leading position in a race. If we fall too far behind at the beginning, we may never catch up. The downside risk of that could well be hundreds of thousands of high paying jobs for our children, billions of dollars of trade balance and billions in wealth which then will go to someone else.

This is not a very hard business strategy problem. The downside of wasting a few thousand or even a few million dollars is far less risky than the downside of losing this possible future industry to foreign competitors. The right decision is pretty clear.

So now I hope you can understand why I came here today even though I am not from Utah and have no interest in palladium. I have an interest in America's future. I see this as an opportunity for America both to develop this science into future American prosperity and also to develop a model for how America can regain world preeminence in commercializing other new sciences in the coming decade.

I have come here today to ask you to prevent another TV or VCR or computerized machine tool or solar cell or superconductor story. I have come to ask you to lead so that we will not be the first of our nation's ten generations to leave its children a country less prosperous than the one it inherited. I have come here to ask you, for the sake of my children and all of America's next generation, to have America do it right this time.

Dr. PETERSON. Thank you, Ira.

The CHAIRMAN. Thank you, Mr. Magaziner.

Dr. Peterson.

Dr. PETERSON. If I may continue—

The CHAIRMAN. Yes, sir.

Dr. PETERSON. I know some science, but I'm not a scientist. My job is to understand the disciplines of the university and to protect those disciplines and support those people, so they can do the primary work, which is research and teaching. That's what your job is, it seems to me. You need not be scientists here, but you are social science protectors and supporters, and your role for the Nation is much like my role is for the university.

What led to these University of Utah experiments? It may have been a capacity to see an old problem from a new perspective, and this chemistry, this electrochemistry matter as opposed to physics may explain some of the humor and the bite as well as the appropriate skepticism that has surrounded this controversy.

Perhaps it's not by chance that it occurred at some place like Utah. We pride ourselves on encouraging unorthodox thinking, while being viewed by the world, I suspect, as a rather conservative place, even a socially-orthodox place. That combination of unorthodox encouragement of thinking in a fairly orderly society isn't the worst of all worlds.

There may also be some value in isolation. America has prospered and innovated at the frontier. Utah is still a frontier. As there are social frontiers in New York City and in California and in Washington and Tennessee, the action is on the frontiers when we are wise enough to recognize what a frontier is. And so the faculty we attract to Utah are those faculty who value their intellectual freedom and capacity for individual entrepreneurism perhaps more than anything else.

So we're dealing with two separate but related issues today. One is the challenging science—you've heard of that and I won't elaborate any more—not yet fully proved. The second is the challenging political and economic policy issue that Mr. Magaziner has raised, the quest of American competitiveness.

I say it in these terms. We may be obliged to build the first floor of commercial development, as well as the second floor of engineering development, while we're still building the basement of scientific confirmation and enlargement.

Now, of course, if any of these phases fail, then the process stops. At Utah, with the University of Utah, with colleagues from Utah State University and Brigham Young University, and faculty gathered worldwide, that's a good place for rapid and perhaps novel development. Our political and social system is remarkably flexible, in some ways a throwback again to the frontier, where we attract those faculty who do value their freedom. And there may be some value in isolation from traditional centers, quite frankly. The physical environment itself promotes vigorous thinking and vigorous living.

Incidentally, we have uninhabitable, remote regions only 25 minutes from the University, and 25 minutes from an international airport, which would serve as a useful place for what you might call special experiments. The University, as Mr. Magaziner has said—the State, rather, has appropriated $5 million last week. We have raised $1.1 million of private funds already, and we can raise much more in that respect. And what we offer you is a willingness on our part to build a novel consortium for this purpose, perhaps a consortium that would work for other scientific ideas that come across your view, a novel consortium of Federal, State, corporate and university resources. Without Federal participation, the race would be handicapped, but I wouldn't even suggest that the Federal participation is the major participation, but both symbolically and with the value of the money involved, we suggest that this consortium—Federal, State, private corporations, and university resources—may, in fact, be the best way to build this first floor of engineering development, a second floor of commercial development, while we, in fact, are building a basement foundation of scientific confirmation and expansion.

Thank you, Mr. Chairman.

[The prepared statement of Chase Peterson follows:]

Statement of
Dr. Chase N. Peterson

Before the
House Committee on Science, Space and Technology
Hearing on Recent Developments in Solid State Fusion Research

April 26, 1989

My name is Chase Peterson. I am trained as a medical doctor. I practiced and taught for five years after residency and fellowship in Internal Medicine and Metabolism, and for the last 22 years have been in educational administration, the last 5 1/2 of which have been as president of the University of Utah.

I know some science but am not a scientist. My job is to have some understanding for the multiple disciplines of a university and to provide protection and support for those who do the primary work of teaching and research. Perhaps then, my role in leading off in these hearings is to sketch a general context of the science under discussion, to suggest its potential importance for humanity and the planet, and to share with this important Committee of the Congress the intellectual and cultural circumstances at Utah that may have played a role in its nurture and expression.

Fission involves the splitting of large atoms into smaller pieces and produces enormous energy. Fusion involves the union of very small atoms into slightly larger atoms and produces even more energy. In each case the products of the reaction, be it fission or fusion, have a smaller mass than the originating atom or atoms and the difference is expressed as energy in keeping with Einstein's formula, Energy = Mass times the Speed of Light squared, $E = mc2$. Fusion occurs spontaneously in the sun in association with enormous heat (millions of degrees) and pressure. Fission fired the first atomic bombs and provides the heat of atomic reactors. Fission has certain disadvantages as a heat source. It's products are intensively radioactive and long-lived, giving us a nuclear waste problem. It's fuel is uranium, a moderately expensive and limited resource. The fuel of fission is contained within an atomic reactor and must be slowed and cooled to be safe. If an accident were to occur, as it did at Chernobyl, the fuel cannot easily be extracted leading to the possibility of runaway heat, melt-down and explosion. For practical fusion reactions to be recreated on earth it had been assumed to require temperatures approximating the heat of the sun. That is difficult to achieve and has challenged investigators for three decades with results that are costly, perhaps encouraging but short of sustained net energy production.

The beauty of the Pons/Fleischmann experiments lie in their simplicity and I will leave it to them to describe them to you.

The importance of the promise of so-called "solid-state fusion" is enormous. The problem of nuclear waste is largely eliminated. The cost of the fuel (heavy water) is moderate and its availability essentially unlimited (there is one molecule of heavy water in every 38,000 molecules of sea water). The already small likelihood of a fission nuclear plant accident is further reduced in a fusion plant were one to be successfully built. As fossil fuel burning is reduced, a major contribution is made to the growing problem of carbon dioxide pollution and the attendant suspected warming of the earth (greenhouse effect). Acid rain is eliminated when sulfur containing coal burning is reduced. Finally, the world is provided with cheap energy for all the purposes to which cheap energy can be put and coal, oil, and gas are saved for the valuable chemicals they provide to produce drugs, plastics, fertilizers and the like.

What lead to the Utah experiments? A capacity to see an old problem from new perspectives was required. Chemists, electrochemists, looked at a problem traditionally reserved to physicists. In fact there-in lies some of the humor and bite of the scientific controversy that is raging. I would like to think that it may not be by chance that it happened in Utah, at a university which has encouraged unorthodox thinking while being viewed by the world as a conservative, even socially orthodox place. There in fact may be something valuable in isolation from more traditional centers. America has prospered and innovated at the frontier and the University of Utah is still a frontier that attracts faculty who highly value their intellectual freedom.

As these studies are confirmed, they will need to be moved rapidly to developmental and commercial phases or we will lose their harvest. In a real sense we are obliged to build the first floor of engineering and the second floor of commercialization in this edifice at the same time we build the foundation of scientific understanding. Ira Magaziner will develop this important concept.

In conclusion, Utah and the University of Utah, working with able faculty of Brigham Young University, Utah State University, and faculty gathered world wide, is a good place to promote rapid and novel development. Our political and social system is remarkably flexible, a throw-back to frontier times when well-intentioned leaders could identify, surround and solve problems quickly. The environment supports vigorous thinking and living. Additionally we have uninhabited, remote areas where special experiments can be conducted 20 minutes from both the university and an international airport. The state has appropriated $5 million to assist. $1.1 million has already been raised privately with the promise of much more. We are prepared to build a novel consortium of federal, corporate, state and university resources if you choose to join us. Without federal participation the race for competitive leadership will be handicapped.

# CHASE N. PETERSON

Chase Peterson was appointed to be the 11th president of the University of Utah in June of 1983. Dr. Peterson grew up in Logan, Utah, earned an A.B. degree from Harvard College, and received his M.D. from Harvard Medical School. He followed this with post-graduate work at the Yale-New Haven Medical Center and service with the U.S. Army Medical Corps in Germany. Dr. Peterson returned to Utah in 1962 to practice at the Salt Lake Clinic and teach at LDS Hospital and the University of Utah Medical School. In 1967 he was asked to be dean of admissions at Harvard, and after five years to be vice president for alumni affairs and development. In 1978 Dr. Peterson returned to Utah to become vice president for health sciences, a position he held until his current appointment. In November 1988 he was named chairman of the National Association of State Universities and Land-Grant Colleges, the lead organization of 149 major public universities. He and his wife Grethe Ballif have three children and two grandchildren.

12/88

# CHASE NEBEKER PETERSON

University of Utah   203 Park Building
Salt Lake City, Utah  84112   #(801) 581-5701

## Education

| | |
|---|---|
| 1936 - 1945 | Utah Public Schools |
| 1945 - 1948 | Middlesex School, Concord, Massachusetts<br>Middlesex National Scholarship<br>Class President and Valedictorian |
| 1948 - 1952 | Harvard College, Cambridge, Massachusetts<br>A.B., American Government and Pre-Medicine<br>Harvard National Scholarship<br>Frothingham Senior Award for "contribution to Harvard College"<br>First Marshal of graduating class |
| 1952 - 1956 | Harvard Medical School, Boston, Massachusetts<br>M.D.<br>Graduate National Scholarship<br>Massachusetts Medical Society Award to the HMS graduate "demonstrating promise for clinical medicine" |
| 1956 - 1958 | Yale-New Haven Medical Center, New Haven, Connecticut<br>1956 - 1957 Internship, Medicine<br>1957 - 1958 Assistant Resident, Internal Medicine<br>(1958 - 1960 U.S. Army Medical Corps, Germany)<br>1960 - 1961 Senior Assistant Resident, Internal Medicine<br>1961 - 1962 Fellow in Metabolism (Endocrinology and Renal Disease) |

## Professional Employment

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| 1962 - 1967 | Medical Practice (Salt Lake Clinic) and Teaching (LDS Hospital & University of Utah Medical School) |
| 1967 - 1972 | Dean of Admissions and Financial Aids, Chair of Faculty Committee on Admissions and Scholarship, and Member of Faculty of Arts and Sciences, Harvard College |
| 1972 - 1978 | Vice President, Alumni Affairs and Development, Harvard University |
| 1978 - 1983 | Vice President for Health Sciences, University of Utah |
| 1983 - present | President, University of Utah |

Personal

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|---|---|
| Born | Logan, Utah; 27 December 1929 |
| Married | Ane Grethe Ballif, 1956 |
| Children | Erika Elizabeth, 1959 |
| | Stuart Ballif, 1961 |
| | Edward Chase, 1965 |

Professional Activities

Chairperson, National Association of State Universities and Land-Grant Colleges 1988-89
Chairman of Council of Presidents, 1986-87

Research Administration
- Coordinator, Southern Utah Radioactive Fallout Study, 1978 - 1983
- University Coordinator and Spokesman, Utah Artificial Heart Project, 1981 - 1983
- Review panelist, Impact of Changes in Federal Policy on Academic Health Centers, AAHC, Washington, D.C., 1981 - 1982;
  Study of the Pros and Cons of Separation of University-Owned Hospitals from the University, AAHC-AAMC, Washington, D.C., 1986-87
- Advisory Council, U.S. Office of Technology Assessment, 1986 - present

Lectures, Symposia and Conferences
- Reynolds Lecture, University of Utah, 1981
- Last Lecture Series, University of Utah, 1981, 1983
  Brigham Young University, 1979
  Westminster College, 1978 (Humanities Lecture)
- National Center for the Humanities Symposia on Bioethics, North Carolina, 1982
- Health Effects of Nuclear War, Conference Chairman, University of Utah, 1981
- Universal National Service, Ford Foundation Study, Washington, D.C., 1983
- Institutional Program Review, American Cancer Society, New York City, 1983
- Project HOPE Conference, "What Is Right About American Medical Care?", Washington, D.C., 1985
- Columbia University Conference on "Managing Our Miracles: Health Care in America", Philadelphia, Pennsylvania, 1985, resulting in PBS Series, "Managing Our Miracles", 1986-87

Board of Directors

Utah Power and Light Company, 1985 - present
Tanner Lectures on Human Values, Chairman, 1983 - present
Utah Symphony, 1983 - present
First Security Corporation, 1983 - present
Intermountain Health Care, 1978 - 1986
Bay Bank/Harvard Trust Co., 1977 - 1978
Trustee, Middlesex School, 1972 - 1975
Harvard Club of Boston, 1970 - 1975

Associations

Porcellian Club, Harvard College
Boylston Medical Society, Harvard Medical School
Aesculapian Club, Boston, Massachusetts
Tavern Club, Boston, Massachusetts, 1972-78
Boston Library Society, 1972-78
Old Docs Society, Boston, Massachusetts, 1974-78
County and State Medical Societies of Utah and Massachusetts, current
Timpanogos Club of Utah, current
Cannon-Hinckley History Club, Utah, current
Harvard Clubs, Boston (life member), New York, current
Phi Beta Kappa, University of Utah (honorary)
Alpha Omega Alpha, University of Utah (faculty)
American Board of Internal Medicine

The CHAIRMAN. I thank the distinguished gentleman.

We will now suspend because we're on a second roll call vote on the rule for the supplemental appropriation. Would the Members please return as quickly as possible so we can conclude our work this afternoon.

[Whereupon, the Committee was in recess.]

DISCUSSION

The CHAIRMAN. The Committee will come to order.

When we recessed to go vote, we were hearing from Dr. Peterson, and I believe you had just concluded.

Dr. PETERSON. I have. We'll take any questions, Mr. Chairman.

The CHAIRMAN. First let me recognize Mr. Owens and then we will see if there's any further questions.

Mr. OWENS. Mr. Chairman, I would like, before my very brief remarks, to point out that the university approached Mr. Magaziner earlier on to hire him as a consultant, but he elected to provide his services without charge, a rather unusual circumstance for a person of his caliber and fees.

It's a great honor for me, Mr. Chairman, representing Utah's Second District, and very difficult to contain the excitement I feel in having been able to bring you today not only a marvelous breakthrough, but a truly innovative legislative idea. As has been discussed, the world's industrial record and history of the last 20 years has been one of transfer of America's discoveries and inventions to Asia and Europe, where superior vision and engineering and marketing have given to others most of the commercial benefit of American ingenuity.

Dr. Magaziner has listed them—the microwave, the color television, the VCR, computers, supercomputers, memory chips, commercial satellites, satellites, solar cells, superconductors. The list goes on and on. A $150 billion trade deficit this year as we live on borrowing from our trading partners. How long will we let this go on? How long will we refuse to use the approaches and tools of the 1980s so that we can win?

Some say solid-state fusion may be man's greatest discovery since fire. Others say, as I do, that it may also be the innovation to protect and perpetuate the Earth's dying life support system, more importantly than the possible salvation of the dying industrial superiority of America. Man cannot stand another century like the last. In those 100 years, we have consumed more of the nonrenewable richness of the Earth than was used during all of man's previous history. We polluted and poisoned our environment with its use, and it literally threatens our continued existence.

The revolutionary discovery, solid state fusion, arrives simultaneously with our entry into the age of true environmental alarm. So, bursting with pride, Utah's Congressional Delegation brings to this Committee the prospect of a second economic chance and a second environmental opportunity. This morning we tell you not only of the discovery which may revolutionize the world's energy system, but more importantly, it may be the answer to the preservation of our home, Planet Earth.

Within the next two weeks, the United Utah Congressional Delegation will present you with an innovative legislative plan, one which will precipitate a whole new concept for a national partnership for action. It will combine private and public investment and the opportunity for America to develop, engineer and champion the most far-reaching innovation of our time.

So I thank you, Mr. Chairman, and Members of the Committee, for your time today, and in advance for your interest in the legislation which we will soon offer.

The CHAIRMAN. I thank our distinguished colleague for his participation and his help in putting this hearing together, by the way. I want to thank you very much because I think you've served a great purpose not only as a distinguished representative from Utah, but on behalf of the Congress of the United States. I'm very appreciative of that.

Mr. OWENS. Thank you. It was a great pleasure, Mr. Chairman. Thank you for your very personal and intense interest in this issue.

The CHAIRMAN. The Chair is going to recognize first the distinguished gentleman from California, Mr. Brown, and then he's going to recognize the distinguished gentleman from California, Mr. Packard, because both of them have other commitments.

The Chair recognizes the distinguished gentleman from California, Mr. Brown.

Mr. BROWN. The question that I raised earlier of Dr. Pons and Dr. Fleischmann maybe is better addressed to you gentleman. You have obviously been studying the possible courses of action, the establishment of an institute, as Mr. Owens has indicated, but have you given any thought to the level of funding that might be desirable at this particular stage in time, and the rate at which we should undertake to move, the speed with which we should move in this area?

Dr. PETERSON. We have, Mr. Brown. The first limiting factor is our own wisdom and intelligence to be able to put together a plan that we all can look at and shape wisely.

The next limiting factor is money, and we have raised $5 million from the State of Utah, 1.1 of private funds has been dedicated. We think there can be considerable funds from the private sector. We are working with companies and have talked about numbers of dollars that might be put into this sort of thing, some after confirmation, some even during confirmation of the scientific significance of this.

The figure that comes to mind is $25 million from the Federal Government. Maybe that needs to be $125 million some day, but that's of not any importance right now. Twenty-five million dollars would allow us to start the "onion" growing, with State and private sources. Ultimately, I would imagine it would be a minimum of $100,000, with the majority coming from non-Federal sources.

Mr. BROWN. A hundred million.

Dr. PETERSON. Million. I'm in Washington now. I've got to remember that. A hundred million dollars would be probably what we would expect to raise, with a minority portion of that being from the Federal Government.

Mr. BROWN. And we should move promptly, this year?

Dr. PETERSON. The third point is, we should move very quickly. We propose to have ideas for your consideration literally within the next week or two. Mr. Magaziner might want to comment.

Mr. MAGAZINER. I think the time issue is very, very important here. You know, we have consulted in Japan, we have consulted with Japanese companies in the past, and we understand the kind of effort that the Japanese are now devoting to this discovery, even before they've replicated it. I think if we, in the normal course of events, wait a year to consider this, or a year-and-a-half or whatever, I think we're going to start off the blocks late. So I would suggest urgency.

And if something does turn up six months from now where the science is not what we hope and think it is, then you don't have to spend all the money. But I would suggest that you get the process going and get the thing going as if it's going to succeed.

Just like the most successful company in the world, over half the projects they'll try to invest in don't work. You know, if that causes you not to invest in anything, then you don't get anywhere. So I would suggest you move very quickly.

Mr. BROWN. Well, very quickly around here is not all that fast. But I think, just by way of background, you should know that this Committee has been looking at the problem that you described so eloquently for several years, and included in the Trade Bill last year as part of our contribution a proposal for authorizing the development of advanced technology initiatives which fit this project like a glove. That bill was signed by the President last September, I think, approximately, but no money was requested in the budget for this year and there doesn't seem to be any process for requesting it for next year.

We have prototype programs in the National Science Foundation and a few other places, funded at a low level, most of which could not be diverted as a matter of fact. But in an emergency situation—and you have created—this situation has created a sense of national urgency. We possibly could get the administration to request funding for this generic advanced technology, and a major portion of that could be devoted to this, if required.

This is what I think probably the path that this Committee would like to follow, because we also need to look at high definition television, superconductivity, several other technology areas, in which the problems are identical. We need to move quickly to grasp the commercial opportunities as the research base expands.

Thank you, Mr. Chairman.

Dr. PETERSON. Mr. Brown, could I ask Mr. Magaziner to share with you just an anecdote of his investigations of what's going on abroad. You've got to talk about what time of night you called.

Mr. MAGAZINER. When I first knew I would be testifying today, I wanted to try to get some detail on what was going on in Japan in this area. I phoned a colleague over there at what was very late at night, about 11:00 p.m. their time, at his home. I asked him to try to make some inquires to some friends of his in corporate research and development activities in Japan.

He found them in the laboratory, and then somebody also with MITI, who was also at work at 11:00 at night, working on the plan for this, that they're going to develop. Because as you may know, they form multidiciplinary committees made up of companies and university research labs and so on, and MITI is now formulating a plan to do that. And both in the case of this one company laboratory, and also in the case of this person associated with MITI, they were working at it at the very late hours. So when I say there's some urgency, that's what drives me to say that.

Mr. BROWN. Thank you.

The CHAIRMAN. If the gentleman will yield, on the term of urgency, while the Chairman was over voting—and we just voted on the rule for the Supplemental Appropriation—we were faced with the following news: that as they will debate the issue this afternoon, there will be two amendments that will be offered. One amendment will be offered by Mr. Conte, which will cut $113 million from this particular Committee's budget—or, rather, funding—and a supplemental appropriation, direct funding. This includes $52-61 million in NASA, $30-35 million in DOE, $11-13 million in NSF, $2-3 million in EPA, and about a million in NIST and FAA.

Then that will be followed by an amendment by Mr. Foley, and Mr. Foley won't cut the 113 but he will cut $96 million. What you're telling us, believe me, with the greatest of respect, we thoroughly understand on this Committee, on a totally bipartisan basis, that the new wealth of tomorrow is not going to be created by cutting our throats today.

So I'm going to have to leave. That's why we've changed our schedule a little bit and Mrs. Lloyd is going to have to take over here while we go to fight on the floor, to get across to those people that it isn't only the $96 million or the $113 million that immediately exacerbates the 1990 budget, which is the thing we're discussing now, which will cut an additional $100—automatically cut us $102 million before we get any cuts further.

What I'm simply trying to say to American citizens is that somebody has got to say to the Congress of the United States, both the House and the Senate, that if you're going to create the new wealth of tomorrow and you're going to compete, then you've got to put the resources where they should be put. Pardon my enthusiasm, pardon my aggravation.

And let me call upon the distinguished gentleman from California, Mr. Packard.

Mr. PACKARD. Thank you. The last word I had, Mr. Chairman, was that Mr. Conte may be withdrawing his amendment—

The CHAIRMAN. Praise be to the Lord. Now I'll have to go to work on Mr. Foley.

Mr. PACKARD. That's your side of the aisle, Mr. Chairman.

The CHAIRMAN. I know.

Mr. PACKARD. I appreciate my colleagues allowing me to go out of order a little bit. I have a meeting with Mr. Lujan shortly and I needed to ask a few questions here.

Last year we spent—in fact, the last two years, I co-chaired the Technology Policy Task Force, and certainly two major items of discussion during that year of hearings was the question of applied technology and how we can do what has been suggested here in terms of trying to keep ahead or certainly keep up with the Japanese and the European marketplace, taking our basic research and transferring it into a marketable product. We don't do well at that and we discussed that at length.

The other area was an area I wanted to discuss with the president here of the University, and that is, the distribution of NSF moneys. How much money has the University of Utah received in the past year, to your knowledge, Dr. Peterson, of grant money from the NSF? Do you have an idea?

Dr. PETERSON. Our total research funding level is between $95-100 million from outside research, and NSF is about $23 million of that.

Mr. PACKARD. My research in the last couple of days has indicated that almost 60 percent of the total—and it goes into the billions of dollars that is distributed—goes to about 20 universities, and that the peer review committee or panel that determines the distribution of these funds are representatives in the bulk of the cases of these 20 top universities. It's an incestuous—in other words, the type of arrangement where the money goes to those that make the decisions.

And there's good reason for some of this being done. There is good reason. But it means that small universities, universities that are not in the inner circle of research dollars coming through NSF, are left out and do not get the money. I have them in my district and we have them in Utah, we have them across this country, that are doing remarkable things—this is more of a statement almost than it is a question—that are doing remarkable research, but never have the benefit of national funds to assist them.

I think that if the truth were known, the University of Utah and other schools that are doing some remarkable research in a variety of areas that this Committee has some interest in, is not getting the funds because, again, it's going to predominant universities that historically have gotten huge sums, in some instances one to two billion dollars per year.

Let me ask you this question. The peer review panel that makes the decisions in terms of distribution of the funds that come out of NSF, has the University of Utah ever been contacted or have they ever been involved in that peer review panel, to your knowledge?

Dr. PETERSON. Yes, they would have been. Vice President Brothy, could you give me any—If you're asking about the number of times our faculty has served on peer reviews, that would, of course, be hundreds of times. Is there any particular panel that would be pertinent here?

But your questions are well put, because the University of Utah may lie in about the mid-zone and maybe we can look both ways. We are probably the 30th ranking university in the country in terms of outside research funding. I happen to be Chairman this year of the National Association of State Universities and Land Grant Colleges, and they include the Wisconsins and Michigans and so forth, and they include 150 other State universities around the country.

We very much want to support the basic theory of peer review, because it has served the Nation well; namely, to have presumably unbiased, objective people making judgments, particularly about the awarding of grants. Now, whether there needs to be something else in the awarding of facilities, that's a battle that is going on, and it would be premature for me to enter into that battle.

Mr. PACKARD. I understand, though, that those are different—that's apples and oranges, the money, the grants for facilities, versus the grants for research.

Dr. PETERSON. Well, some people think they are and some people think they aren't, but that's the issue and it's being discussed widely, and no one has quite come to grips with where the compromise ought to be.

Mr. PACKARD. So in your view, you are not uncomfortable with the arrangement as it's now established, but in your view, are there universities that do good research work that simply cannot get the funds?

Dr. PETERSON. I think that Dr. Pons himself and his research would illustrate perhaps the best answer to your question. He has been funded by peer review funding. He has also been funded, if I understand it correctly, by funds that were directly assigned to him. Is Stan here?

The way that Dr. Pons would have me say this—and I think I agree—is that ongoing research, research that has achieved a level of respectability and recognition, is far better funded by peer review. But there are many things that are innovative and new and haven't reached the level of acceptance, and it is wise for the Government to have an alternative pathway. I believe that Dr. Pons' funding through the Office of Naval Research has been when scientists got together, simply talked over what ought to be done, and they said let's fund it.

Mr. PACKARD. In our national policy task force last year, it was obvious to some of us at least that universities that wanted to break into the research opportunity simply could not get in because they had not had an experience level, they had not had a staff and researchers that were—and thus they were, almost by nature of the structure, were locked out of any opportunity to begin, that they couldn't get into the entry level of some of the funded programs.

Dr. PETERSON. I think that's the point, the entry level.

I've served on an NIH panel that tried to think through this issue, as to how we might get funding for facilities and biomedical research, and what would the criteria be for funding. I've worked with Dr. Langenberg, who is the distinguished President of the University of Illinois at Chicago, who wrote a paper for AAU two or three years ago, and they all were trying to look at this zone between the clearly established people and those that have the capacity to become established with a bit of encouragement.

Mr. PACKARD. Madam Chairman, I appreciate the opportunity. I realize that this is not the time to make those policy decisions, but I think, because of the unique way in which this research program has gone, without Federal funding, that it was a time to at least evaluate the sizeable amount of money that goes through NSF to universities and often leads to these kinds of breakthroughs.

Thank you.

Dr. PETERSON. If we can ever pay back Dr. Pons the $100,000 we owe him for the research he did on his own, then perhaps he could be applied to invest that in a new investigator.

Mrs. LLOYD. (Presiding.) Thank you very much.

Indeed, one of the frustrations of chairing the Energy, Research and Development Subcommittee is to receive testimony such as we have received, not on this magnitude, but we recognize fully that we are producing more Nobel Laureates and fewer patents as the years go by. It is a great frustration.

One of the major objectives of the Subcommittee in this 101st Congress is to really produce some meaningful legislation so that we can transfer more technology from our universities and from our national laboratories to the marketplace. You're indeed right, Mr. Magaziner. There is no way we'll ever get any sort of control over our $135 billion trade deficit unless we do take advantage of what's coming from our national laboratories and our universities, that truly they are the best kept secrets of our country, but they are also the storehouse, the treasure house of the new wealth of our country.

In the last Congress, my bill for speeding up technology transfer did pass the House of Representatives. It died in the Senate. We will be moving and we will be having hearings on technology transfer, so that we can legislate our technologies from our laboratories and universities find their way to the marketplace, that we can wed the two before the Japanese and our other industrialized partners captivate the marketplace, as we have seen here.

At the same time, I think it's true that there is a collision course between industry people, between business and the Federal Government. It's not true in other countries. We do assume an adversarial role so many times. And this is not always the fault of the Federal Government, that private industry is afraid for anyone to get close to anything that they're developing, and we're so afraid that somebody is going to take advantage of what we've developed that we fiddle around and other countries beat us to the marketplace. I hope that we can do a better job in this area because, when we held our hearings on superconductivity, we described the technology as the last frontier, that if we don't move ahead with this and the Japanese beat us with this one also, then we're gone. And here's another prime example of our responsibility as a nation.

Mr. Magaziner, what do you feel is the appropriate role of the Federal Government in terms of furthering such efforts? You spoke of a research institute. I think that really sort of scratched the surface.

Mr. MAGAZINER. I think with respect to this particular project—and I think it could serve as a model, which is one reason why I'm so interested in it—is I think you can have a basic research institute and an applied research institute housed in the same house, if you will, and have it be one where there can be corporate funding for the endowment which is in trade for corporations then getting some access to patents at a favored rate if they're willing to underwrite some of the basic research.

The Federal Government I think can play two roles. One is to help underwrite the basic research with grants, but also to do something which is done widely around the world, which is to provide these conditionally reimuburseable loans for the actual commercial development of a product. That's a model that's been used effectively in about a dozen other countries. We have it on a very, very small scale in a couple of States here, but that's all.

I think the Federal Government role really is not so much one of the one who bales out tons of money. It's more the catalyst role, I think the kind of role that helps get the thing off the ground, because that's something that private industry would have difficulty doing on its own.

Mrs. LLOYD. Now we're back to the question when it comes up to commercialization of any technology, that you have the Federal Government subsidizing private industry. That's always a real stumbling block.

Mr. MAGAZINER. Right. And I think the way to handle that is, if you do it through loan mechanisms, where the Federal Government is paid back on a sliding scale, depending upon the success of the project, as they do in Japan, as they do in France, as they do in Germany, Sweden—

Mrs. LLOYD. But you can see this is a hindrance that we run into.

Mr. MAGAZINER. Sure. But I think—you know, if it's one where there can be a return to the Federal Government for its money, where those projects operate in other countries, they operate in the black, so that you don't have a net outlay of Federal money, and I think something along that model might be palatable.

You know, this is not an ideological question. I mean, we don't have to debate ideologies. It's really a very practical matter about how you compete successfully. We may wish that some of our competitors elsewhere were doing it differently, but they're not. They're doing what they're doing, and they're succeeding with it. We have to react to that.

Mrs. LLOYD. Dr. Peterson, I have one question for you. I realize my time is up.

To what extent is concern over intellectual property rights and patent applications affecting your ability to disseminate the technical information as a result of the research that was done?

Dr. PETERSON. We have been sharing that information. Dr. Pons and Dr. Fleischmann have spoken at six or seven major international meetings since the first announcement. They are actively preparing new papers.

You are correct in identifying the issue of how do you protect yourself on patents and still have open, academic publications. We think both can be done. Ideas are patented and then put in publications. As you know, the patenting process can be done almost overnight.

Mrs. LLOYD. You aren't concerned over the intellectual property rights at this point?

Dr. PETERSON. Yes, we are concerned over the intellectual property rights, and we do have patents on them, but we don't think that's holding up dissemination of information.

Mrs. LLOYD. But you do not feel this is an impediment to the dissemination?

Dr. PETERSON. It hasn't, as I've observed it from some distance. If you're talking about hours and days, yes, but not weeks and months.

Mr. MAGAZINER. Could I add one thing to that, and that is the question of patent rights in other countries. There is a long industrial history now, particularly in Japan, but also in certain countries of Europe, of American firms having difficulty establishing patents in as easy a fashion as foreign companies can in this country. I would suggest that this again may be one area where we want to take a very close scrutiny on how the Japanese patent process, the German patent process, worked with these patents, because they are things that were very clearly developed here. I think it may be something the Congress wants to keep an eye on, as to how well these patents are granted abroad.

Mrs. LLOYD. Are you saying that patents to our technologies are granted in our country to other countries faster than we can—

Mr. MAGAZINER. There's no question. The whole process is very different. Also, even in—

Mrs. LLOYD. No, you misunderstood me, I believe.

Are you saying they can obtain a patent on our technology in the United States—

Mr. MAGAZINER. No, no. Sorry. I'm suggesting this is something where we're talking about an international competition that may develop, and if we can't make our patents hold in Japan or in Europe because of the way they conduct their patent processes, that's something that could well be unfair in terms of the way that trade should take place and something we ought to look at.

Mrs. LLOYD. Thank you very much.

Mr. KETCHAM. Madam Chairman, could you yield to me for just one second?

Mrs. LLOYD. Yes. I yield to Mr. Ketcham.

Mr. KETCHAM. In your testimony, sir, you mentioned conditionally reimburseable loans.

Mr. MAGAZINER. Yes.

Mr. KETCHAM. Could you provide additional research on that for the record?

Mr. MAGAZINER. Sure, I could give you some documentation on that.

Mr. KETCHAM. Thank you.

Thank you, Mr. Chairman.

Mrs. LLOYD. Mr. Morrison.

Mr. RITTER. I think I was here.

Mrs. LLOYD. All right, I will recognize you, then. Mr. Ritter.

Mr. RITTER. Thank you, Madam Chairman.

As defined by our Chairman as the "resident stickler" on this Committee, maybe I should just dive right in with a question that I think goes to the core.

First of all, before I do, I would like to commend you, Mr. Magaziner. I think you have said in your testimony something I have been trying to say for ten years in Congress, and you said it a heck of a lot better than I have. It is just an incredible short synopsis of where we're not doing as well as we should be doing and how to perhaps do better. I think you're hooking up with the Utah group is a great combination.

But, you know, two years ago we were in this Committee room, we had a full committee hearing, I recall; we had the television cameras whirring. We had a lot of fanfare surrounding the covers of Business Week, Time Magazine, Newsweek, front pages of the New York Times and Washington Post—sound familiar? It was the innovations, the inventions, the new research which was coming to light in high temperature superconductivity. We had a great deal of activity around Washington as well. The President finally got involved. There was a White House conference, there was the appointment of a Superconductivity Chairman of a group of wise men and women, and that report was called the Gomory Report. It thus far has not been implemented. Some excellent strategies of the type not dissimilar to what you're recommending have not been implemented.

This year, there is a fantastic center, almost one for one, a different level of the science. It's more technology, it's more product, but it deals with the high definition television, saying, you know, the front pages, the evening news. That's out there, too. We are in the process of seeking to design a policy there.

Then you have crucial solid state technologies. As you know, the Japanese have really—this is the underpinning of the electronics and the soon to be photonics, opto- electronics revolution. Tremendous work has been done in putting together collaborative efforts, right, in our competitor nations.

Now, what makes this, particularly at this stage of the game, what makes this particular issue stand above high temperature superconductivity, high definition television, crucial solid state sciences, photonics and optoelectronics? What makes cold fusion the one to go for now?

Mr. MAGAZINER. I don't—First of all, I agree with what you say, and I've been watching this for ten years, as you have. I don't think it's so much an issue of, is this science more important necessarily than superconductivity, or commercially more important that high definition television. I don't think that's what makes this different.

From my point of view, though, one thing that does make this different is that you have a group of people in Utah—and this is one reason why I'm willing to volunteer my services to them—you have a group of people in Utah who are determined to put together an effort in the way that it should be done, and they have already started to do that. They are already making the steps to set up the institute. You have a legislative grant. You have a number of things in action which I think give this an opportunity to kind of develop and move ahead in ways that the others didn't have.

Mr. RITTER. What makes that different, if I might just interrupt. I don't think it's that different from the—it's different because it's a different subject. But I mean the parameters are not that different from—

Mr. MAGAZINER. No, theoretically it shouldn't be different from, say, the superconductor situation. But, in fact, that got off the blocks very slowly in terms of thinking about commercialization on the part of those who were involved in it, whereas I think the people—

Mr. RITTER. Not in Japan it didn't.

Mr. MAGAZINER. No, here in this country.

Mr. RITTER. But I mean the possibilities exist that we will commercialize high temperature superconductors far quicker than we'll commercialize this. I mean, you don't know.

Mr. MAGAZINER. That's right. That's why you have to proceed on a number of these things at once. No Government panel or no Government agency should be trying to pick what's going to win and lose in a future technology. That's a losing game. I think what you've got to do is say that when there are major centers of activity going on, you need to fund them, and you need to monitor carefully, so that when something does turn out to be a blind alley, you cut back. But you've got to fund a number of these steps.

Mr. RITTER. Basically, according to your argument, though, we need to go ahead with high temperature superconductivity; we need to go ahead with high definition television; we need to go ahead with the material solid state revolution—

Mr. MAGAZINER. Yes, because—I mean, if you look at the programs—why can't we do it if the Europeans and the Japanese can? The European programs—Eureka, Race, Brite, Esprit—are funding 500 different projects right now, in all the technologies you're talking about, spending a couple of billion dollars a year on commercial R&D. The agency for industrial science and technology, MITI, is doing the same thing in Japan. They're not sitting there deciding one or another. They're funding all of them.

You know, we're a bigger country. Why can't we do that if we care about our children? I mean, that's what it comes down to.

Mr. RITTER. Basically, that's what we have to do, too.

Mr. MAGAZINER. Yes, absolutely.

Mr. RITTER. You should get the message down to the floor of the House with an amendment coming up.

Thank you, Madam Chairman. I yield back my time.

Mrs. LLOYD. Mr. Morrison.

Mr. MORRISON. Thank you, Madam Chairman.

Dr. Peterson, I guess a comment as much as anything. I think the need to verify is absolutely paramount right now, and I know from my personal discussions with you and our two eminent doctors, that that's uppermost in your mind.

I just would renew an offer made by Pacific Northwest Laboratories to bring in equipment at no cost to you and be part of that reverification procedure. I suppose you're getting that sort of offer from a number of different directions.

Dr. PETERSON. We are, but that doesn't mean that isn't critically important. Dr. Pons mentioned that he's setting up the trade of equipment and people with Los Alamos, which would be a comparable opportunity—perhaps the wrong State, but still a good place.

Mr. MORRISON. Well, perhaps not quite comparable, either, but that's fine.

[Laughter.]

I think all of us are just so eager to do what we can to help, and this is one offer that is there and outstanding. I would be pleased to help with that.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Morrison.

Mr. Schiff. And I would like to remind the panelists that we do have a key witness from Stanford that has a plane to catch, in our next group. We want to get him on as soon as we can.

Mr. SCHIFF. I'll be brief.

Mr. Magaziner, since joining the Congress, I have heard discussions that you've outlined about the apparent—the fact that Europe and Japan appear to be ahead of us in commercialization of scientific ideas. That subject has particularly come up on my two subcommittees, one Energy, Research and Development, because of the superconductor semicollidor proposal, and one on the Space Subcommittee because of the manned space station proposal, the argument being the American taxpayers will pay for the basic research and somebody else will then commercialize it and reap the economic rewards. I'd like to ask you a couple of questions along those lines.

The first is, our Chair asked some questions about the patents. Are our patents under international agreements not strong enough, to where we develop an idea, to protect those ideas against commercialization in other countries?

Mr. MAGAZINER. Well, there are two things that happen. One is that in many cases we've, I think, probably too freely licensed our patents to others, and, you know, you maybe get the couple of percent profit that you get from a patent but you don't get the jobs and the full economic benefit. But also there have been some cases—and we described one regarding coining optical waveguide fibers in a book that I've just written, where basically the patent processes in Japan and Germany in particular are much harder to apply for and be accepted, and secondly, where in some cases as a matter of government policy patents are held up and not awarded so that a local company can go ahead and do something.

Now, that local company then doesn't have the option to sell in the United States, but they can sell elsewhere in the world. And so I think there is an issue with respect to reciprocity on patents which I think we ought to look at.

Mr. SCHIFF. You don't feel that the international accords are strong enough when there is an American patent on a particular idea?

Mr. MAGAZINER. No. I mean, a number of these international accords on patents, and also I would point to GATT and a number of other international accords, where a number of our foreign competitors could write an encyclopedia on how to get around them. I mean, you know, with GATT, when it comes down to financing of exports and so on, there are all kinds of ways to get around it, and the same with patents.

I would commend to you this one story, but also a number of others that I could tell about the process that American companies had to go through to try to file their patents in Japan, and the delays that took place and so on and so forth.

Mr. SCHIFF. I'm going to be brief here because of the Chair's observations on time. But I would like to ask, did the Japanese and Europeans spend a great deal of government money on commercialized R&D projects?

Mr. MAGAZINER. Absolutely. That's the main focus of most of their activities. All the programs—Eureka, Race, Esprit—in Europe are all devoted to commercial research and development. They have to involve at least two companies working together and preferably working with some research institute that's doing applied research with them. And it's precompetitive, but it's shared research.

For example, somebody mentioned high definition television. In Europe they've been spending a couple hundred million a year for a number of years now in cooperation with Thompson and Phillips and using these government-funded activities purely on commercial research and development.

The same is true in Japan. The agency for industrial science and technology is within the Ministry of International Trade and Industry, and it's the main coordinator of these research projects, and they're primarily commercial research and development. Now, that's not to say you should ignore the basic research. I mean, basic research needs to be funded fully. But they take that other step of funding the commercial research and development and they'll give money off into companies to do it.

Mr. SCHIFF. That would be a rather historic departure from the way the American Government has viewed its relationship with our industry, wouldn't it?

Mr. MAGAZINER. Well, you know, it may well be not the American way to do things that way; on the other hand, it's not the American way to be a second-rate economic power, either. I think if the world environment has changed, the competitive environment has changed in the way things are being done, I think we have to adapt. It doesn't mean we should copy somebody else's model. I think we can make our own way of doing this. But I think we have to recognize that commercial research and development has to be publicly funded to match what the others are doing. It's not a question of do we think it's right or wrong. It's a question of matching what others are doing.

Mr. SCHIFF. May I ask one last question, Madam Chair?

Mrs. LLOYD. I regret the time is up. I'm very sorry.

Mr. SCHIFF. All right.

Mrs. LLOYD. Mr. Schiff, you may submit additional questions in writing for our witnesses for the record.

Mr. SCHIFF. Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much.

Dr. Peterson and Mr. Magaziner, we thank you for your contribution to our hearings today, and we will be submitting additional questions to you in writing for you to respond to for the record.

Congressman Owens, we appreciate you being with us, and we invite you to sit with the panel.

Mr. OWENS. Thank you very much, Madam Chairman.

Mr. MAGAZINER. Thank you very much.

Dr. PETERSON. Thank you.

[The material referred to follows:]

**Questions for Mr. Chase Peterson**

1. To what extent is concern over intellectual property rights and patent applications affecting your ability to disseminate te4chnical information on the results of the research work at the University of Utah, and to give access to National Laboratories, such as Los Alamos or Oak Ridge, to your scientists and experimental supporters to obtain the details apparently necessary to verify the results under reprocible conditions.

2. We also understand that a good friend of this committee, Dr. James Fletcher Former Administrator of the Aeronautics and Space Administration, has decided to return to Utah to direct the Research Program in this area. Could you describe for us the role that Dr. Fletcher will play in this whole effort and what is envisioned for the program?

ANSWERS:

1. Upon the advice of our patent counsel, it is not possible for the University of Utah to share research results with other laboratories, particularly national laboratories, until the information has been incorporated into a patent application and the application is on file in the patent office. After that, dissemination to others can, and has, been done. This is the usual conflict between science and commercial interest, exacerbated in this case by the potential importance of fusion technology.

2. Dr. James Fletcher has agreed to act as an unpaid advisor to the University of Utah fusion effort, but not as a full time director. He plans to reside both in the Washington, DC area and in Salt Lake City.

Mrs. LLOYD. Our next panel includes Dr. Steven Jones, Department of Physics and Astronomy at Brigham Young University; Dr. Daniel Decker, Chairman, Department of Physics and Astronomy, Brigham Young University; Professor Robert Huggins, Stanford University, Materials Science and Engineering Department; Professor George Miley, Director, Fusion Studies Program, University of Illinois; and Dr. Mike Saltmarsh, Fusion Energy Program, Oak Ridge National Laboratory, Oak Ridge, TN.

Gentlemen, we welcome you today. We also welcome Congressman Nielson, who will introduce Dr. Jones.

Professor Huggins, you may go first because we know that you have a plane to catch. We apologize for delaying you. We hope you do make your plane, but we also hope that we have the advantage of your testimony as well. So you please proceed.

## STATEMENT OF DR. ROBERT A. HUGGINS, DEPARTMENT OF MATERIALS SCIENCE & ENGINEERING, STANFORD UNIVERSITY, STANFORD, CALIFORNIA

Dr. HUGGINS. Before I begin, I would like to thank the other members of this panel for kindly letting me go first. As you will see in what I have to say in a few minutes, I'm going to talk about the matter of verification, which I think you'll find interesting, but with regard to timing, I'm committed to give a technical paper on this very subject in San Diego this evening and, in order to make a flight, I have to leave rather soon. I do appreciate being placed first on your list.

Mrs. LLOYD. Dr. Huggins, because of this, we will be submitting questions in writing for you to respond to for the record.

Dr. HUGGINS. Fine. I would be delighted to respond to any questions.

Mrs. LLOYD. So we will excuse you as soon as you finish your testimony.

Dr. HUGGINS. Also, if your staff can change my flight, I can perhaps stay a few minutes longer.

Mrs. LLOYD. I can't guarantee that.

Dr. HUGGINS. We'll see. We shall see.

Dr. JONES. Even Congress has limits.

Mrs. LLOYD. They say they're working on it.

Dr. HUGGINS. That's my understanding.

Mrs. LLOYD. Sometimes they work miracles around here.

Dr. HUGGINS. Ladies and gentlemen, I am delighted to have this opportunity to make a presentation to you on what may turn out to be an immensely important topic—the possibility that an entirely new and unexpected source of energy has been uncovered.

First let me say a few words to introduce myself. I am a professor in the Department of Materials Science and Engineering in the School of Engineering at Stanford University. I have been at Stanford for many years, after academic preparation in physics and physical metallurgy, the latter at MIT. I initiated Stanford's Center for Materials Research and was its director for 17 years. I also spent two years in Washington as Director of Materials Sciences at what at that time was called the Advanced Research Projects Agency. That was roughly 20 years ago. Thus, I have experience on both sides of the research enterprise, in the acquisition of scientific understanding in support of technological development, and in the management of research activities from the viewpoint of the sponsor. I am keenly interested in the question of how one can most effectively translate new scientific progress into useful technology.

My research group has been involved in recent years in a number of matters that directly relate to the recent observations of solid state fusion. We were, however, completely surprised by the recent announcement of Professors Fleischmann and Pons.

This so-called "cold fusion" is really a solid state phenomenon. For years, my group has been involved in an area called solid state ionics, in which we use electro-chemical concepts, tools and techniques to study solids, some of which have very unusual properties, related to the extremely rapid motion of atomic or ionic species within them.

Especially relevant to the topic at hand is the fact that, as I pointed out in a review article some 12 years ago, a number of metals containing hydrogen—and thus, also deuterium—have some of these same unusual properties, which means that hydrogen and deuterium can be rapidly incorporated into their internal crystal structures.

For this reason, we have been studying the properties of a number of metals and alloys containing hydrogen because of their potential as hydrogen—transparent membranes and for the solid state storage of hydrogen. One special program, supported by the Brookhaven National Laboratory, involves the use of such materials in a novel concept for the formation of hydrogen and oxygen gases by the electrolysis of water at somewhat elevated temperatures. This is closely related to the procedures used in the electrochemical experiments that have exhibited "cold fusion".

There are a number of metals and alloys that under certain conditions have many properties similar to those of hydrogen or deuterium-doped palladium. It may, therefore, be possible that other materials are even perhaps better and less expensive and will also be found to also exhibit the characteristics now being associated with the solid state fusion phenomenon.

Now let us go into the subject at hand. Since the press conference announcement by Fleischmann and Pons on March 23rd, who reported the observation of excess heat generation, neutron and gamma ray emission, and the presence of tritium in electrochemical experiments in which deuterium had been inserted into palladium electrochemically, there has been a great deal of interest in the possibility that some kind of solid state fusion reaction can occur in this and perhaps in other material systems.

Supportive reports have now appeared in a number of countries that similar phenomena have been observed by others. Press reports indicate that this has been achieved in Hungary, Japan, Russia and Italy, as well as in the United States. We'll be hearing shortly from some of the United States work.

On the other hand, experiments undertaken in many other laboratories have evidently not been successful in reproducing the reported effects. This has led to a great deal of skepticism in parts of the scientific community.

As was pointed out at a long session on this topic during the recent meeting of the American Chemical Society in Dallas, the validity of the reported results would be greatly enhanced if there were direct experimental evidence of a significant difference in behavior between the hydrogen-light-water-palladium system and the deuterium-heavy-water-palladium system. Such experiments would subtract out any contributions from spurious chemical effects, for they would be present in both.

We in the Solid State Ionics Laboratory of the Department of Materials Science at Stanford have undertaken experiments that address just that question; that is, whether there is a significant difference when deuterium rather than hydrogen is electrochemically inserted into palladium.

The results that we have obtained lend credence to the Fleischmann and Pons contention that a significant amount of thermal energy is evolved when deuterium is inserted into palladium, and that this phenomenon is quite different from the behavior of the otherwise analogous hydrogen-palladium system. On the other hand, except for neutron and gamma ray monitors used for safety purposes, no radiation detection measurements were undertaken in our study. Others with better equipment and greater expertise in such areas have performed experiments of that type.

I shall not repeat a description of our experiments and results here. They are to be presented in full at a meeting of the Materials Research Society in San Diego this very evening. That's why I'm leaving as soon as I can.

In one of the more extensive series of experiments, the excess power was found to be some 14 percent of the applied power over a wide range of voltage and current; that is, the ratio of internally-generated power obtained from whatever reaction is occurring within the palladium containing deuterium to the power supplied, through both the deuterium and hydrogen systems, is 1.14. So a direct comparison between the two, in this set of experiments, shows 14 percent excess energy in the deuterium case.

In other experiments over longer time periods, at a constant applied voltage, the excess power generated in the deuterium-containing palladium cell, compared to that containing only hydrogen, increased continuously, while the temperature of the hydrogen-based cell remained essentially constant.

The ratio of excess power in the deuterium versus the hydrogen case, to applied power, the ratio of excess power to applied power, rose from 20 percent to over 40 percent over a period of some 52 hours. We terminated our experiment after 52 hours for entirely other reasons that in no way imply that the experiment failed or stopped.

It should be pointed out that our calculations of the excess heat generated by whatever is happening within the palladium are conservative, in that they do not include the thermal value of the chemical fuels formed by the electrolytic reaction.

This method of calculation has not always been employed by others. If the fuel values of the chemical products were to be included in our calculation, this would contribute an additional component to the excess power, leading to an apparent enlargement of the overall effect.

Because of the direct comparison obtained between the deuterium-palladium and hydrogen-palladium systems as a result of these experiments, we conclude that there is an appreciable internal heat generation effect in the case of the deuterium-palladium system, regardless of the presence of any chemical or thermal effects in both systems, both the deuterium-palladium and the hydrogen-palladium systems.

We have observed this phenomenon a number of times in more than one sample and also in several electrochemical cell and calorimetric configurations. The magnitudes of the observed effects are comparable to those reported earlier by Fleischmann and Pons and lend strong support to the validity of their results.

Now I would like to make some comments on the apparent lack of success obtained in other experiments.

One of the interesting quandaries in this area at the present time is why some investigators seem to be successful in the observation of various effects—neutron flux, gamma ray flux, tritium concentration increase and thermal effects—and others are not. There are several materials science aspects of the experimental approach that are critical and which may not have been taken into account in some of the unsuccessful cases. One of the two major reasons has to do with the preparation and condition of the palladium samples being investigated. Hints concerning this possibility have been appearing in the public press in the last few days.

We shall be discussing this matter in detail in our technical presentation in San Diego this evening, and hope that we can be of help to others who wish to pursue experiments in this area.

We are, however, not in a position to contribute here to the debate about the heat generation mechanism. However, a proposal made by Walling and Simons that the products of the solid state fusion reaction are primarily helium-4 and heat is very interesting. If true, this is a very attractive circumstance, for it implies that one may be able to generate useful heat without the associated radiation hazards.

It is ironic that the research program that we are undertaking for the Brookhaven National Laboratory is one of the last three small efforts still underway in this area in the United States. I don't have to remind you about how severely the Federal budget for hydrogen-related research, and energy-related research in general, has been reduced during recent years. Perhaps now is a good time to give this matter new consideration.

I am sure you will also give some attention to the question of the distribution of effort and funding between a few very large and very expensive efforts and the possibility of many somewhat smaller, yet perhaps more innovative, efforts. I need not also point out that essentially all of the major advances in the types of science that may have some relevance to our national technological welfare have been in what is sometimes called "small science" rather than in "big science". Illustrative examples, in addition to the phenomenon being discussed here, include the discovery of new materials that exhibit high temperature superconductivity and the invention of the tunneling microscope, which for the first time allows us to see the structure of solid surfaces and phenomena occurring upon them on a truly atomic scale.

Thank you for this opportunity to address you.

[The prepared statement of Robert Huggins follows:]

Statement Before the Committee on Science, Space and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, DC 20515
April 26, 1989

Robert A. Huggins
Dept. of Materials Science & Engineering
Stanford University
Stanford, CA 94305

Ladies and Gentlemen:

I am delighted to have this *opportunity* to make a presentation to you on what may turn out to be an immensely important topic - the possibility that an entirely new and unexpected source of energy has been uncovered.

First let me say a few words to introduce myself. I am a professor in the Department of Materials Science and Engineering in the School of Engineering at Stanford University. I have been at Stanford for many years, after academic preparation in physics and physical metallurgy, the latter at MIT. I initiated Stanford's Center for Materials Research, and was its Director for 17 years. I also spent 2 years in Washington as Director of Materials Sciences at what was then called the Advanced Research Projects Agency, roughly 20 years ago.

Thus I have experience on both sides of the research enterprise, in the acquisition of scientific understanding in support of technological development, and in the management of research activities from the viewpoint of the sponsor. I am keenly interested in the question of how we can most effectively translate new scientific progress into useful technology.

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Especially relevant to the topic at hand is the fact that, as I pointed out in a review article some 12 years ago, a number of metals containing hydrogen (and thus also deuterium) have some of these same unusual properties, which means that hydrogen and deuterium can be rapidly incorporated into their internal crystal structures.

For this reason, we have been studying the properties of a number of metals and alloys containing hydrogen because of their potential as hydrogen - transparent membranes, and for the solid state storage of hydrogen. One special program, supported by the Brookhaven National Laboratory, involves the use of such materials in a novel concept for the formation of hydrogen and oxygen gases by the electrolysis of water at somewhat elevated temperatures. This is closely related to the procedures used in the electrochemical experiments that have exhibited "cold fusion".

There are a number of metals and alloys that under certain conditions have many properties similar to those of hydrogen - or deuterium - doped palladium. It may, therefore, be that other materials, perhaps better and less expensive, will also be found to exhibit the characteristics now being associated with the so-called solid state fusion phenomenon.

Now let us get to the subject at hand. Since the press conference announcement by Fleischmann and Pons on March 23rd, who reported the observation of excess heat generation, neutron and gamma ray emission, and the presence of tritium in electrochemical experiments in which deuterium had been inserted into palladium electrochemically, there has been a great deal of interest in the possibility that some kind of solid state fusion reaction can occur in this, and perhaps other, materials systems.

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On the other hand, experiments undertaken in many other laboratories have evidently not been successful in reproducing the reported effects. This has led to a great deal of skepticism in parts of the scientific community.

As was pointed out at a long session on this topic during the recent meeting of the American Chemical Society in Dallas, Texas, the validity of the reported results would be greatly enhanced if there were direct experimental evidence of a significant difference in behavior between the hydrogen-light water-palladium system and the deuterium-heavy water-palladium system. Such experiments would subtract out any contributions from spurious chemical effects, for they would be present in both.

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In one of the more extensive series of experiments, the excess power was found to be some 14 % of the applied power over a wide range of voltage and current, i.e the ratio of the internally generated power obtained from whatever reaction is occurring within the palladium containing deuterium to the power supplied is 1.14.

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We have observed this phenomenon a number of times in more than one sample and also in several electochemical cell and calorimetric configurations. The magnitudes of the observed effects are comparable to those reported earlier by Fleischmann and Pons and lend strong support to the validity of their results.

Comments on the Lack of Success Obtained in Other Experiments

One of the interesting quandaries in this area at the present time is why some investigators seem to be successful in the observation of various effects - neutron flux, gamma ray flux, tritium concentration increase, and thermal effects - and others are not. There are several materials science aspects of the experimental approach that are critical, and which may not have been taken into account in some of the unsuccessful cases. One of the two major reasons has to do with the preparation and conditions of the palladium samples being investigated. Hints concerning this possibility have been appearing in the public press in the last few days.

We shall be discussing this matter in detail in our technical presentation in San Diego this evening, and hope that we can be of help to others who wish to pursue experiments in this area.

We are not in a position to contribute here to the debate about the heat generation mechanism. However, a proposal has been made by Walling and Simons that the products of the solid state "fusion" reaction are primarily $^4He$ and heat.

If true, this is a very attractive circumstance, for it implies that one may be able to generate useful heat without the associated radiation hazards.

It is ironic that the research program that we are undertaking for the Brookhaven National Laboratory is one of the last three small efforts still underway in this area in the United States.

I don't have to remind you about how severely the federal budget for hydrogen - related research, and energy research in general, has been reduced during recent years. Perhaps now is a good time to give this matter new consideration.

I am sure that you will also give some attention to the question of the distribution of effort, and funding, between the few large and very expensive efforts and the possibility of many somewhat smaller, yet perhaps more innovative, efforts. I need not also point out that essentially all of the major advances in the types of science that may have some relevance to our *national technological welfare* have been in *what* is *sometimes* called "small science", rather than in "big science". Illustrative examples, in addition to the phenomenon being discussed here, include the discovery of new materials that exhibit high temperature superconductivity, and the tunnelling microscope, which for the first time allows us to see the structure of solid surfaces, and phenomena occurring upon them, on a truly atomic scale.

Thank you again for this opportunity to address you.

DISCUSSION

Mrs. LLOYD. Thank you very much, Dr. Huggins.

At this time the Chair would like to recognize one of the members of the committee, Tom Campbell.

Mr. CAMPBELL. Thank you, Madam Chairman. I simply wanted to be recognized to say at the start what your eloquence has already said, that I would urge the committee to take Dr. Huggins' testimony with great value and to recognize him as not only a constituent but a colleague on the Stanford faculty. Among the differences that are obvious between us is that I'm presently on leave. Any other comparisons between his professorship and mine would be invidious to me, so I shan't make them.

I am glad you're here, Dr. Huggins. We will try to be as brief as possible so you can get to San Diego, and you should know that a Stanford professor is most welcome in Washington.

Thank you, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Campbell.

At this time we will excuse you, then, if you need to catch—

Dr. HUGGINS. I would be glad to answer questions, and perhaps— Could I interrupt for just a second?

Mrs. LLOYD. Certainly.

Dr. HUGGINS. How do we stand on the airline? Two minutes. I'll be glad to answer a few questions. Perhaps if I could get on a later flight, I could answer more.

Mrs. LLOYD. I was wondering, Dr. Huggins, has your work been subjected to outside reviews?

Dr. HUGGINS. We have submitted a full professional paper to an internationally recognized journal.

Mrs. LLOYD. What further work do you plan to do to either expand upon your previous results to prove the validity of the Utah work?

Dr. HUGGINS. May I answer one—make one further comment on your first question? We're also presenting this paper this evening, in which we will discuss our work in great detail, in San Diego.

Mrs. LLOYD. Could we have a copy of this for the record, your paper or statement?

Dr. HUGGINS. That is not a written—that is not written at the present time.

Now, you asked what we were doing—

Mrs. LLOYD. Maybe you can expand upon your work.

Dr. HUGGINS. Yes. We are pursuing, with as much vigor as we can, recognizing the limitations of zero funding in this area.

Mrs. LLOYD. Your point is well taken. We are also looking at zero funding.

Dr. HUGGINS. My pocketbook has been the major support up to the present time.

There are a number of questions having to do with the important parameters involved in this phenomenon, some of which we will be addressing as rapidly as we can. We believe that there's a very good chance that other materials will show this same phenomenon and we would like to pursue that issue, that question, as well.

And I point out there is a further important issue, and this is whether the same phenomenon can occur at higher temperatures.

High temperature, high quality heat, is much more useful from a commercial technological standpoint than room temperature heat. We believe, from our experience in related matters, that this is a very strong possibility, we have not yet done anything in that direction.

Mrs. LLOYD. Dr. Huggins, I understand a copy of your article is being sent to the international journal. Could we have a copy?

Dr. HUGGINS. I would be glad to distribute copies of that after we have heard from the reviewers.

Mrs. LLOYD. Thank you.

Mr. Ritter.

Mr. RITTER. Thank you, Madam Chairman.

You mentioned that the problems, Dr. Huggins, in some of the other experiments might have been due to bad palladium, impure—is that what you're talking about, the palladium does not perform—

Dr. HUGGINS. There are two major reasons—and I will be discussing both these this evening at our technical meeting. One of these has to do with the method of preparation of the material, another has to do with an additional phenomenon having to do with the experiements and their conduct. Both of these are materials science problems. They're not problems of electrochemistry and they're not problems of physics.

I recognize that there's been a lot of criticism in the press because of the question of the order in which information is presented. On the one hand, everybody is anxious to know what's going on and what the latest results are, and on the other hand, we prefer to try to work within the scientific community as much as possible, and to be as open and free in following the normal procedures. As a result, I am trying to be rather careful not to present details at this hearing or to the public press before we present them this evening in San Diego. That's a regular professional scientific forum and that appears to us to be the appropriate place to talk about details.

I will, however, tell you that there are two major reasons why we believe many other people have been unsuccessful, yes.

Mr. RITTER. As someone who practiced a form of materials science in a previous life, I am delighted to see that the profession is getting involved in this. Of course, palladium being one of the more expensive materials known and, of course, virtually monopolized by the difficult situation in Southern Africa, a country with a lot of problems. It would be fantastic if the materials could move off the palladium base, so to speak.

Madam Chairman, I have no further questions. Thank you.

Mrs. LLOYD. It's your call. Do you have time for a question, Mr. Schiff and Mr. Campbell?

Mr. SCHIFF. In view of the time, I will pass, Madam Chair. Thank you.

Dr. HUGGINS. Are we okay? I can stay longer, if you like, evidently. My flight's been put off.

Mrs. LLOYD. Mr. Schiff.

Mr. SCHIFF. Am I back on?

Mrs. LLOYD. You're back on.

Dr. HUGGINS. Mr. Schiff, yes.

Mr. SCHIFF. If I understood, Dr. Huggins, you're saying that the experiments thus far conducted at Stanford tend to confirm Dr. Fleischmann and Dr. Pons' stated results; is that correct?

Dr. HUGGINS. We are confident that what we have measured is correct. We are confident that it's reproducible. The results we get on the thermal measurements are comparable to some of their measurements. We have made no other measurements besides thermal measurements.

Other people have done a very nice job of looking for neutrons and gamma rays, tritium and so forth. We have not done that yet. We've measured only heat effects. But our measurements confirm the measurements in the general magnitudes presented by Professors Fleischmann and Pons, yes.

Mr. SCHIFF. Do you feel confident that this is fusion?

Dr. HUGGINS. I'm not in a position to discuss what the mechanism is. I think there are many people who get involved with that and at this moment we have nothing further to add.

Mr. SCHIFF. Thank you, Madam Chair. I have no further questions.

Thank you, Dr. Huggins.

Mrs. LLOYD. Thank you very much, and thank you, Dr. Huggins.

Mr. Stallings, any questions?

Mr. STALLINGS. No, Madam Chairman.

Mrs. LLOYD. At this time the Chair will recognize our colleague from Utah, Mr. Nielson.

Mr. NIELSON. Thank you, Madam Chairwoman. I appreciate the opportunity of being here today. It's rather unique. I graduated from the University of Utah and have pardonable pride in that institution. I taught for 25 years at Brigham Young University, where the two gentlemen who are going to speak to you next are from, and I got my doctorate from Stanford. It's nice that Stanford is one institution that's lending support to the University of Utah claim, so I'm in a very interesting position of being supportive of all three.

Let me introduce Steven Jones of Brigham Young University. He's been an associate professor of physics and astronomy since 1985. He received his doctorate at a fine institution in Tennessee, Vanderbilt University, and has been a principal investigator for the Department of Energy since 1982, working on muon catalyzed fusion for DOE's Division of Advanced Energy Products. He has also published a recent article, is about to publish an article that's been accepted by Nature Magazine, it's been referred to—the Chairman referred to earlier today.

He's accompanied by Dr. Daniel Decker, the Chairman of the Department of Physics and Astronomy, who came to Brigham Young University one year after I did, and has been a colleague of mine there for many years. It's a pleasure to have them here, Madam Chairman.

Mrs. LLOYD. Thank you very much, Mr. Nielson.

We welcome you and look forward to your testimony now. When you finish your testimony, we will hear from the remaining witnesses before we resume our questioning.

Please proceed.

## STATEMENT DR. STEVEN JONES, DEPARTMENT OF PHYSICS AND ASTRONOMY, BRIGHAM YOUNG UNIVERSITY, PROVO, UTAH

Dr. JONES. Thank you, Madam Chairman, and Congressman Nielson, for that fine introduction.

I appreciate sincerely the opportunity to participate and testify at this hearing on cold nuclear fusion. I have been a member of the Physics Department faculty at Brigham Young University since September of 1985, and actually became active in this type of research, this specific type of research, just about a few months before that.

I would like to, by way of introduction—and pardon my cold, but I'll try to speak so you can hear me—I have been active in nuclear fusion research since 1979, when I joined the Idaho National Engineering Laboratory. Congressman Stallings may remember that we sat on a plane flight from Washington to Salt Lake City together, and we discussed muon catalyzed fusion, which is a precursor to the current research, and I will talk briefly about that.

In 1981 I wrote a proposal to study muon catalyzed fusion, which is a form of room temperature fusion, at the Los Alamos Meson Physics Facility. Following peer reviews by other scientists, this proposal was approved and funding was received from the Department of Energy, the Advanced Energy Projects Division. I would like to say in the strongest possible terms my appreciation for the support that we received from the Department of Energy through the years, including funding on this particular project.

I have been the principal investigator for experimental muon catalyzed fusion research then since 1982. Most of our experiments have been conducted at the Los Alamos laboratory.

In the spring of 1985, I began research on cold nuclear fusion without muons, the subject of today's hearing. Thus, I have been actively engaged in fusion research for ten years, in muon catalyzed fusion research for eight years, and cold nuclear fusion research for four years, so that I feel qualified to make some comments on this subject.

I would like to start by briefly reviewing muon catalyzed fusion, since this form of room temperature fusion has been very carefully studied and is closely related to what I call cold nuclear fusion.

Muon catalyzed fusion was theoretically predicted in the late 1940s by F.C. Frank, a British professor, and by none other than Andre Sakharov. The process was first seen experimentally by the late Louis Alvarez in 1956 at Berkeley. This was the first demonstrated observation of cold nuclear fusion involving, in this case, muons. In fact, I have discussed my recent work on muon catalyzed fusion with Professor Alvarez several times before his demise. I rather wish he were here today in my place. I miss his firm, no nonsense voice.

A muon is an elementary particle, a very heavy cousin to the electron. We create muons with large particle accelerators such as the one at Los Alamos. When muons are put into hydrogen mixtures at room temperatures, or near room temperature, hydrogen-like molecules form in which the hydrogen nuclei are held very closely together. The muon accomplishes this squeezing together of the nuclei because it is so heavy, about 200 times more massive than its cousin, the electron.

Without the need for high temperatures, this squeezing effect results in rapid fusing. As we call this sometimes piezonuclear fusion, piezo being the Greek term for to squeeze or compress. It is a truly remarkable process that was observed by us at Los Alamos and confirmed elsewhere, that the muon catalyzed fusion yields can approach—that is, the energy output can approach the energy which must be invested to produce muons in the first place. We have made tremendous strides in the last decade in research on muon catalyzed fusion. But I hasten to add that commercial power production would require a ten-fold improvement, approximately, in current conditions, in current fusion yields. It is not at all clear that we can bridge that gap, even though our yields achieved to date exceed those seen by Alvarez by a factor of several hundred.

I would like to show an overhead slide, if I could, to discuss a few points related to muon catalyzed fusion with regard to energy applications.

Mrs. LLOYD. If someone can dim the lights for us now, please.

Dr. JONES. Thank you.

This slide shows the "pot of gold" that we're all hoping for, truly, fusion energy. And it is, indeed, a noble and important goal. It also shows the obstacles that we have identified in our path of muon catalyzed fusion as an approach to realizing fusion energy.

Now, you notice the Department of Energy watching our progress and supporting it very well. We have been able to find that the first obstacle, which I won't describe in detail, but that obstacle turns out to be just a mole hill instead of a mountain.

The second obstacle, however, is the bottleneck in this process. It's not clear that we'll be able to surmount that, although research does continue.

Can you move the little man on the mountain? There you go. That's me, or us. He deserves not to be in the river, if possible, but on one of the mountains.

The point is, he's trying to get a shortcut to fusion energy, but he's not going to make it. The point is, even after we can achieve yields that are comparable or greater than the energy input to make the muons, which we have not yet done but we're getting close, we still have other obstacles, too, in our path to realizing fusion energy, in particular the engineering issues, actually building a reactor.

Now, in 1982, our first experiments at Los Alamos showed that we had actually achieved what we called scientific break even by this process, which means that there is more energy, more thermal energy output from fusion than there was energy in the driver of the fusion reaction—that is, the muon. Now, that ignores an awful lot. That ignores all the energy that must be invested to generate the muon. Therefore, this victory was a bit hollow. But I want to make a point here.

The management at the Idaho National Engineering Laboratory realized that this could be a significant achievement, the achievement of scientific break even. And so a press conference was planned. As scientists, we decided that, well, let's be careful here. We need to have our results reviewed by peers and their significance evaluated by other scientists. After consultation with—and I should give, I think, credit to the management at Idaho National Laboratory. They decided let's wait until publication. Let's give the peer review system this chance to evaluate the significance of the results and then, after three months—you see that time to publish—we'll decide whether or not we need to announce this to the world.

Well, at this stage I'm rather glad we did not announce scientific break even in 1982 to the world. It's accepted that this is a fact, that we did achieve it in 1982 by muon catalyzed fusion. But if we had announced it to the world, I'm afraid the public would have expected commercial power around the corner. As we see now from the perspective of seven years later, this was certainly not the case.

And so the first point I would like to make is that I think at this stage, in this research as well, we need just a few months, perhaps two months, to evaluate the significance, and in this case also the facts, of the scientific discovery.

I would also mention that there was contemplation of starting a cold nuclear fusion center, requesting large amounts of money from the Federal Government, as we had achieved scientific break even. Those plans were put on the shelf until scientific confirmation, wisely so, and then were deferred indefinitely as it became clear that while the process of muon catalyzed fusion is interesting, the possibility of energy applications are distant.

Now we can turn on the lights. I'm through with muon catalyzed fusion for the moment.

In view of the bottlenecks that we encountered in muon catalyzed fusion, I began with colleagues in 1985 to look for possible ways to achieve cold fusion at room temperature without muons. I published a paper on this subject entitled "Piezonuclear Fusion in Isotopic Hydrogen Molocules". This paper provides the theoretical framework for our understanding of cold nuclear fusion to this day, although there are some modifications and some other ideas involved. This was, I believe, the first theoretical paper that outlined in detail this process of cold nuclear fusion. It was published in March of 1986.

Shortly thereafter, we began—Well, I should give credit to Professor Palmer of Brigham Young University, who connected this notion of piezonuclear fusion with his knowledge of Helium-3 coming from the Earth, and made the startling hypothesis that geological minerals or metals might help catalyze nuclear fusion at room temperature.

Professor Johann Rafelski, now at the University of Arizona, added the notion that nonequilibrium—that is, rapidly changing conditions—would also be important to this process.

We had several exciting brainstorming session in March and April of '86 in which we planned experiments to study and test our hypothesis. We have since then loaded isotopes of hydrogen such a deuterium into metals. We began this research in May of 1986 with electrochemical cells. But we have used various means of loading hydrogen into metals, and I should emphasize at this stage that there are other ways besides electrochemical that we think will lead to cold nuclear fusion at a low level.

We began to look for the end products of cold fusion—neutrons, gammas, gamma rays particularly, but also helium and tritium. This work, as I mentioned, was started in 1986.

We soon realized that in order to see fusion yields, we would have to be sure that this is, indeed, fusion. We would have to have a very sensitive neutron counter, one that would allow us to not only count neutrons but to determine their energy. Because a result of deuterium fusion, which we were studying primarily, is that fast-moving neutrons are created, whose speed or energy, 2.5 million electron volts, is characteristic of the fusion reaction. If this is seen, then we know that fusion has occurred and not just some other reaction.

After years of painstaking work, we have been able to prove that fusion in metals does occur at very low levels by measuring the energy of the neutrons produced. Our work was conducted independently of that done at the University of Utah, and our results will be published tomorrow in Nature, the journal in Great Britain.

Recent experiments at other laboratories such as Italy, Moscow and Hungary, confirm the measurements of neutrons at very low rates, similar to the rates measured at Brigham Young University. This is not the same as saying that they confirm that energy-producing levels have been achieved. These are very low rates.

I hasten to add here that peer-reviewed and published papers, these must be first presented before we can accept these and understand these results in detail. So far, the findings have not passed the scrutiny of other scientists. Even the University of Utah paper, as I believe Martin Fleischmann mentioned this morning, is called a—I have it here—is called a "preliminary note", interestingly enough. So there is still a great deal of work that needs to be done to confirm and certainly understand this process.

Now, how much fusion energy is represented by these tell-tale neutrons? Roughly, a billionth to a trillionth of a watt in our experiments and in these others that I mentioned that measure neutrons. This is nothing to get excited about from an energy production point of view at the moment.

Yes, a new door—a new approach to fusion is interesting. A new door has been opened. But the gap between the bona fide fusion yield and energy production by fusion is roughly equivalent to that which separates the dollar bill from the Federal national debt, a factor of about a trillion to one. That is an enormous gap.

How about fusion without neutrons, as claimed for the Pons-Fleischmann experiments? Here we gain a great deal of insight by analogy to muon catalyzed fusion, which has been carefully studied for many years. Since the electrolyte contains lithium, it has been suggested that perhaps the dueteron-lithium-6 reaction is occurring. This produces alpha particles, helium-4, without neutrons. However, lithium-7 is also present in the electrolyte. This reaction with deuterons produces a neutron. If the d-lithium-6 reaction occurs, then the d-lithium-7 reaction ought also to occur. Indeed, lithium-7 is in greater abundance than lithium-6. But this neutron is not reported.

Another difficulty with this explanation is the vanishingly small fusion rate that comes from the fact that the lithium nucleus has a charge of 3 rather than 1, as the case of hydrogen. In 1957, J.

David Jackson, a well-known theorist, predicted that d-lithium reactions in muon catalyzed fusion would be impossible. So I think that explanation is pretty well excluded from what we know of this reaction.

There's another possibility. A normally extremely rare reaction which is involving 2 deuterons to produce an alpha plus an energetic gamma would be possible. This has never been detected in muon catalyzed fusion. Furthermore, in these experiments, it would show up in our gamma detectors. If no other way, by electron/positron pair production, the gammas would produce these electron/positron pairs. But these are not seen. I might add this particular reaction would also be deadly at the heat yield claimed, or at least very dangerous.

Okay. So there's another possibility. Perhaps the gamma ray is completely absorbed by the palladium lattice, or the energy is transferred to a heavy electronic quasi-particle in the lattice. I'm sorry I'm getting a little technical. I need to hit this point. This condition is a very interesting hypothesis, also a stretch of the imagination. But the problem that I have with it is that this should produce high energy electrons which would, in turn, produce radiation by the process we call bremsstrahlung. Such radiations are absent, evidently, in the University of Utah data, and certainly have not been seen at BYU. These should be seen in our detectors.

Now, we have not attempted to make measurements of heat production. Our level of fusions that we've detected, the bona fide fusions, are at such a low level that we have not found it expedient to try to measure heat. I will say in passing that I have found that palladium electrodes loaded with deuterium have become hot to the touch when exposed to the air. I've checked this in the last few weeks. This, I believe, is the chemical reaction. In discussing this with scientists at Los Alamos yesterday, I believe that's what it is. In any case, this was done next to our sensitive radiation monitors, and no burst of radiation accompanies this effect.

I will state it as my opinion—although I must emphasize this needs to be checked by numerous scientific experiements that are ongoing now and will take months—that the bona fide fusion component is a factor of many millions below energy output of commercial interest at this time. Therefore, I make three concluding statements:

First, cold nuclear fusion does not offer a short cut to fusion energy. It is another door to take, but it's just a start.

Secondly, and based on my work of ten years in fusion, and particularly on cold fusion, I will say that magnetic and inertial approaches currently represent the best paths to achieving controlled fusion energy. I would also add that I believe that funding for cold nuclear fusion should come by peer reviews from such organizations as the Department of Energy and NSF, in an established peer review way.

Finally, I would like to emphasize that cold nuclear fusion is an exciting scientific discovery. Let us appreciate it for what it is and not decry it for what it is not. I would like to compare cold nuclear fusion to this little plant, which is starting to wither—that may have some significance as well. This jar, by the way, is the size of jar we use for our electrochemical cells. It's one of the jars we actually use.

Now, this is a tender shoot, as you can tell. It is difficult to say what it will become. Some think and suggest strongly that this is a tree, and it will grow up very quickly and provide us enough wood for all our energy needs for generations.

I do not think it is. Let's give it a chance to grow. I think adding too much fertilizer at this stage will be detrimental.

[Laughter.]

I think we need to give it time, at least a couple of months, please, to see whether this is something that's a rose or a tree. If it should turn out to be a rose, we can then admire it for its beauty, even if we are a bit disappointed it was not a tree.

Mrs. LLOYD. Dr. Jones, your point is well taken.

Have you completed your testimony?

Dr. JONES. Yes, I'm finished.

Mrs. LLOYD. Thank you very much.

[Additional questions for Dr. Jones follows:]

U.S. HOUSE OF REPRESENTATIVES

# COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515
(202) 225-6371



May 4, 1989

MAY 31 1989

Professor Steven Jones
Department of Physics and Astronomy
Brigham Young University
Provo, Utah 84602

Dear Professor Jones:

As a follow-up to our April 26, 1989 hearing on cold fusion, I would appreciate it if you would send us a written reply to the questions attached.

Please mail your response to the attention of Kathryn R. Holmes, Subcommittee on Energy Research and Development, B374 Rayburn House Office Building, Washington, D.C. 20515 (202/225-8056).

I appreciate your prompt attention to this matter in order to assure their inclusion in the publication of our proceedings of the hearing.

Sincerely,

Marilyn Lloyd

MARILYN LLOYD, Chairman
Subcommittee on Energy
Research and Development

ML:cl
Attachment

**Questions for Professor Jones**

1. Beyond possible applications in the field of energy, your experiments on cold fusion also appear to have implications to increased scientific understanding of geologic and volcanic phenomena. Could you enlighten the Committee of your thoughts or plans for further studies in those directions, pertinent to conditions inside the earth?

I would be happy to.

But first, I should re-emphasize my opinion, based on several years of scientific investigation, that I do not believe that the "excess heat" claimed by Drs. Pons and Fleischmann is due to fusion reactions. A careful analysis of the quantity of fusion by-products, such as helium, will definitively resolve this question. Therefore, I have urged those claiming to see excess heat in palladium rods to submit those rods to immediate analysis.

It is true that we have observed the cold nuclear fusion process at extremely low levels using a sensitive neutron detector developed at Brigham Young University over the past few years. These results are of no immediate economic interest, particularly as regards energy production. However, the implications are profound with regard to studies of the earth and planets. Cold fusion even at small rates can contribute significantly to heat and helium-3 formation in the earth's interior. Volcanic gases and rocks show large amounts of

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helium-3 which corroborates, but does not prove, our hypothesis of geological cold fusion. The observation of tritium in volcanic gases would provide solid evidence for geological fusion reactions, and we are preparing to analyze such gases for this fusion by-product. We will also look for fusion by-products in minerals and diamonds from the deep-earth.

Cold fusion processes may help solve several outstanding puzzles in science, including:

Why do geothermal sources release large quantities of helium-3?

What is the source of the heat emanating from the planet Jupiter? What accounts for the large helium-3 abundance there?

Thus, while cold nuclear fusion may never be of importance from an energy point of view, our discovery of low-yield cold fusion may be of immense scientific interest.

-Steven E. Jones

Mrs. LLOYD. We will move now to Dr. Decker. We look forward to hearing from you. I think we have your written statements. Any of you that would like to summarize, please do so. Be assured that your entire testimony will be made a part of the record.

**STATEMENT OF DR. DANIEL L. DECKER, CHAIRMAN, DEPARTMENT OF PHYSICS AND ASTRONOMY, BRIGHAM YOUNG UNIVERSITY, PROVO, UTAH**

Dr. DECKER. Thank you, Mrs. Chairman. I did submit a written statement and will be glad, however, to now just speak freely on some of my own views and ideas.

We're grateful to have this opportunity to meet before the committee, and I'm glad that my former colleague, Congressman Nielson, is here to hear us.

At Brigham Young University, I would also like to say that we are very grateful that five years ago, four years ago, that Steven Jones accepted an offer to become a member of the Department of Physics and Astronomy. He's been a very productive and a very worthwhile member of our department.

I would first like to say and point out, which has been also I think well understood by most people, that the experiment at BYU and the experiment at the University of Utah are quite different experiments. In the one case we are actually looking for a nuclear process that is already known, which people understand is part of fusion, and at the University of Utah they have been looking for heat production, the origin of which we're still quite uncertain as to...

The next thing that we might say is that the results that are observed at the University of Utah are interpreted by the scientists as being unexplainable by normal chemical processes. Actually, a nuclear physicist would say—and, by the way, I'm not a nuclear physicist; I'm a solid state physicist—that I think a nuclear physicist would say, in the same vein, that those results could not be explained by any known nuclear process. So now we have two unknown chemical processes or a nuclear process, neither of them possible.

We might ask at this point what is it. It's too early to tell. There are many, mostly from the media, many ideas out there floating around. In fact, just yesterday I read on the electronic mail that the University of Berlin had repeated the energy measurements of the University of Utah and claimed that they could definitely show it to be a purely chemical process. We've also heard here today—and I also read that on the electronic mail yesterday—that the University of Stanford had fairly strong evidence that it may not be a chemical process. So at this point I think that's up in the air. We can't tell you whether it's a chemical or a nuclear process.

It is probably time for these physicists to go back to their laboratories and start doing some experiments, instead of giving speeches all the time, Steven.

I would like to also present a few ideas, however, on this disparity between the nuclear theory and chemical theory, because Dr. Pons and Dr. Fleischmann know very well their chemistry; therefore they feel that the answer to the problem cannot be chemical because they understand chemistry and they say it's beyond anything you could imagine by a factor of 100.

However, looking on the same side of the coin in light of a nuclear physicist, he would say, if you want to consider a branching ratio into a fusion process, where nothing comes out—only heat is generated within the lattice—that branching ratio is something like a factor of 10 to the 12th. So we now have to make a 10 to the 12th jump in physics, a factor of a 100 jump in chemistry, and to try to explain the origin of this energy source.

I think that gives you an idea of why we feel, at least as physicists, that maybe the chemists should also look very seriously into possible chemical reactions and not tell us physicists that we need to change our physics to explain the process.

Now, it is true that any good theoretical physicist can explain anything, but I think first we've got to have some experimental data to explain. So my plea is to wait. Let's go back and really find the experimental evidence, publish it, get it criticized by our peers, and then we'll be ready to give some answers here.

Thank you.

[The prepared statement of Daniel L. Decker follows:]

REPORT TO THE U.S. HOUSE OF REPRESENTATIVES
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY
Given by Dr. Daniel L. Decker April 26, 1989

Brigham Young University became involved in "cold fusion" research in September, 1985 when Dr. Steven E. Jones joined the physics faculty. He had been working in muon catalyzed fusion since 1981 prior to coming here.

Muon catalyzed fusion is a process wherein muons are injected into a gas of hydrogen or deuterium. The muon acts like an electron and will exchange places with an electron in the hydrogen or deuterium molecule but being 200 times as massive as the electron, it reduces the distance between the nuclei in the molecule by a factor of 200. At this close distance, the tunneling probability of the deuterons is sufficiently large to cause the nuclei to fuse in a very short time. The resulting nuclear products--Helium-3 plus a neutron or Tritium plus a proton--leave with so much energy that the muon is freed to exchange with electrons in another $H_2$ or $D_2$ molecule. This process continues for the lifetime of the muon ($2 \times 10^{-6}$ sec).

Before coming to Brigham Young University, Steve discussed with Dr.Van Siclen cold fusion in a solid by pushing deuterium atoms close together. For this process, they coined the phrase "piezonuclear fusion" and published a paper on the subject in June, 1985. In March, 1986, Steve presented a colloquium at Brigham Young University and discussed muon catalyzed fusion along with some of the other possible concepts for cold nuclear fusion. Dr. Paul Palmer was motivated by that colloquium to consider cold fusion as the possible answer for some questions that he had run into concerning geology of the earth, such as excess ratio of Helium-3 to Helium-4, tritium from the earth and the overall heat balance in the earth. He discussed these with Dr. Jones and in May, 1986, they began experiments with electrolytic cells to deposit hydrogen or deuterium in metals and look for nuclear evidence of fusion.

After a year of encouraging but inconclusive results, it became apparent that if any fusion was taking place, it was at a very slow rate and would take a more elegant detection system. At this same time, Drs. Bart Czirr and Gary Jensen were working on a neutron spectrometer for MeV energy neutrons. They decided to concentrate on developing this detector system in order to discern whether there was any fusion between deuterons concentrated in metals. In late 1988, the neutron spectrometer was fully conditioned and preliminary studies were carried out on titanium, palladium, tantalum, nickel, aluminum, iron, and lanthanum electrodes loaded with deuterium by electrochemical methods and gas pressure methods. The results were "tantalizingly positive" and those on electrochemically loaded titanium were considered in early February to be publishable.

A complete copy of the paper submitted to Nature to appear April 27, 1989 will be furnished to the committee and a brief discussion of it follows.

In the deuterium molecule where the equilibrium separation between deuterons is 0.74 A, the d-d fusion rate from tunneling is calculated to be $10^{-74}$ per $D_2$ molecule per second. If one could decrease that separation by a factor of 2, the d-d fusion rate from quantum mechanical tunneling is calculated to be of the order of $10^{-20}$ per $D_2$ molecule per second which is a small but measurable rate. This is about the rate needed to explain the flux of Helium-3 out of the mantle of the earth.

The best proof of d-d fusion occurring in a metal loaded with deuterium would be to detect the 2.45 MeV neutrons emitted by one branch of the d-d reaction. One could moderate these fast neutrons and detect them with conventional thermal neutron detectors. This is not easy for a low rate of neutron production because one is competing with the thermal neutron background from cosmic rays. This method would be successful if the experiment is done in a deep mine where the cosmic ray background is small.

In our experiment, a high-energy neutron detector was developed which could distinguish the neutron energies. One could then examine the neutrons in the energy range near 2.45 MeV. Other events can be removed from the background by discrimination techniques. The detector consisted of a liquid organic scintillator in which three $^6Li$-doped glass scintillator plates are embedded. Neutrons deposit their energy in the liquid scintillator via multiple collisions giving a light output to the photomultipliers from which this energy can be determined. These now low-energy neutrons are then scavenged by the $^6Li$ in the glass and a glass scintillation pulse is emitted. The glass and liquid scintillation pulse shapes differ and can be distinguished in the photomultiplier signals. A liquid pulse followed by a glass pulse within 20 microseconds identifies a neutron and by pulse height analysis of the liquid pulse, the neutron counts can be distributed according to energy.

The counting system is calibrated with 2.9 and 5.2 MeV neutrons generated by d-d interactions at $90^o$ and $0^o$ with respect to the deuteron beam from a Van de Graaff accelerator. Background runs with this system showed a smooth decreasing count rate with no indication of excess neutrons in the window near 2.5 MeV. However, when electrolytic cells containing titanium cathodes being loaded with deuterium are placed near the detector with 10-500 mA current passing through the cells, a definite bump appears in the spectrum indicating excess neutrons at 2.5 MeV energy--4 standard deviations above the background on either side of the 2.5 MeV window. This corresponds to a detection rate from fusion of 2 neutrons per hour; the background being composed of 3 cosmic ray neutrons per hour and 1 gamma/gamma coincidence per hour. Since our counting efficiency is about 10% and the solid angle for the detector is about 0.1 steradian, this corresponds to 200 fusions per hour in this branch of the d-d reaction.

With a signal this small, one must worry about all possible very small sources of neutrons. In our experiment, this difficulty is lessened since only high-energy neutrons would affect the count rate. One worries about diurnal and sunspot activity on rates of cosmic ray generated neutrons. A two-week study of this background reveals no diurnal effects which is consistent with standard cosmic ray data--the latter also indicates less than a 6% variation in cosmic ray level during periods of intense sunspot activity. We have looked for as many sources of systematic error as we can think of and have confidence in our results. A statistician has studied the data and gives a confidence level of 99.99.

Several other laboratories have undertaken to check on these results. Some have not good enough counting systems and see nothing above background. Some have good facilities and have reported detection of neutrons at levels similar to our results. One group in Italy has recently reported detecting neutrons in titanium loaded with deuterium in a high-pressure gas system. None of these reported results constitute verification for true verification requires publication of a refereed paper showing method, technique, results, and analysis. If those laboratories have verified our results, then such papers will be forthcoming. It is too early to tell.

The world is also very curious about another experiment recently performed at the University of Utah. In that experiment, Ors. Fleishman and Pons reported finding heat energy liberated at a palladium cathode loaded with deuterium in an electrolytic cell. They argue that the amount of heat is greater than one could explain by a chemical reaction. There is no present evidence that this heat is related to fusion; even by their estimates of fusion rates, the number of neutrons or the number of tritiums is deficient by many orders of magnitude. Rather than consider the possibility of some heretofore unknown chemical reaction being responsible, they prefer to suggest that a violation of various laws of physics is necessary to explain this Pons-Fleishman effect. The only similarity between this experiment and the one at Brigham Young University is the use of an electrolytic cell. The electrodes, the electrolytes and the results are completely different. In the Brigham Young University experiment, the emission of neutrons from fusion began just a few minutes after application of current rather than after many days. This could be because of our using a thin foil or a sponge rather than solid rods. In the University of Utah experiment, the objective and detection was calorimetry. Other laboratories have tried to reproduce the Pons-Fleishman effect, many with no success, but there is an "off-and-on" verification by the group at Texas A & M and a new report from Stanford. Again, whether these represent a real verification must await a publication of a refereed paper where the scientific world can see the result in a more precise manner than a news report. It is very difficult to sort truth from rumor by what one observes in the media so, in the final analysis, I must conclude that these results have also not yet been duplicated.

If the Pons-Fleishman effect is duplicated, we are still left with the puzzling question--what is it? Several hand-waving theories have been reported through the media. Physicists and chemists in nearly every laboratory in the world have pondered this question. To generate a theory of a physical effect that turns nuclear excitation energy of the order of 10 MeV into lattice thermal energy of the order of 1 eV before the nucleus can radiate its energy by emission of a neutron or a proton (in something less than $10^{-20}$ seconds) is a tall order. That's probably not as hard as discovering a means of getting the order of 1 KeV energy per molecule from a chemical reaction where the electrons involved have only about 10 eV energy. Either way will require some very subtle reasoning but until that question is resolved, we shall not know if the Pons-Fleishman effect represents a new source of energy for an energy-hungry world or just a fantastic battery.

I append the summary of a review of this subject recently written by a colleague, Dr. B. Kent Harrison.

Scientific Comments on the Current Status of the Fusion Matter
by B. Kent Harrison, Professor of Physics, Brigham Young University, April 18, 1989.

In Summary: at present, a nuclear fusion source for the energy production at the level claimed has not been demonstrated, and indeed is unlikely. Nuclear fusion at much lower levels has been demonstrated, but cannot account for the energy. A clear demonstration of high level fusion in the present experiments would require a positive identification of the actual reaction(s) taking place.

In the absence of this confirmation, one may infer that the energy source is either one or more chemical reactions, heating from a resistance in the electrical circuit, "Peltier" effects, in which energy is released at the junctions of dissimilar metals in an electric circuit, or more than one of these. All are possible. The Utah experimenters have discounted these possibilities, claiming that all have been thoroughly investigated.

If chemical reactions are the source of the energy, would this constitute a large new energy source? No. The virtue of nuclear reactions is that a sizable fraction of the rest mass of the nuclear particles is converted to useful energy (by Einstein's famous equation, E equals mc squared.) However, in chemical reactions this effect is negligible; such reactions are similar to ordinary combustion. It is possible in the current experiment that the charging of the cell for many hours–which the researchers indicated was necessary–simply stored energy which was later released; in effect, then, the cell would simply be an ordinary storage battery, not a source of energy.

If high level fusion is not demonstrated in the current experiments, does that mean that it never will be demonstrated at room temperature? No. Continued research is definitely warranted, since it is still possible that, by squeezing deuterons from other nuclei together sufficiently tightly in some yet to be determined experiment, economically useful energy production may be achieved in the future. Many scientists believe this is not possible at all. If it is possible, it will likely require considerable time and effort before it is achieved.

**DANIEL L. DECKER**, Professor, Brigham Young University
**Birth:** 22 September 1929
**Wife:** Bonnie Beardall Decker
**Children:** Kenneth, Renae, Ralph, Max, Eldon, Keith, Lois

**Education**

B.S. Brigham Young University, 1953
M.S. Brigham Young University, 1955
Ph.D. University of Illinois, 1958

**Positions Held**

1952-5 Teaching and Research Assistantships: Physics and Math, Brigham Young University
1955-6 Research Assistant, University of Illinois
1956-7 Celanese Corporation of America Fellow, University of Illinois
1957-8 National Science Foundation Fellow, University of Illinois
1958-63 Assistant Professor of Physics, Brigham Young University
1959 Research Physicist at David Sarnoff Laboratories, RCA, Princeton, New Jersey (Summer)
1961 Research Physicist at Bell and Howell Research Center, Pasadena, California (Summer)
1963-7 Associate Professor of Physics, Brigham Young University
1964-5 Visiting Staff Member, Los Alamos Scientific Laboratories, Los Alamos, New Mexico
1967- Professor of Physics, Brigham Young University
1968 Visiting Scientist, Los Alamos Scientific Laboratories, Los Alamos, New Mexico (Summer)
1971-2 Resident Associate Physicist, Argonne National Laboratory, Argonne, Illinois
1977-8 Research Scientist, Centre d'Etnde Nucleaires de Saclay, Gif-sur-Yvette, France
1979 Research Scientist, Centre d'Etude Nucleaires de Saclay, Gif-sur-Yvette, France (Summer)
1982 Research Scientist, Centre d'Etude Nucleaires de Saclay, Gif-sur-Yvette, France (Summer)
1985 Visiting Scientist, Argonne National Laboratory, Argonne, Illinois (Summer)
1988- Chairman Department of Physics and Astronomy, Brigham Young University

**Professional and Honorary Societies**

Sigma Pi Sigma
American Physical Society
American Association of Physics Teachers
Phi Kappa Phi
Utah Academy of Sciences, Arts, and Letters

**Honors and Offices**

Charles E. Maw Scholarship (1953)
Celanese Corporation of America Fellow (1955-6)
National Science Foundation Fellow (1957)
President of the local chapter of Phi Kappa Phi (1967-8)
Karl G. Maeser Research and Creative Arts Award (1969)
Chairman - Physical Sciences section of the Utah Academy of Sciences, Arts and Letters (1969-71)
Sigma Xi Lecturer (1974)
Distinguished Faculty Lecturer Brigham Young University (1980)
Fellow of the American Physical Society (1980)
President of the Idaho-Utah Section of the American Association of Physics Teachers (1984-6)

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**Daniel L. Decker**
**Brigham Young University, Provo, Utah**

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Mrs. LLOYD. Thank you very much, Dr. Decker.
[Additional questions for Dr. Decker follows:]

BRIGHAM YOUNG
UNIVERSITY

THE GLORY OF GOD
IS INTELLIGENCE

May 25, 1989

Subcommittee on Energy
Research and Development
ATTENTION: Kathryn R. Holmes
B374 Rayburn House Office Building
Washington, D.C. 20515

Honorable Subcommittee Members:

In response to your question: "Could you attempt to predict, or suggest, what research directions might be fruitful in an effort to increase the rate of piezonuclear fusion, and what the scope of such a program might be in terms of resources", I respectfully submit the following comments.

It is still somewhat early to predict the outcome of piezonuclear fusion experiments. The present measured fusion rates are so low that a useful energy source seems impossible. However, there are several new developments that tempt one to suggest certain areas of research that might show some promise.

It has been quite convincingly argued that the rate of fusion of an equilibrium distribution of deuterium diffused into metals is vanishingly small and that the small rates that are observed must therefore arise from transcient effects in the solid. The observed fusions are probably related to some rapid adjustments of deuterons' positions in the structure caused by current flow, temperature changes, structural cracking, etc. The exact nature of the microscopic processes in the metals are still unknown. It has also been observed that neutrons from fusion are often liberated in bursts consisting of sometimes 50 to 200 neutrons. The fusion rate during the less than 200 microseconds time duration of the burst is quite respectable but the bursts are few with long times between them such that the overall fusion rate is very slow.

One line of research that may prove promising would be to study these bursts in order to discover what triggers them and use this information to attempt to increase the burst rate and consequently the total fusion rate.

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There are many unsolved problems in understanding the microscopic process that takes place within a solid to allow dissolved deuterons to approach close enough to fuse. Are there resonances in the vibrational structure? Are there cracks that form generating accelerating fields which give energy to the deuterons? Could one enhance these effects by electromagnetic radiation of a certain frequency or by ultrasonic waves in the solid or some other means to cause non-equilibrium in a material? All of these problems are in the domain of solid state physics and material science. With the answers to some of these questions, then one could intelligently ask the question: "How can we enhance the effect"?.

As to resources necessary: Since most of these research projects do not require large machines, the actual resources are not very expensive. However, in order to "see" the fusion, very sensitive neutron detectors are required which are not available in most laboratories. These are not expensive but are rare. I am not accustomed to making this kind of cost estimate but I would think ten to twenty million dollars spread over several laboratories would be adequate. One of the major expenses is in supporting the time for many scientists to devote some serious thinking to the project and try out many ideas.

Sincerely,

Daniel L. Decker

Daniel L. Decker
Department Chairman

DLD/wjw

Mrs. LLOYD. The Chair at this time would like to recognize Congressman Bruce from Illinois.

Mr. BRUCE. Thank you, Madam Chairman.

This is certainly an interesting and important university segment, this panel, and their information on fusion research is particularly important.

I'm happy that we have on that panel Professor George Miley of the university of Illinois from my district. I am pleased that he could be with us today. His credentials have been distributed and they speak for themselves.

But I would like to just note to the Committee that he is currently the Editor of the Journal of Fusion Technology and serves on several important committees focusing on fusion research efforts, and we're happy to have him here and share with us his information.

**STATEMENT OF DR. GEORGE H. MILEY, PROFESSOR OF NUCLEAR AND ELECTRICAL ENGINEERING AND DIRECTOR, FUSION STUDIES LABORATORY, UNIVERSITY OF ILLINOIS, URBANA, ILLINOIS**

Dr. MILEY. Thank you, Congressman Bruce. My testimony is more lengthy, and I have submitted it, so I wanted to just run through quickly some slides.

Mrs. LLOYD. Your entire written statement will be made a part of the record, and you may move ahead and share your slides with us, if someone will dim the lights for us again. Thank you.

Dr. MILEY. Can you hear me from here?

Mrs. LLOYD. It is a problem. I think if you can have some assistance with the slides and return to the microphone, we will be able to proceed in an orderly fashion.

Dr. MILEY. I'll sit here so I can be by the mike.

Some people have asked why I'm on this panel. I thought I should explain my views. Number one, I'm a long-term proponent of fusion in any form, but I've been a pioneer, I think, in the search for so-called advanced fuels or aneutronic fusion, which would have reduced radioactivity such as is viewed for cold fusion as proposed here. I also have always been a proponent of alternate confinement concepts, which this certainly is, and I've written a book on direct energy conversion for fusion which should reduce heat pollution, which is another big problem. So all these pertain to cold fusion, and I guess that's why I'm here.

Next slide, please.

We do have some experiments in Illinois, but I didn't want to talk about those since so much has already been said about experiments without definite results yet. The issues that I had planned to cover are listed here, and they are ones that are fairly obvious and many have already been covered. A few of the points I will make may be somewhat repetitious, but I'll go ahead anyway.

I would like to get one crack at verification and talk about technology development needed. What I'm going to do is at that point assume that this heat-producing cold fusion occurs, as stated, and talk about some of the other ramifications.

And then I couldn't help but talk a little bit about issues that we've already covered, what Congress might do to ensure U.S. leadership in implications and creativity.

Next slide, please.

Verification, I feel, has to involve heating and simultaneously measurement of reaction products from whatever this mysterious reaction is. This is not easy, as people are discovering. But as I've already said, I will assume that's done. Let's go ahead to the next slide.

Technology issues have been touched upon, and without knowing more about exactly what we're dealing with, it's difficult to go on to any great detail. But I've covered a few here.

Number one, temperature limitations and efficiency. This is a very severe problem for the configuration as presently envisioned, and it seems to be limited by material considerations along with the question of the reaction rate versus temperature. That's something that has to be looked at very seriously in order to get a system that has a reasonable conversion efficiency.

Materials we've already talked about. It is certainly very crucial to find other materials not only to reduce costs, availability, but also to allow increased efficiency. There's a question of lifetime of electrodes, recycle, tremendous problems. There are questions about scales, scale and a control.

I would like to jump down to the next—pull that down a little—alternate configurations. Dr. Jones just mentioned something which seems obvious to me, that there are other ways of getting deuterium into palladium or metals that really need to be explored. It isn't clear that this electric cell is the best approach. Certainly other approaches might lead to a better efficiency, direct energy conversion, if it's possible.

A question earlier was raised about radioactivity. Now, at the moment it is stated that some neutrons, some tritium produced the bulk of the energy comes out is heat. I would remind you, though, that this is operated over a long period of time. The sum accumulates. And so it shouldn't be stated this is without radioactivity. We're going to be accused of starting an energy scenario over again like we did in fission. The statement there that it was "too cheap to meter" will now become "there's no radioactivity".

That isn't quite true. In fact, one of the crucial questions that no one has alluded to yet that needs to be looked at is what other fusion reactions might be excited in this fashion.

Now, on the upside of this good part would be if you could work with some that inherently tend to have less radioactivity involvement, D-Hel-3 and PB-11, as I have listed. That might even help the seat production more. Who knows.

On the other hand, there is another obvious difficulty, and that is the question of what happens if D-T fusion works. This is a very high intensity neutron source which would be cheap. One has to worry about proliferation. The final line in this slide, if you would move it up a little bit more, R&D balance. There have been a number of suggestions about how to carry forward development. I tend to agree. I like this approach of a combination of attempts to understand the microscopic theory and development of technology simultaneously. So I won't say much more about that.

Could we go to the next slide, please.

Comments about effect on other R&D research, I guess the one true comment is it's going to affect it. If this turns out to be true, there's a tremendously strong competition. If not—Now I like the thought of all right, let's take some risks and try to develop something quickly. But if we jump into something and it crashes—I'm reminded of the sad experience in the early days of fusion in Britain, where the ZETA experiment reported neutrons, hit the headlines of all the papers, and it was a very exciting time. Then it was discovered these neutrons weren't thermal nuclear; they can from instabilities. The fusion program in Britain was set back years, years. The same thing would happen here if we launch an endeavor and view it as a "wildcat" oil well, I'm afraid. Thus, the consequences have to be looked at from a very broad perspective before we do something like that.

The next slide.

Action by Congress and others. I urge that all of us don't overreact and start to put all the eggs in one basket.

Now, there are some interesting issues, though, that come out of all this. At the moment, we've talked about large programs. I'd like to talk about small programs because I relate to—since I'm involved in such.

These discoveries, muon catalyzed, cold fusion, et cetera, have come from small groups. It's very important to the research activities of this Nation that we have seed money to allow smaller groups to do the exploratory research, to do that. I think that in fusion now, with this possible type of innovation that's coming out, there needs to be a mechanism whereby—perhaps a steering committee or something could pull together the university and industrial and small group exploratory research. I'm not talking about a main crash project, but there are so many ramifications and possibilities that the real question is how to get the vigor of the individual maintained in this. That might be a way.

I think we need to all recognize and acknowledge that public recognition of the importance of this new, clean energy source, the recognition may not turn out to be possible, but certainly the energy crisis has been forgotten, but people now have begun to realize again, with the global warming, et cetera, that this is a problem that we are going to have to face again. There should be national goals.

I feel that fusion—as I've said, I'm a lifelong proponent of this fusion. It has so many possibilities, that one of the difficulties of the moment is there is no room for funding for innovative research. Something needs to be done. I, in brainstorming, put this note "perhaps there needs to be a new office to tie together interests of DOE, NSF, NASA." DOE has the responsibility and interest of ultimately developing electrical production from fusion. NSF has the role of basic research, but some questions came up earlier about NSF funding. NSF at the moment will not fund fusion research because they feel it's a DOE responsibility. DOE has only enough funds to fund their large projects and a few exploratory efforts. There is a tremendous limit and gap between these. It's amazing that such a fundamental energy source as fusion cannot be supported in this country by the National Science Foundation on a basic level.

The other thing that I would put down is the note that there are additional agencies becoming interested and perhaps will want to contribute to this effort. For example, NASA is interested in fusion with the discovery of the possibility of mining helium-3 on the moon and using this for B-helium-3 fusion, and the whole concept of fusion space power, fusion propulsion. This brings in other dimensions to this energy problem.

The last slide, please.

I simply wanted to thank the committee for the opportunity to comment on this extremely important and urgent issue. Thank you.

[The prepared statement of George H. Miley, plus additional questions and answers for the record follow:]

TESTIMONY AT INFORMATIONAL HEARING ON RECENT DEVELOPMENTS IN FUSION ENERGY RESEARCH BEFORE THE HOUSE COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY, APRIL 26, 1989 BY

GEORGE H. MILEY, PROFESSOR OF NUCLEAR AND ELECTRICAL ENGINEERING, AND DIRECTOR, FUSION STUDIES LABORATORY, UNIVERSITY OF ILLINOIS, 103 S. GOODWIN AVENUE, URBANA, IL 61801. PHONE 217+333-3772

Let me begin by explaining my involvement in cold fusion. First, having worked on fusion for many years, I became very excited when this new discovery was announced. So did my colleagues at Illinois. We now have two experiments in the Fusion Studies Laboratory, one in Chemistry and one in Physics. There have been numerous meetings and informal seminars among the interested scientists. I am a co-author on a theoretical paper that addresses a possible mechanism -- I have put out a call for Technical Notes for publication in the journal Fusion Technology which I edit. Last, but not least, I read the technology section of the Wall Street Journal every morning to get the latest news!

The experiments at Illinois have not yet produced definitive results. However, based on other reports, I am personally convinced that solid-state catalyzed cold fusion occurs and this is an unexpected and very important new regime of physics. The fusion I refer to, however, is the conventional D-D reaction, and the reaction rate is quite low. There is not yet sufficient data to evaluate the possibility of a high reaction-rate, heat-producing reaction such as reported by the University of Utah workers. Rather than debate that issue now, for the present discussion I will simply assume that this is possible and consider some of the consequences. (Let me stress that I hope that this turns out to be true, but there are clearly many unanswered questions.)

There are a variety of issues to be considered. These include: What verification experiments are needed? How attractive is this approach for fusion power? What technology must be developed and how fast can this be done? What will the impact on other fusion research be? Are there special problems such as increased potential for proliferation? What should congress do to insure that the U.S. maintains a lead in this field? What does this discovery imply about creativity in fusion research in the U.S.?

I will briefly consider each of these questions in turn.

A variety of verification experiments are in progress over the world. I do not have time to comment on this in detail but will simply note that, in my view, the most important studies are aimed at identification of the fusion reaction products. The most likely reactions lead to He-4, He-3, and tritium. Measurements are demanding, however, due to the small quantities of these products generated, their retention in the palladium electrode, and normal contamination of heavy water by trace amounts of these elements.

At first glance cold fusion looks quite attractive for power production due to the simplicity of the cell. However, there are a number of issues that must be evaluated before the route and time required to scale up to commercial operation becomes clear. First, unless higher temperature electrodes are possible, the conversion efficiency may be low. Temperature limitations depend on the materials employed and on the effect of temperature on the reaction rate. Both must be studied. A second key issue revolves around the availability and cost of the electrode materials, the lifetime of the electrodes, and our ability to recycle the materials used. In these respects, much depends on our ability to find suitable substitutes for palladium. Indeed, in principle, there are a number of possible candidates, and some experiments along these lines have already begun in various labs.

When considering scale up, it should be recognized that an electrolytic cell may not be the only (or the most desirable) configuration possible. Thus the recent Italian experiment did not use a cell of this type but simply cycled the temperature to obtain a phase change. One of our experiments at Illinois also uses an entirely different approach. One objective in seeking alternatives is to find a configuration that offers advantages such as more efficient energy extraction, for example a configuration that is compatible with direct energy conversion techniques.

Another key "attractiveness" issue is the amount and type of radioactive inventory that will be involved in a power producing cell. This strongly depends on what the reaction mechanism turns out to be. Though the University of Utah experiments mainly produced heat, some neutrons and tritium were detected implying a non negligible accumulation of radioactivity over long run times. The resulting complications for maintenance and material handling must be evaluated. This would be especially critical if small "nuclear battery" type units are contemplated.

Radioactivity issues raise additional important questions: How "aneutronic" is the new fusion reaction suggested by the Utah experiments? Can fusion reactions other that D-D be catalyzed this way? The ability to use other aneutronic reactions like D-He3 and p-B11 would be an important goal. On the other hand, if D-T also works and can be developed into a strong, cheap neutron source, proliferation issues could become a serious concern. Thus the feasibility of causing fusion reactions other than D-D should be studied early in the development of this technology.

Another issue involved in the development of cold fusion is the division of effort between theoretical (microscopic) physics studies and technology development. Certainly strong efforts in both areas are needed. However, it should be realized that the basic mechanisms involved may be quite complex, requiring years to obtain a full understanding. This is not necessarily an unusual situation. Consider, for example, superconductivity. This phenomena had been known and used in laboratory devices for years before Prof. J. Bardeen and colleagues proposed a possible theory. Recently, the discovery of high temperature superconductors forced a rethinking of the theory. The theory remains unsettled. Still this situation has not stopped the rapid development of the technology needed for practical applications of superconductors. Likewise the desire for a fundamental understanding should not be used as a reason to retard the development of cold fusion technology. A theoretical understanding would be a most valuable asset in guiding such developments but a balance is needed between basic research and technology development.

Regardless of the outcome of confirmation experiments, cold fusion will have an effect on the present MFE and ICF programs. If the outcome is negative, the whole fusion community will be accused of unfounded optimism (a criticism frequently voiced beginning when reports of fusion neutrons from the British experiment, ZETA, were widely publicized in the early 1970's and then withdrawn). If the outcome is positive, the present programs will brace for a new competition for what is already a very tight budget.

Regardless of the actual outcome, I feel that congress and all concerned must be patient and not over react. Considerable time will be required to unravel the situation, and great harm could be done by acting prematurely, for example by putting "too may eggs in one basket." Still, it is clear that cold fusion, if real, could have a tremendous impact on future energy technology. Quick action is needed to keep the U.S. in a leadership role and insure that the subsequent decisions are made with sufficient facts in hand.

The real need at the moment is a supply of "seed" money for a number of small groups to carry out exploratory research. Many workers such as myself have jumped in using enthusiastic voluntary workers, $500 to $1000 for materials, and borrowed equipment to get started. However this obviously can't last long. Meanwhile there are no easy ways to raise a modest amount of support to develop a more sophisticated experiment. Most agencies are waiting until the next fiscal year for new starts and they typically take a half year or more for reviews. This is too slow. Consideration should be given to setting up a mechanism for rapid dispersion of "seed" money for exploratory and confirmation experiments. A steering committee could carry out fast reviews and establish priorities. Also, as in superconductivity research, every attempt should be made to pull universities and industry together in this effort.

Finally, several points stand out from the way the cold fusion discovery captured so much attention. The energy crisis may have been temporarily forgotten due to more pressing near-term concerns, but most people still realize that it is still a fundamental problem facing us over the long term. Now the time scale appears even shorter than envisioned earlier due to the Greenhouse effect, acid rain, planning for more aggressive space missions, etc. The development of an attractive fusion energy source would be a real breakthrough, and this is widely recognized by the public. Thus it seems timely and important to review our whole R&D program in fusion to see if improvements are possible.

One problem that is clear is that the fusion program has become so focussed on current major projects that innovative new work is curtailed due to lack of funding. For example, the National Science Foundation will not fund fusion related research because it is the mission of DOE. Alternate approaches and innovative research receive less money each year from DOE's Office of Fusion Energy due to obligations to large projects. The Inertial Confinement Fusion office doesn't even fund unsolicited research proposals, leaving that to the National Laboratories who obviously have other top priorities. This is no way to find innovative approaches in an area that should be a top national goal.

Several years ago, as President of the University Fusion Association I testified before the House Energy subcommittee that a mechanism to fund a number of small innovative projects in fusion energy was urgently needed. I am repeating that proposal now. The need to establish such a mechanism grows more urgent as the necessity for development of new, clean energy sources becomes more and more pressing.

In view of the complexity of the organizational structures involved, and the need to cut across lines of magnetic, inertial, and now cold fusion, the only solution may be to set up a new office to handle this. If so, the new organization should encourage and integrate efforts in DOE, NSF and other agencies such as NASA, with potential interests. Relative to NASA, I would note their increasing interest due to the possibility of lunar mining of He3 (a key fusion fuel) and the need for an energy source such as fusion for deep space missions.

In closing, I want to thank the committee for this opportunity to comment on these most exciting and important developments in fusion research and development.

Background: George H. Miley, is Professor of Nuclear and Electrical Engineering and Director of the Fusion Studies Laboratory at the University of Illinois. He is well known for his contributions to fusion energy research, especially related to advanced fuel fusion and direct energy conversion. He is a fellow of the American Nuclear Society, of the American Physical Society, and of the Institute of Electrical and Electronic Engineers. In 1985-86 he held a Guggenheim Fellowship for research achievements and served as an Associate in the Center for Advanced Studies at the University of Illinois. Dr. Miley is also a consultant at various industrial and Department of Energy laboratories, is the editor of the ANS journal, *Fusion Technology*, and is managing editor of the Cambridge University journal, *Laser and Particle Beams*. In addition he serves on the Radiation Protection Council for the State of Illinois and chairs the Technical Advisory Committee for the Illinois-Kentucky Compact Low Level Radioactive Waste facility.

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May 5, 1989

Subcommittee on Energy Research and Development
Attn: Kathryn R. Holmes
B378 Rayburn House Office Building
Washington, D. C. 20515

Dear Ms. Holmes:

As requested by Representative Lloyd in her letter of April 28, I am enclosing a copy of the slides which I presented at the April 26 Hearing on Cold Fusion. If you need further information about my presentation do not hesitate to contact me at Telephone (217) 333-3772 or Fax (217) 333-2906.

Sincerely,

George H. Miley

George H. Miley, Director
Fusion Studies Laboratory

GHM:cs

Enclosures

Slides presented at the Hearing on Cold Fusion, April 26, 1989, Washington D. C.



George H. Miley

University of Illinois

Urbana/Champaign, IL

<u>A long-time proponent of</u>:

- fusion
- <u>advanced fuel</u> (aneutronic) fusion
- <u>alternate confinement</u> concepts
- <u>direct energy conversion</u> for fusion
- fusion energy for
  - electricity on earth
  - space applications/ power & propulsion

<u>PERSONAL</u> View

- solid-state cold fusion occurs!

  = unexpected new physics regime

  = low reaction rate D-D

  = great physics but long road to power production

- heat-producing, aneutronic, high-reaction rate fusion must be verified

  = <u>key</u> for power production

Remainder of talk, <u>assume</u> latter is feasible.

ISSUES

- Verification experiments needed?
- Technology development needed? Time scale?
- Impact on other fusion research?
- Any special problems?
- What should congress do to insure U.S. leadership?
- Implications re: creativity in U.S. fusion research?

# Verification Experiments

Heating & simultaneously
measure
Reaction Products

n
$\gamma$
$^{3}He$
$^{4}He$
T
p
x-ray
e

not easy!

# DEVELOPMENT ISSUES

- <u>Temperature</u> <u>limitations</u> & efficiency

    material limits

    reaction rate vs. temp.

- <u>Materials</u> (substitutes for palladium?)

    availability
    cost
    efficiency
    lifetime
    recycle

- Scale limits
- Control
- <u>Alternate</u> <u>configurations</u> (must it be an electrolytic cell?). What is the role of the electrical current? Improve efficiency?
- <u>Radioactivity</u>: depends on reaction mechanism and products

    must evaluate long term build-up

    <u>other</u> <u>reactions?</u>

    aneutronic? D-$^3$He, p-$^{11}$B...

    D-T? (proliferation concerns)

- <u>R&D</u> balance needed between microscopic theory vs. technology development. Analogous to hi-T superconductivity R & D.

<u>Effect on other fusion R&D</u>

If true = strong competition!

If not = ZETA revisited!

ACTION BY CONGRESS & OTHERS

- <u>don't overreact</u> -- avoid all "eggs in one basket"
- "<u>seed" money</u> -- small groups exploratory R&D fast reaction→steering committee pull universities & industry together
- acknowledge public recognition of importance of new, clean energy source
- national goals
- establish <u>funding for innovative research</u> in fusion

  <u>new office</u>?? tie together interests DOE, NSF, NASA. . .

DOE = electrical production

NSF = basic research (none now)

NASA = space power, propulsion (none now)

:

Thanks for the opportunity to comment on this extremely important and urgent issue.

U.S. HOUSE OF REPRESENTATIVES

# COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515
(202) 225-6371



May 4, 1989



Professor George Miley
University of Illinois
103 South Goodwin Avenue
Urbana, Illinois 61801

Dear Professor Miley:

I would like to express my sincere appreciation for your participation in our April 26, 1989 hearing on cold fusion. Chairman Roe and I were impressed and pleased at the conduct and substance of the hearing. The Members of the Committee, as a result of your report, now have a heightened awareness of the significant recent developments and their potential far-reaching implications.

So that we may have a timely and complete record of the testimony you presented at the hearing, I would appreciate it if you would send us a written reply within two weeks to the questions attached. Please mail your response, at your earliest convenience, to the attention of Kathryn R. Holmes, Subcommittee on Energy Research and Development, B374 Rayburn House Office Building, Washington, D.C. 20515 (202/25-8056).

I appreciate your attention to this matter, and may I wish you all the best for your continued outstanding efforts in the future.

Sincerely,

Marilyn Lloyd

MARILYN LLOYD, Chairman
Subcommittee on Energy
Research and Development

ML:Lcl
Attachment

Response to your questions are attached.
G. H. Miley

1. Professor Miley, as you are aware the United States has for many years funded efforts in magnetic fusion research. Do you think that present activities should in any way dissuade us from that work?

As I indicated in my testimony, I do not believe that cold fusion should force any near-term changes in funding for the older established fusion research programs, (i.e., magnetic or inertial confinement fusion). We simply do not know enough about cold fusion yet to allow this development to disrupt other programs. Not only must the basic physics be firmly demonstrated, but much more effort has to be put into understanding how cold fusion can be scaled up to useful power plants. As indicated in my testimony there are still a number of open questions about scaling, ranging from materials problems and economics on through to efficiency issues. If cold fusion turns out to be real and scalable, it will no doubt find an important niche in the energy mix for the future. However, until much more work is done, what that niche may be is not at all clear. Just to give an example, assume that cold fusion is found to lead to small units and/or battery type operation but is useful for large units. In that case, magnetic or inertial confinement devices might still be the preferred path for central power plant applications. In addition, there may be special features that favor one approach over the other. For example, in space propulsion, if direct drive by plasma particle emission is desired, cold fusion does not appear favorable. On the basis of power-to-weight ratio, which is crucial in such applications, it is not yet clear which approach might be best. In other words, as stated in my written testimony, I advocate patience so that a considered evaluation of this new approach is possible before any drastic decisions are made. The magnetic and inertial confinement fusion approaches have received close scrutiny over a number of years and they offer some clear advantages for certain applications. Cold fusion should receive a similar scrutiny and then be compared to the other alternatives before a decision is made.

2. What work is going on now in Europe and Japan in this "cold fusion" field? Are they likely to take an aggressive stance on development of this solid state fusion concept should it prove to be correct?

To my knowledge considerable exploratory work in cold fusion has quickly emerged in both Europe and Japan. Indeed, by coincidence, I happened to be at the University of Tokyo the day after the newspapers announced this development. The news immediately excited a number of staff there and they called me into a conference room to ask what I knew about this new discovery. It is clear that they had already grasped the concepts and were planning experiments. Since then, I know that their experiments have progressed. Also, newspaper accounts indicate that the effort is quite widespread throughout universities and industry. Most work appears focussed on verification rather than scale up, however. And, I don't think that the intensity is that different from work in the U. S. I doubt that a significant difference in effort will occur until a consensus view develops in the U. S., Europe or Japan that cold fusion is real. Thus one or more of the countries may chose to jump into a crash program. However until confirmation experiments are reported and scale up issues are better understood, I do not believe that anyone has reached that point yet.

3. Do you think because of the complications associated with this experiment, that collaborative work among several laboratories would be the best way to proceed to demonstrate that the cold fusion process is real? What would be your recommendation to the Congress as to how to proceed in the matter of assuring that this research is adequately supported at least until it is either proven or disproved?

I stand on my written testimony with respect to support for cold fusion research. It is my opinion that the immediate crucial problem is obtaining seed money to allow a variety of smaller groups to stick out innovative exploratory experiments over sufficient time to do sound work. For this reason I advocate the establishment of a fund and review board to rapidly provide "seed money" support for a diversity of small efforts. It was stressed in the oral testimony even that the national laboratories have a problem because they do not have much flexibility with "discretionary funds." I would emphasize that the situation is much more difficult in universities and other similar research groups. University faculty have virtually no discretionary funds since research is funded largely through specific grants which require proposals, lengthy reviews, and are directed at specific goals which do not allow the flexibility of exploratory studies.

The question also raises the issue of having collaborative work among several laboratories due to "complications" associated with such experiments. That should certainly be encouraged, but I believe the real issue is the interdisciplinary nature of topic which brings together a variety of areas: chemistry, physics, materials, electrochemistry, nuclear physics, diagnostics, etc. Thus, I believe that more crucial than collaboration among laboratories is the necessity for a group to bring together persons with a variety of backgrounds. Thus the interdisciplinary character of the working group would be

ιe of the factors that would be considered by a board
ınsidering "seed" money requests.

.cidentally, I cannot help but add the thought which I
ought out in written testimony that the need for "seed"
ıney to support innovative research is not restricted to
ıld fusion. A mechanism like this would be extremely
ıportant to moving magnetic and inertial confinement fusion
ead also. No doubt there are other areas of science where
.ch an approach is needed too.

Mrs. LLOYD. Thank you for your excellent testimony. I think we can turn on the lights now. Thank you very much.

Our final witness on this panel is a constituent of the Chair. Dr. Michael Saltmarsh is Associate Director for Operations of the Fusion Energy Division of the Oak Ridge National Laboratory, located in Oak Ridge, TN. He obtained his doctorate in nuclear physics from the University of Oxford in 1966, working at Grenoble, France, and CERN in Switzerland, before coming to Oak Ridge National Laboratory in 1968. He has worked in the field of magnetic fusion since 1977.

Welcome, Dr. Saltmarsh, and proceed with your testimony.

STATEMENT OF DR. MICHAEL J. SALTMARSH, ASSOCIATE DIRECTOR, FUSION ENERGY DIVISION, OAK RIDGE NATIONAL LABORATORY, OAK RIDGE, TN

Dr. SALTMARSH. Thank you, Madam Chairwoman.

You have on record my written testimony. As many of the things that I discussed there have been—

Mrs. LLOYD. And you may proceed as you wish. It will be made a part of the record.

Dr. SALTMARSH. I'll make it shorter, if possible.

I thank you for this opportunity to testify before you on behalf of my colleagues at Oak Ridge National Lab. I would like to try and summarize quickly the status of the cold fusion effort at Oak Ridge which I believe closely parallels the status at all of the DOE's other major labs and many other institutions with whom we're in contact.

It's now about a month since the initial intriguing announcements were made by the groups first from the University of Utah and later from Brigham Young. Both these groups described evidence that nuclear fusion has been observed to have occurred in simple electrolytic cells under conditions where conventional theories would have predicted immeasurably small reaction rates. As has been pointed out, despite the apparent similarities, these are quite different experiments with quite different interpretations, and most of what I shall have to say applies mostly to the Utah experiments, which we have been attempting to duplicate at Oak Ridge.

The news releases, of course, generated a great deal of excitement in the scientific community, as well as the public at large, fueled by speculation that the enormous potential of commercial fusion power, which this committee is well aware of, might well be brought closer to realization. The results triggered an immediate and concerted effort to duplicate the reported effects at many institutions, including Oak Ridge. There has even been the start of some theoretical speculation with some papers about to be published, that I'm aware of, about possible mechanisms. Thus, the normal scientific process of duplication and experimentation aimed at understand the new results has begun.

However, despite the initial high hopes and the apparent simplicity of the experiments, it has generally proved extremely difficult to reproduce the reported results. It's clear that experimental details must be important, but because the mechanisms which produced the reported results are not known, we're not sure which aspects of the experimental procedures are crucial. The task of duplication has been hampered by a lack of detailed written technical information on the precise details of the original experiments. Certainly, the scientific process of verification is far from complete, as we've seen today. Nevertheless, careful scientific scrutiny will eventually provide solid conclusions on the reproducibility of the original results and on their interpretation.

Immediately following the initial news release, work began at Oak Ridge to reproduce the new results. Today there are four separate groups actively running experiments. They represent a rather wide variety of disciplines not normally connected with what has now become to be known as the conventional approach to fusion R&D. At least a dozen different experimental configurations have been tried, most of them attempts to reproduce the electrochemical conditions reported by Dr. Pons' group. The table which forms part of my testimony shows the chronology of these things and the status as of last Friday.

We have radiation detectors of far greater sensitivity than the Utah group, and in one or two cases, of even greater sensitivity than the BYU group, which is itself a fairly sophisticated neutron detector. Calorimetry is now being used more. Three of the four groups are now using calorimetry. However, we're still not sure we have replicated all the relevant features of either of the original experiments, nor do we understand which are the relevant features.

The results so far have been negative. We have seen neither excess heat nor radiation in any of these experiments, certainly not of the scale reported by the Utah group.

It is worth emphasizing at this point that, to our knowledge, most of the other institutions with whom we are in contact have a similar status to report. At a meeting last week, where all DOE's major national labs were represented, they all reported a similar level of effort, in some cases greater, and similar results. In other words, they have not been able to confirm these results. Nationally and internationally, the vast majority of experiments have failed to duplicate the reported results, but the details of these experiments have not yet been reported in the scientific literature or at open meetings. Dr. Huggins' San Diego meeting is probably the first. Thus, the process of rigorous review, which has only just begun for the original work, hasn't even started for the attempted duplication.

It would be a real mistake to try and draw firm conclusions at this point.

The normal process of dissemination of scientific information will eventually resolve the problem. More details will become available and we will learn what are the important questions to ask. In the short term, it will be most helpful—and I understand from these proceedings it's actually happening—if one or more of the major labs would collaborate with the Utah and perhaps even the BYU groups by bringing a range of different diagnostic equipment to bear on an already working experiment.

This would short-circuit the difficulties which we and others are experiencing in obtaining an effect to study and provide a more rapid means of examining and perhaps understanding the original results.

Finally, I would like to say that for me, as for many others, the excitement generated by these reports has been incredibly stimulating. Whether these results and their interpretations will be totally or partially confirmed is still very much an open question, which can only be resolved in the course of time by careful scientific scrutiny. Whatever the final outcome, I hope that the renewed discussion of the potential promise of controlled fusion power has been very healthy.

Thank you, Mrs. Chairman. I will be pleased to answer questions.

[The prepared statement of Michael J. Saltmarsh, plus additional questions and answers for the record follow:]

Statement of

Michael J. Saltmarsh

Associate Director
Fusion Energy Division
Oak Ridge National Laboratory
Operated by Martin Marietta Energy Systems
for the Department of Energy

Oak Ridge, Tennessee

Before the

House Committee on Science, Space, and Technology

April 26, 1989

**TESTIMONY**
**COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY**
**APRIL 26, 1989**

**Michael J. Saltmarsh**
**Oak Ridge National Laboratory**

Mr. Chairman and Members of the Committee, I thank you for this opportunity to testify before you on behalf of my colleagues at the Oak Ridge National Laboratory (ORNL). I shall summarize the status of the cold fusion effort at Oak Ridge, which I believe closely parallels the status at all of the Department of Energy's (DOE) other major national laboratories.

It is now about a month since the initial intriguing announcements were made by the groups from the University of Utah and from the Brigham Young University (BYU). Both groups described evidence that nuclear fusion reactions had been observed to occur in simple electrolytic cells under conditions where conventional theories would have predicted immeasurably small reaction rates. Despite the apparent similarities between the two experiments, it should be noted that there are substantial differences between the two experimental setups, between the two sets of reported results, and between the interpretations of the data.

The news releases generated a great deal of excitement in the scientific community, as well as among the public at large. This excitement was fueled by speculation that the enormous potential benefits of controlled fusion power, which are well understood by the members of this committee, might be brought very much closer to realization.

The possible importance of these results triggered an immediate and concentrated effort to duplicate the reported effects at many scientific institutions, including ORNL and all of DOE's other major national laboratories. There has been theoretical speculation as to possible mechanisms which might account for these phenomena, with some work (e.g., that of Dr. P. L. Hagelstein at the Massachusetts Institute of Technology) about to be published. Thus, the normal scientific process of duplication and experimentation aimed at understanding the new results has begun. Despite initial high hopes and the apparent simplicity of the experiments, it has generally known as the "conventional" approach to fusion research and development. At least a dozen different experimental configurations have been tried, most of them attempts to reproduce the electrochemical conditions reported by Dr. Pons' group. The table which forms part of this testimony indicates the chronology of the ORNL experiments with the status as of last Friday, April 21. The first experimental run on March 29 had only a neutron detector for a diagnostic. It was assembled using information gleaned largely from press and television coverage and terminated fairly quickly as more technical information on the original experiments became available. With time, the experiments have become more sophisticated and have incorporated more of the features of the original work as these became known. Some have been running for two weeks or more. Radiation detectors for neutrons and/or gammas have been used in all cases. For some experiments, the neutron detection sensitivity is three to five times higher than that employed by the BYU group. Materials and equipment are available to re-commission a much larger and more efficient neutron detector if that should be required. Calorimetry is being employed by three groups at present. To date, no observations of excess heat or fusion radiation have been recorded. However, we are still not sure that we have replicated all the relevant features of either of the original experiments, nor do we understand which are the relevant features. Much more detailed descriptions of the apparatus and experimental procedures, such as material purity and preparation, electrolyte makeup, etc., are needed than are currently available.

It is worth emphasizing that, to our knowledge, most of the other institutions with whom we are in contact have a similar status to report. At a meeting last Wednesday with representatives from all of the DOE's major national laboratories, all reported similar efforts and similar results. Nationally and internationally, the vast majority of experiments have failed to duplicate the reported results, but the details of these experiments have not yet been reported in the scientific literature or at open meetings. Thus, the process of rigorous review, which has only just begun for the original work, has not yet started for the attempted duplication.

The normal process of dissemination of scientific information will eventually resolve this problem. More details will become available and we shall learn what are the important questions to ask. In the short term, it would be most helpful if one or more of the major laboratories were to collaborate with the Utah and BYU groups by bringing a range of different diagnostic equipment to bear on an already working experiment.

## CHRONOLOGY OF ORNL COLD FUSION EXPERIMENTS

| Start Date | Duration | Electrode Geometry (milimeters) | Diagnostics |
|---|---|---|---|
| 3/29/89 | 1 Day | 1 x 3 x 75 | Neutron, Gamma |
| 4/1/89 | 1 Day | 1 x 3 x 125 | Thermometry |
| 4/3/89 | In progress | 2 x 7 Disk | Neutron, Gamma |
| 4/7/89 | 2 Days | 1 x 25 x 25 | Neutron, Gamma |
| 4/8/89 | 3 Days | 6 (diam) x 50 Rod | Neutrons |
| 4/9/89 | 3 Days | 1 x 25 x 25 | Neutron, Gamma |
| 4/7/89 | 2 Days | 1 x 64 x 50 3 Plates | Neutron, Gamma |
| 4/9/89 | 2 Days | 1 x 64 x 50 3 Plates | Neutron, Gamma |
| 4/10/89 | 1 Day | 3 (diam) x 50 Rod | Neutron, Gamma |
| 4/11/89 | In Progress | 3 (diam) x 50 Rod | Neutron, Gamma |
| 4/12/89 | In Progress | 3 (diam) x 100 Rod | Neutron, Gamma, Calorimetry |
| 4/17/89 | In Progress | 3 (diam) x 50 | Neutron, Gamma |
| 4/21/89 | In Progress | 6.35 (diam) x 100 3 Rods | Neutron, Gamma, Calorimetry |
| 4/25/89 | ? | Ti + $D_2$ (no electrolysis) | Neutrons |

MICHAEL J. SALTMARSH

Oak Ridge National Laboratory

Dr. Michael J. Saltmarsh is the Associate Director for Operations of the Fusion Energy Division at the Oak Ridge National Laboratory (ORNL) located in Oak Ridge, Tennessee. He obtained his doctorate in Nuclear Physics from the University of Oxford in 1966, working at Grenoble, France, and CERN in Switzerland before coming to ORNL in 1968. He has worked in the field of magnetic fusion since 1977.

MAY 24 19[illegible]

**OAK RIDGE NATIONAL LABORATORY**

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May 15, 1989

The Honorable Marilyn Lloyd
House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D.C. 20515

Dear Representative Lloyd:

I would like to thank you for the opportunity extended to me to participate in the April 26, 1989, hearing on cold fusion. I trust the testimonies were beneficial to the work of the committee.

Attached is my response to the questions of the committee. If you have any further questions, please let me know.

Yours sincerely,

Michael Saltmarsh

Michael J. Saltmarsh
Associate Director for Operations
Fusion Energy Division

MS:jcm

Attachment

cc/att: B. R. Appleton
M. W. Rosenthal
J. Sheffield
A. W. Trivelpiece

QUESTIONS FOR DR. MIKE SALTMARSH

1. It seems that recent experiments have been focused on either calorimetric measurements or searching for energetic neutrons from the fusion reactions. Would it not be better scientifically to make these measurements simultaneously on the same experiment? Is your group now doing work along these lines?

<u>Answer</u>

It is correct that the recent experiments have tended to focus on either calorimetric measurements or a search for energetic neutrons from fusion reactions. The observation of energetic neutrons is by far the most sensitive method of detecting the most likely fusion reactions in the systems under investigation. One might expect to see neutrons from fusion even when the heat output is immeasurably small, but one would not expect to detect heat without the presence of neutrons or some other energetic radiation. Thus, it is better to simultaneously measure neutrons when making calorimetric measurements, but not necessarily better to make calorimetric measurements when measuring neutrons.

In the case of the ORNL work, all experiments to date have had neutron detection capability, although fewer have incorporated calorimetry which takes more time to set up. In the most recent experiments, the most sensitive neutron detection systems have not been combined with the most sensitive calorimetry setups as a matter of experimental convenience. However, should any experimental configuration show an effect, all associated diagnostics will be upgraded.

Mrs. LLOYD. Thank you, Dr. Saltmarsh, Dr. Miley, Dr. Decker, Dr. Jones. We certainly value your testimony.

Why has there been so much difficulty reproducing the experimental results at other locations in the United States? Dr. Jones.

Dr. JONES. With regard to the—I'll address with regard to the Brigham Young University experiment which show that neutrons are produced at a very, as I mentioned, a very low level. To give you a number, it's about a thousand neutrons per hour, roughly a trillionth of a watt of power equivalent.

Now, at that level, it's difficult to separate neutrons being produced by fusion from neutrons being produced by other sources, such as cosmic rays. It's just such a small amount of neutrons. As Dr. Saltmarsh mentioned, it requires a sophisticated detector.

Now, I was at Los Alamos yesterday, I'll be returning there tonight to conduct experiments, incidentally, with people there. You see, I've been working at Los Alamos for many years, and I have good friends there. I'll be working with a couple of groups.

I think the main reason at our level is that it's just so difficult to separate out the background. I will say that we have just begun at Los Alamos, really. I learned yesterday in our meeting that there is one group that has tried one cell of our type. The other work has been done strictly on the Pons-Fleischmann apparatus, which is a much higher level of fusion, as you know. And those results are negative. I mean, they haven't seen anything.

Now, at our level, they have done just one preliminary test and didn't see anything. But they didn't have the metal salts that we use. They didn't have all the salts. I realize it's a Campbell Soup use, but there's a history behind that going back to 1986, when we began our experiments.

Anyway, we're not claiming that—Well, that's a scientific. You can tell our soup of metals works is optimum. In fact, in recent experiments at BYU, I've reduced the number of salts dramatically and I still get the effect, reproducibly so. But now, I agree, it needs to be done. My thought is we need to work together. We will be working with scientists at Los Alamos very hard the next few days, and then there's another set of meetings, and then we'll go back to do some more experiments.

But I agree, it has to be done. And the only reason for the difficulty of our results, I would say, the bottom line is that it's such a low level.

Mrs. LLOYD. Dr. Decker, do you feel that because the work was directed less toward demonstrating nuclear fusion as an energy source and more an understanding of the process could explain some of the disparity?

Dr. DECKER. It could. At least in our case, we weren't looking for an energy source, only indirectly. We had hopes, of course, that some day this could become an energy source. However, I think part of the reason that—You go back to the last question, why it's difficult to reproduce or do the same experiment as was done in the University of Utah, it is difficult to really know exactly what they did. Even with the one paper that has been published, it's not absolutely clear. We need to have more scientific editorials, actually, really more publications that really distinguish what has happened and explain why.

Mrs. LLOYD. Dr. Miley, you referred to the level of funding in fusion research. If my memory serves me correctly, we're spending more in the area of fusion research, in the Subcommittee on Energy, Research and Development, than any other energy technology, so we are giving attention to the fusion R&D.

How do you recommend that Congress proceed in this matter? Would it be so that we can adequately support the research to see whether it's proven or disproven? Do you think that a collaborative effort among our universities or several laboratories would be the best way to proceed?

As you know, our dollars are limited. We aren't able to fund all the programs that we would like to fund, but that is the dilemma that we're in at this present time.

Dr. MILEY. Yes. I think you asked me two questions, if I understood—

Mrs. LLOYD. I think it was three.

Dr. MILEY. Three. All right, maybe more.

First of all, let me talk about the university funding and fusion in general. The difficulty is that universities often are trying to look at alternate approaches, different basic research in fusion, and the national program is aimed at large demonstration types of experiments, and there's a—the two are not completely compatible when it comes to money. So what I argue is that this is still an area, as is shown with these developments where new and innovative research can come along and it needs to be nurtured, and there needs to be a funding mechanism which supports that throughout the system, including NSF. That was the first thing.

Now, the second question you asked had to do with how to go about this verification and so on. It certainly seems to me that there is tremendous talent throughout the country to do that, but at the moment, there is not—there aren't what I would call seed money funds to really do it adequately. It's easy to jump in and do a makeshift experiment. For example, ours has volunteer help due to the enthusiasm and a little bit of money and materials borrowed, diagnostics and what have you. But as we were saying, to really unravel this, there has to be a dedicated, longer-term effort. So there has to be, I think, some mechanism on the national level for seed money to carry out some reasonable but yet very small experiments to understand what's going on.

Again, I would say the effort in the national labs is commendable, but you don't want to forget the rest of the community.

Mrs. LLOYD. We did have the problem of where we are going to draw the line, where we are going to look for levels of funding, and that's why it seemed to me, if we could have more collaborative efforts on this new vision, it would be helpful to all of us.

Dr. Saltmarsh, what do you think of the national laboratories, with all of their sophisticated diagnostic equipment and facilities, working in a collaborative arrangement with our key universities, to either disprove or prove this technology?

Dr. SALTMARSH. I think that's a very reasonable idea. As I said in my testimony, I think collaboration with Utah and perhaps with Brigham Young. Although Dr. Jones is correct, that's a very small effect and it's hard to see, it does make sense. It is much, much more efficient in my view at this time than setting up a whole new center to look at this. The center already exists, I think, for the verification process.

If I may make a point about—

Mrs. LLOYD. Certainly.

Dr. SALTMARSH. —discretionary funds, I think that is something that has come up a number of times here. The national labs were able to, with DOE and Congress' permission, use some of our funds in a discretionary manner, and that is an extremely important feature of any large enterprise. That is, in fact, what we're doing this work on now, of course.

Mrs. LLOYD. Thank you very much. Your point is well-taken. Your testimony was excellent. Again, we will be submitting questions for you in writing, and we would like you to answer and return for the record of this hearing.

My colleague, Mr. Brown.

Mr. BROWN. No questions.

Mrs. LLOYD. Mr. Bruce.

Mr. BRUCE. Dr. Miley, in your testimony, in discussion here, you highlighted the need for innovative research projects in fusion. What specific policies out at DOE should be changed in order that there may be more innovative fusion research funded?

Dr. MILEY. Well, could I begin by saying I tried to stress—the basic problem is that DOE is the only agency that has this as a major mission, so that the National Science Foundation has a policy against funding fusion research, even though it might be very basic, due to the responsibility of DOE. So I think one of the problems is correcting some of this difficulty so that other agencies like NSF can fund basic fusion research. That would be the first thing.

Within DOE itself, I would still stand by the testimony I gave several years ago, representing a university fusion association before this subcommittee, where it was recommended that a small amount of the money be put into an area which would support innovative research. I don't know if I want to call it "strange" research, but innovative research.

Dr. JONES. Can I comment on that?

Mr. BRUCE. Absolutely.

Dr. JONES. Thank you.

There is an existing agency in the Department of Energy that does support wild and crazy ideas, such as the ones I've been working on for years, seven years. That is the Advanced Energy Projects Division. That is—they do fund a number of these small, I would say, alternative energy approaches, in particular fusion, any ideas, a high-risk, high pay off research. It's equivalent to DARPA, I would say, in Defense. So there is a mechanism in the DOE for this.

Frankly, I think it ought to be encouraged. Certainly our support level has been very generous, what we need.

Mr. BRUCE. And how does that all relate to university research and the national labs, as the chairwoman was mentioning?

Dr. JONES. Of course, the money that comes from—the money that we receive from the Department of Energy through the Advanced Energy Projects Division, comes to us—I understand that most of the funding does go to universities. Some does go to industry, certainly I know that, and some does go to national labs, such as Los Alamos. So I'm not sure I'm addressing your—There is a balance there, and the idea is to work—For example, I work very closely with Los Alamos National Laboratory, and people at Idaho, the Idaho National Engineering Laboratory, on this muon catalyzed fusion research. That's funded by this agency.

Mr. BRUCE. Thank you.

Dr. Miley, given the comments of both of you about the amount of research and funding for sort of independent, small researchers, why do you think this was done by two fellows who had no association with a national lab and were not funded by any Federal research project? Why didn't we find these two guys?

Dr. MILEY. They testified earlier that this was because they were somewhat isolated and free to take an independent view. But I think it isn't necessary to be isolated to take independent views. You can be part of a research program and do the same. So I think the real issue is the freedom to do so and also the freedom to study basic issues.

This is one thing I wanted to come back and comment on Dr. Jones' comment on my comment. I think it's excellent that we have this one program in DOE that they will fund high-risk work. However, basic research in fusion, basic plasma research—now if we have cold fusion, basic solid state plasma research—needs a home. It doesn't have one.

Mr. BRUCE. Even in this innovative section, the Advanced Research Section?

Dr. MILEY. I'm saying there you have to prove it to work, has some possibility of high payoff in terms of a possible project that could develop out of it, as opposed to understanding basic physics or basic engineering science. It's the basic physics and engineering science which underpins our future developments, and in the universities, it's what underpins our ability to attract the best students and train them in basics that aren't going to be outmoded with time when one project or another comes along. So that's the lifeblood of developing a field, and that's what we need.

Mr. BRUCE. Thank you, Madam Chairman.

Mrs. LLOYD. Thank you, Mr. Bruce.

Mr. Schiff.

Mr. SCHIFF. Thank you, Madam Chair.

Gentlemen, I have two areas I would like to ask about briefly. One is, Dr. Jones, and I believe also Dr. Decker, you described fusion experiments that you had been involved with, and these were not high thermal fusion as being studied elsewhere. And yet I didn't get the impression they were identical to the Utah experiments, either. I wonder if, briefly, you could compare your experiments to the Utah experiments, in terms of seeking to produce fusion.

Dr. JONES. All right. Of course, the difference in results are dramatic. I won't go into that.

The difference in setup, we have explored a number of methods of loading hydrogen isotopes into metals—

Mr. SCHIFF. Into what?

Dr. JONES. Into metals.

Mr. SCHIFF. Okay.

Dr. JONES. I should say, by the way, that this has been funded by this same Advanced Energy Projects, this particular idea of cold nuclear fusion, since '86.

Now, we do use electrochemical cells. That was our first technique of loading hydrogen isotopes into metals. We pioneered that in May of 1986. But then in June, we started another—Oh, let me comment on that.

As soon as you get this idea that perhaps loading or forcing deuterons, for example, deuterium into metals is going to lead to fusion, which we had in early '86, and using electrochemical cells, from there the electrochemical cells can be expected to look fairly similar. I mean, there's only so many ways you can do electrochemistry. You've got to have two electrodes. We outlined the use of palladium—I should emphasize titanium has given us better results than palladium. I don't know why the palladium price has shot up so much. Titanium has given us better results. So there is that issue.

The similarity is obvious. Once you get this idea that fusion can occur in metals, you had to load hydrogen into metals. Electrochemistry comes to mind. That's the first way that we used.

We have used other techniques, too, which have since been tried. At Los Alamos, we're just going to try that tomorrow, Mr. Schiff. I'll let you know how it turns out. You'll hear about it if it works. But probably not until a scientific meeting comes along. Next week.

Anyway—

Mr. SCHIFF. Maybe we'll get the research facility at Los Alamos if they do well.

Dr. JONES. Yeah. Now, the idea there is we're adding deuterium under pressure, and the deuterium that is forced into metals, we're using titanium. We've done some of this at BYU and our results were tantalizing but not real significant.

The people at Ferscotti in Italy used a large quantity of titanium, the same procedure, identically, high pressure deuterium, very simple, no electrochemical magic or complications. Just pressured deuterium on metals. Boy, they got a very interesting fusion rate. It's at roughly our level. I'm not talking about energy, again, but I'm talking about fusion in metals. We'll try to do this at Los Alamos, I believe, in the next few days, hopefully tomorrow.

Mr. SCHIFF. They confirmed fusion through these other processes?

Dr. JONES. Yes, it's been confirmed twice in Italy, once at Ferscotti near Rome, and once by the Genoa group. The Italians are very into this fusion fever, too.

And, by the way, BYU has a collaboration with the University of Bologna, and we have some positive results out of that, too. That's electrochemical. But again, these are small level neutrons.

Mr. SCHIFF. Dr. Decker, you're having some slightly different experimental approach, is that correct?

Dr. DECKER. Not being Dr. Jones, he does the experiments. I'm just the department chairman and watch what he does.

Mr. SCHIFF. Can I take up a second subject, then, a final subject.

We were urged, as you may have heard, by the president of the University of Utah and by Mr. Magaziner, his consultant, to immediately invest in, if you will, a research institute on cold energy fusion, based upon Drs. Fleischmann's and Pons' experiments. I don't think I'm meaning any insult to the gentlemen from the president of Utah to suggest I assume he wants the center to be in Utah. I didn't hear him suggest it ought to be anywhere else except in Utah.

The suggestion was that if Congress does not authorize and appropriate the funds for this investment, that next year we'll be purchasing cold fusion-powered automobiles from the Japanese and the Western Europeans. On the other hand, I heard Mr. Jones say that the difference between that experiment and commercial productivity was the difference between a dollar and the national debt—a low blow, but a fair one, I think, to make before the committee. And Dr. Miley referred to a ZETA experiment in the United Kingdom.

I wonder if I could take a second—Dr. Miley, I'm not familiar with that. You referred to it as a fusion experiment in England, the United Kingdom, that went awry, I believe. Is that right, sir?

Dr. MILEY. It was, I recall, a reverse field pinch experiment, I would say a high temperature plasma experiment. High neutron rates were measured there, indicating that they should achieve break even fairly quickly. However, it turned out the neutrons were not coming from the bulk of the plasma but due to interactions that were created by instabilities in the plasma and, hence, couldn't be scaled up to a reactor. It was an interesting phenomenon but not something that led to an ultimate system. And so all the publicity which had surrounded that backfired and it really caused the public and funding agency to turn against it.

Mr. SCHIFF. I think you even testified, Dr. Miley, that that experiment set back fusion research in the United Kingdom for a number of years, if I understood you correct, because of the poor result.

Dr. MILEY. That's my private view. I don't know if they share that part of it.

Mr. SCHIFF. What I'm getting at in conclusion here, and would ask each of you briefly, we're being counseled on the one hand and we, except for perhaps one of our Members, do not have scientific backgrounds. I think you know that. We're being counseled on the one hand, "Do it now, or be in second place forever." And we're being kind of implied on the other hand, "But if it goes wrong, it could be more than just an 'oh, well'. It could be a serious side effect in addition to cost."

If you were us, in terms of funding a research center based around the cold fusion experiment, what would you do?

Dr. DECKER. I think I'd still want more confirmation than we have right now before I jumped in completely. I'm not sure what is meant by a "research center", but at least from what Mr. Magaziner indicated, he is strongly encouraged that we start off into the technology immediately before we have really confirmed very much of the science.

I think there's enough evidence that we should put some money into trying to confirm the science and finding out what is truly the source of the heat that is observed. I'm not sure we're ready to do the technology yet. At least I think, if my grandfather were here, he would say to me, don't say "gee haw" to the oxen before you attach them to the covered wagon.

[Laughter.]

Mr. SCHIFF. Anyone else care to—Dr. Saltmarsh.

Dr. SALTMARSH. Yes, I would agree. I don't think we're talking about a very long time scale here. There's a workshop scheduled at Santa Fe at the end of May, where they may get 600 or 1,000 people involved in this. I don't know whether Steve is right, a month or two, but two or three months would be my guess and we'll have some fairly firm idea. So I wouldn't rush into it, either, not on that time scale.

Mr. SCHIFF. Dr. Jones?

Dr. JONES. Amen.

Mr. SCHIFF. Thank you very much, gentlemen.

Thank you, Madam Chair.

Mrs. LLOYD. Mr. Brown.

Mr. BROWN. Just a comment on the questions raised by Mr. Schiff.

We have, of course, a number of proposals to establish either institutes or major consortia involving Government, industry and universities in fields outside of the fusion field. There is a real problem with our ability to utilize the results of our research as quickly as other nations are, and it may be that we do need to consider some alternative mechanisms. And fusion would not be the reason for that. The reasons would be broader. But a fusion research and development program might benefit from that in the long run.

I would tend to concur that we don't need to act precipitously, but what precipitous is, it varies in the eye of the beholder sometime. We think it's precipitous around here if we get something done in two years. We may need to move a little bit faster.

I wanted to just raise one question. It's obvious from the thrust of all the testimony that we've heard that this development has stemmed out of a base of research in the field of materials, and particularly hydrogen, that's been going on for some time. We have programs to fund materials research and hydrogen research and various areas, but it has been a question as to whether the level of such funding was adequate. I would really like to get out of this testimony some support for increasing the funding levels for some of the basic research that we have been doing in materials science and in hydrogen as ways of utilizing it and ways to—to find cheaper ways to produce it and things of that sort.

This is a leading question. I'm going to ask you to comment whether or not you agree that it would be helpful if we could increase the level of funding for basic materials sciences and hydrogen.

If any of you say no, I'll be very disappointed.

Dr. JONES. You know, I must admit my research focuses mainly on finding out scientific facts, and the funding from DOE has been so gracious, I haven't had to worry too much.

But let me say that I do think there is a need in the country for support, as I look around and interact with other scientists, a need for support for basic research which could potentially lead to applications but doesn't tie into those possibilities. Because ultimately, progress comes from these starts, the new information. Then applications come later. I think that's obvious. But even though it's obvious, I don't know that we have a good mechanism for funding this basic research.

Dr. Miley mentioned basic plasma physics research and basic solid state physics research. I'm sure there are other areas. But I do think there is room for small scale, perhaps, but innovative research that should be supported in the basic research areas without pegging that to applications. I definitely agree with that.

Mr. BROWN. Yes. Well, I'm not calling for research aimed at applications. It seems clear from this work, from the work in superconductivity, from the problems associated with the superconducting supercollider, a lot of these are materials based problems that need a recognition at least that there is more research funding needed in these areas.

Dr. DECKER. I think in the area of materials research, that is something that can be done at the smaller universities fairly efficiently—

Mr. BROWN. Right. It's small funds.

Dr. DECKER. It's true that a lot of the money does go to the big universities with more political pull. It would be nice, even though Steve Jones feels all right, I have several other faculty members on my department who would like to have funding in our smaller university. So I would recommend yes.

Mr. BROWN. Yes. Okay.

Dr. MILEY. Not being in that area, I have trouble commenting with any expertise. But if you would expand this to say basic energy research, then I would say yes.

Mr. BROWN. Okay.

Dr. Saltmarsh.

Dr. SALTMARSH. I really don't have enough information to answer the question directly, but a related question which I do have some experiences, the discretionary part of any scientific enterprise, at LAMPF, at Los Alamos Meson Physics Facility, where I was on the program advisory committee for some time, the director always used to retain unto himself the ability to direct five percent of the beam time on wild-eyed ideas. Similar things have to be done at major radiotelescopes and so on, and I think that, in my experience, is something that often gets forgotten.

Mr. BROWN. That issue comes up frequently in discussing the way the laboratories, the national laboratories manage their own resources. I think the chairlady would agree that this subject has been one of considerable interest in allowing for more discretion on the part of laboratory directors to undertake targets of opportunity—funding for interesting developments that may not have been inserted into their budget two years before when they were preparing it, but now appear to be promising. This falls within that kind of category.

Thank you.

Mrs. LLOYD. Mr. Brown, your comments are well taken. I think here the subcommittee needs to direct a little bit more attention and effort.

Mr. Nielson.

Mr. NIELSON. Thank you, Madam Chairwoman.

I only have two questions. The first question has to do with the [ature article that you just submitted. You submitted a paper for ublication, as did Dr. Pons and Dr. Fleischmann. The editors of [ature asked you certain questions; they asked them certain ques- ons. You responded and your article will be printed, their article ill not be printed. They said they did not want it because it would eveal some trade secrets and some patentable processes.

How is it that you were able to respond to Nature and have your rticle printed with the detail they asked for, and the University of tah refused to do so?

Dr. JONES. Well, I won't attempt to comment for—I think they entioned some things, Congressman Nielson, on that. I won't at- mpt to answer for them.

For our side, I will say that we have, in fact, done some patent pplications, but these do not relate to energy. We don't feel that ey—that energy applications are relevant on the horizon at the oment, so obviously—But we have done some patent applications. 'e've gone ahead and done those quickly.

You see, the patenting process, as I'm sure you know, once some- ing is published, you have a year in the United States to secure tents. Well, that's—I don't know a great deal about that. But yway, our patents are in today, as of—you know, they're already

.

Mr. NIELSON. The second question relates to the same subject. If, fact—You made two comments. You made a comment you have be verified, we have to hitch the hoses to the wagon, oxen to the agon, I guess you said. Is it easier to get confirmation from your ers if you publish a full article with all the scientific aspects re- ested, than it is if you withhold essential details and let every- dy else try to do it, duplicate it?

Dr. JONES. Absolutely. The best thing is to publish all the details at you can. I mean, after all, we hope for the scrutiny of the sci- tific community to find out whether what we've done is correct not. There is a scientific community out there that's internation- . It's really a marvelous group of men and women, it really is. I ve a great deal of respect as I interact with people all over the orld—Russia, Italy, all over. These people are kind, but they're so very meticulous. And when an idea, especially a new idea like is one, hits the scientific filter, it's very carefully checked. Ex- riments are done, theories are created. But at this point—This es me a chance to mention that we are at that filter now. We ve not passed the filter, and it will take, I think, a couple of onths to pass or not the filter.

But it's a mechanism that has been developed over, I suppose, ndreds of years, to filter out the wheat from the chaff. Right w in the press, for example, it's not their fault that there's a lot wheat and chaff out there. The scientific filtering process has to given just a little time to filter this out.

Mr. NIELSON. I want to thank the chairwoman for giving me the portunity to say something. Just to make one comment.

I am very interested in this project work. I have a lot of pride, th for me and at the University of Utah. I would like very much for it to be true, and I hope against hope that we can verify that to everyone's satisfaction and that this committee will provide the proper funding.

I really believe that we should take certain risks. Someone said a certain amount of money should go to test things out. So I'm going to support the recommendation that Mr. Walker made, and perhaps the higher one as well, because I think it's an important investment. A very little bit of money could produce a tremendous return for the country, and I would like to see our country get the first crack at it.

But I'm like you. I would like to see all the details published in such a way that it can be verified and repeated, because then you have the whole world at your feet. I believe that's the suggestion I would make to the chairwoman.

Thank you again.

Mrs. LLOYD. Thank you very much, Mr. Nielson.

Thank you, distinguished panelists. You have provided excellent testimony and we're very appreciative.

Our final panel of witnesses today includes Dr. Harold Furth, who is certainly no stranger to this committee. He is director of the Plasma Physics Laboratory, Princeton University, Princeton, NY, and Dr. Ron Ballinger from Massachusetts Institute of Technology, in the Nuclear Engineering Department.

Gentlemen, welcome. It's good to see you this afternoon. We are running a little over time, but I think you'll agree that the testimony has been excellent. Harold, you may proceed as you wish. Your entire statement will be made a part of the record.

## STATEMENT OF DR. HAROLD P. FURTH, DIRECTOR, PRINCETON PLASMA PHYSICS LABORATORY, PRINCETON, NEW JERSEY

Dr. FURTH. Madam Chairman, members of the committee, thank you for inviting me.

Mrs. LLOYD. Please turn on your speaker, Dr. Furth.

Dr. FURTH. Thank you once more for inviting me.

The subject of my written testimony is the verification of the reality of cold fusion, and without going once more into all the nitty-gritties of that, I would like to summarize the main points of logic for you which have been mentioned also by previous speakers.

Now, one very important remarks is that there is not one kind of cold fusion; there are two kinds of cold fusion, which are quite distinct, and one might call the one calorimetric cold fusion because what one does there is to use a calorimeter and discover excess heat which it is believed cannot be accounted for by chemical means. That is the heart of the most exciting discovery made at the University of Utah.

But then, quite separate from that, there is nuclear cold fusion, where you make nuclear reaction product measurements and you infer that accompanying heat, of course, has been released, and that has been done at Brigham Young and a number of other places, including also the University of Utah.

Now, those two findings so far inhabit quite different worlds, and it is not yet clear, in fact, that there is a connection between them. For instance, one could ask do the findings or nonfindings of nuclear cold fusion evidence, do they serve a useful purpose in proving or disproving the reality of the calorimetric cold fusion, and the answer is no. As you heard over and over today, they are in different worlds which are apart by at least a factor of a billion, and to come up with a new example of that, which I think is becoming increasingly timely, that factor is sort of like the difference between your personal lunch money and feeding the whole human race. That's quite a large factor. Therefore, coming up with the lunch money doesn't fee the human race, and coming up with some billionth of the energy release, inferring that from nuclear reaction products, doesn't account for the energy release.

But on the bright side, the opposite is also true, that if one should fail to confirm the reality of the nuclear evidence, that wouldn't damage necessarily the reality of the calorimetric evidence. Because at the moment, the argument is that the excess heat is not produced by known fusion reactions at all, but it is produced by a new kind of fusion reaction not previously known which does not have visible nuclear reaction products. And so if all evidence of nuclear cold fusion went away, it would not undermine the case for calorimetric cold fusion. So in a sense, they're really quite separate.

In order to verify the reality of the calorimetric cold fusion, one has to explore it by calorimetric and chemical means, and there a number of approaches are really quite obvious. One experiment is to do the light water control experiment. This was discussed two weeks ago at the American Chemical Society meeting in Dallas. The idea here is that when you have found, or think you have found, excess heat in heavy water, then you repeat the experiment in light water and see if there is excess heat there.

Now, Dr. Huggins here has said he has done this and has found the excess heat in heavy water, not in light water. There are other groups who have had precisely the reverse result, and there are still other groups who have found excess heat in neither heavy water nor light water, from all of which I conclude merely that they can't all be right and I leave it to the electrochemists to sort out the truth.

I am rather struck by the notion that the University of Utah surely must have considered doing control experiments in light water after these many results in heavy water, and I was struck by the remark of Dr. Fleischmann when the subject came up here, namely, that he didn't want to talk about it. So I would say, if I were Sherlock Holmes, I would refer to this as 'The Case of the Missing Control Experiment" and I would ponder what it meant. I have a feeling it does mean something, and it is conceivable to me that if this committee were to encourage Dr. Fleischmann and Dr. Pons to say something further on this topic, they may, indeed, have already further things to say. Indeed, they may have control experiements. It is just that this is among the evidence that has not yet been laid on the table.

Now, aside from these calorimetric experiments to seek to verify the reality of the excess heat as fusion, one very obvious thing to do, which has occurred to many people, is to look for helium production, because so far, even the most erudite and elegant of our theorists have not been able to find a way to release microscopic heat without either visible radiation or helium production. Therefore, in the heart of these palladium rods there must be lurking helium which accumulates as the excess heat is produced. And here, what one should not do is just see if there is some helium, because there is some helium everywhere—in metal, in glass—ready to come out. What you should look for is a very specific thing, namely, that the helium accumulates as it is produced in the course of these alleged exotic new fusion reactions, and it should then be produced in a very definite known ratio between helium and excess heat.

So what I would recommend is very simple, could really be done in a matter of a week, that people who have done successful excess heat experiments should chop up of few rods, and let's have some that have produced a lot of heat and some that have produced a little heat, and some that haven't produced any heat at all, and we'll number them and we'll make a little list saying just how much heat each produced and we'll send them out to a laboratory that does analyses of helium. Then they will make a little list of how much helium there is in these rods, and then we will put together those two lists. That, to me, would be an enormously interesting and significant test, which could be carried out next week, not next month or next year.

Okay. So I have said by way of introduction that the nuclear cold fusion effects don't have the capability to prove or disprove this microscopic calorimetric cold fusion.

The reverse is also true, that if it were to happen that all the excess heat in the end were, after all, explained by chemical means, that would not necessarily caste out the nuclear cold fusion results such as those reported here by Dr. Jones. They also, although not of such obvious dramatic energy interest, are of great physical interest and should be pursued. Now, those, again, need to be investigated by the techniques of nuclear physics. One has to make sure that the signals are above the background; one has to make sure one is really seeing neutrons from fusion events and not from some other cause, and so forth.

In conclusion, let me echo a statement very eloquently expressed by Dr. Fleischmann, namely, the overriding fact in this situation is that society needs fusion, and the great positive future to me of the recent events is that they have drawn the attention of society very vigorously, far more vigorously than we could have done, I'm sure, to this need for fusion as the great energy crunch of the next century comes into view and as we need to prepare to deal with it in some economical and environmentally benign manner.

So that's a very great plus. But the immediate need is verification of the reality of the thing that we've been talking about for the last six hours. That is a very fundamental point.

It's certainly true that one should not waste too much time in a breakthrough situation going from science into technology. It's wonderful if you can overlap it, but don't skip the verification stage, particularly if it is really only in some cases a matter of weeks and in other cases a matter of months, or one month. So I think the committee could do a wonderful service by focusing attention on that point.

If that is done, then the truth will come out in fairly short order. Then maybe it will point one way or maybe it'll point another way, and in either case, we should pursue the best road to fusion power, and we should make a good plan, and maybe, in view of what has been happening recently, we will pursue that plan somewhat more expeditiously and vigorously than fusion power has been pursued in recent years.

I know that in this case I'm only telling Mrs. Lloyd and Mr. Roe what they well know. They have been wonderfully forceful and insightful in supporting fusion over the years, and I have full confidence that they will find the right way.

Thank you very much.

[The prepared statement of Harold Furth, plus additional questions and answers for the record follow:]

Statement of Harold P. Furth
Director, Princeton Plasma Physics Laboratory
before the
House Committee on Science, Space, and Technology
26 April 1989

Mr. Chairman, Members of the Committee:

Thank you for inviting me to comment on the subject of "cold fusion energy." The prudent leadership that this Committee has long provided in the fusion area will be particularly valuable in encouraging the science community to reach a speedy resolution of its present puzzles and to apply the resultant lessons for the advancement of fusion research. In my testimony, I will try to outline the present status of the process of experimental verification of "cold fusion" and will comment very briefly on the potential for practical applications.

Two distinct types of experimental data have been cited as evidence for the existence of "cold fusion": (1) calorimetric data involving unexplained heat releases at levels of about one watt or more; (2) a variety of nuclear-physics data, such as unexplained levels of nuclear radiation or tritium concentration, corresponding to fusion-energy releases in the range $10^{-12}$ - $10^{-8}$ watts -- typically at least one billion times smaller than would be required to account for the calorimetric "excess heat". Because of the vast shortfall in the magnitude of the observed nuclear phenomena, they cannot be used as experimental proof of the "cold fusion" interpretation of "excess heat"; on the other hand, even a demonstration of the complete absence of observable nuclear phenomena would not constitute a clear-cut <u>disproof</u>, since the possible existence of new radiation-free D-D fusion reaction modes is still under theoretical debate.

A more definitive experimental approach, currently in progress at a number of laboratories, is to search for traces of the D-D reaction products helium 4 and helium 3 (as well as continuing the search for tritium). As in the case of the nuclear-radiation measurements, the proof of the "cold fusion" hypothesis depends on establishing a <u>quantitative</u> correspondence between the measured concentration of helium atoms and the associated "excess heat" production. The helium measurement allows much more effective discrimination against the natural background, because the observed helium concentration should be cumulative -- increasing in proportion to the reaction time and the level of "excess-heating" power. Since there is little hope for a theoretical model that accounts for deuterium-fueled fusion power without any production of helium (or tritium), failure to observe the appropriate accumulation rates would constitute a clear-cut disproof of the "cold fusion" interpretation of "excess heat."

While awaiting the results of sensitive helium-accumulation studies, experimental groups that have observed the "excess heat" phenomenon should be able to obtain significant additional evidence by simple calorimetric means, If "excess heat" is being found during the electrolysis of heavy water ($D_2O$), the most obvious control experiment is to repeat the same electrolytic procedures using ordinary light water ($H_2O$). If "excess heat" continues to be observed, the "cold fusion" interpretation can still be maintained by invoking H-D fusion reactions between hydrogen and "background deuterium" -- such as the natural deuterium content in light water (about one part in 6000). In that case, the decisive control experiment would be to mix small quantities of heavy water into the light water: The "excess heat" from H-D reactions should be observed to rise proportionately as the fraction of heavy water is increased.

When carrying out light-water control experiments, it is important to keep in mind that gross differences in chemical behavior are known to characterize the electrolysis of heavy and light water: These differences have been exploited on an industrial scale for the separation of heavy water from light water. Provided that the control experiment addresses itself specifically to the comparison of "excess heats" (rather than to the comparison of gross electrolytic heating effects), a systematic finding of large "excess heat" in heavy-water experiments and "no excess heat" in the light-water control experiments would provide a significantly encouraging sign in favor of the "cold fusion" hypothesis.

The documentation of calorimetric results pointing towards a nuclear energy source would further stimulate the search for the responsible nuclear mechanism. On the other hand, the emergence of a non-nuclear explanation of calorimetric "excess heat" would not rule out the "cold fusion" interpretation of the much smaller unexplained energy releases being projected by some of the experimental groups that are currently studying nuclear-physics phenomena in electrolytic and non-electrolytic systems. The accompanying versions of "cold fusion" theory invoke only the previously known D-D fusion reactions, so that "excess heat" of calorimetrically measurable magnitudes would not be expected in the first place.

To test the reality of the "cold fusion" interpretation of nuclear-physics phenomena, a number of control experiments would be helpful: (1) In the case of deuterium-based experiments, verify whether the observed neutron counts can be identified with the characteristic 2.45-MeV fusion neutron emission -- at levels that are decisively above the radiation background. (2) Carry out the appropriate control experiments, using hydrogen. (3) Investigate hydrogen-deuterium mixtures, look for the characteristic 5.5-MeV gamma-ray emission of the H-D reaction, and compare its magnitude with that of the D-D neutron emission.

During the past month, the Princeton Plasma Physics Laboratory, in collaboration with Electron Transfer Technologies of Princeton, New Jersey, has sought to reproduce both the calorimetric and nuclear D-D experiments and has initiated a modest experimental research effort directed along the lines of item (3) above. A fundamental physics motivation for the latter experiments is that the observation of H-D reactions would be more clearly indicative of quantum-mechanical tunnelling than the observation of D-D reactions, and would therefore provide a more direct test of "cold fusion" theory.

Historical experience with the exploitation of scientific breakthroughs shows that the most useful applications are seldom recognized from the outset. Considering that the very existence of "cold fusion" remains uncertain at this point, comments on possible practical applications can clearly take only a very general form.

If the calorimetric "excess heat" is found to be of nuclear origin, the billionfold shortfall of nuclear radiation, which is currently a disappointment to experimentalists, could turn into an asset: The production of radioactive by-products of fusion energy would be further reduced relative to the fission alternative. On the other hand, the conversion of "cold fusion" heat releases into useful energy might present problems of thermodynamic efficiency. In any case, it is worth noting that the advantage of small physical size, which is currently helpful in carrying out low-powered "cold fusion" experiments, is unlikely to project to correspondingly small-sized future power plants, since physical size is correlated with power-handling capability, somewhat independent of the nature of the energy source.

Summary

Experiments decisively proving or disproving the reality of "cold fusion energy" remain to be done.

The explanation of calorimetrically measurable levels of "excess heating" (about one watt or more) can be pursued by simple control experiments using light-water or light-water/heavy-water mixtures, or (most definitively) measuring the accumulation of helium.

The explanation of nuclear "cold fusion" phenomena (with implied energy releases in the range $10^{-12}$ - $10^{-8}$ watts) needs to be pursued by means of more powerful nuclear-radiation diagnostics, along with control experiments using deuterium-hydrogen mixtures.

In our present state of knowledge, the practical potential of "cold fusion" cannot be assessed, but the emergence of such a remarkable new phenomenon of physics would clearly be exciting and promising.

Over the years, this Committee has played a leading role in fostering fusion-energy research and public awareness of the potential benefits of fusion as an environmentally benign, inexhaustible energy source. Whatever the immediate resolution may be concerning the reality and utility of "cold fusion energy," some progress may have been made during the past month in focussing the world's attention on the potential value of a realistic long-term strategy for the achievement of the fusion-energy goal.

1. The members of this Committee would appreciate learning of your views concerning future funding support for fusion energy R&D programs - especially do you foresee a significant shift in emphasis from one area, such as magnetic confinement programs, to other approaches.

In response to this general question, I should like to begin with specific comments on the relationship between magnetic confinement research and "cold fusion" research. Since the time of the Committee's April 26, 1989 hearing, the trend of the experimental evidence has been unfavorable to the interpretation of calorimetrically observed "excess heat" in terms of "cold fusion energy." A number of powerfully instrumented experimental studies -- including one at Harwell that received advice and materials from Professor Fleischmann -- have yielded negative results. Radiation experts have challenged the reality of the "fusion neutron" emissions that had been reported to accompany the "excess heat" phenomenon. Chemists have pointed out that electrochemical energy-storage mechanisms plus ambiguities in calorimetric technique have the potential to account for the various types of "excess heat" phenomena that have been reported. In response, the proponents of "cold fusion" should be encouraged to improve their case by permitting open inspection of experimental apparatus, procedures, and results, particularly in the area of helium "ash" accumulation. Until some sort of positive experimental results are produced, however, there seems to be no basis for associating the finding of "excess heat" with the quest for nuclear fusion energy.

Responding more generally to the Committee's question, I should like to note that very strong progress is currently being made in the understanding of magnetic confinement physics and in the achievement of reactorlike magnetic confinement objectives. As members of the Committee have already pointed out, the productivity of magnetic fusion could be further enhanced by making full use of existing experimental facilities and accelerating the construction of the major next-generation research device, the CIT. Continued reprogramming of funds from the magnetic-confinement effort into other areas is likely to have a significantly damaging net effect on the strength of the U.S. fusion effort.

Harold P. Furth
May 11, 1989

U.S. HOUSE OF REPRESENTATIVES

# COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, DC 20515
(202) 225-6371



May 4, 1989



Dr. Harold Furth
Princeton University
P.O. Box 451
Princeton, New Jersey 08543

Dear Dr. Furth:

I would like to express my sincere appreciation for your participation in our April 26, 1989 hearing on ccold fusion. Chairman Roe and I were impressed and pleased at the coere appreciation for your participation in our April 26, 1989 hearing on cold fusion. Chairman Roe and I were impressed and pleased at the conduct and substance of the hearing. The Members of the Committee, as a result of your report, now have a heightened awareness of the significant recent developments and their potential far-reaching implications.

So that we may have a timely and complete record of the testimony you presented at the hearing, I would appreciate it if you would send us a written reply to the questions attached. Please mail your response, at your earliest convenience, to the attention of Kathryn R. Holmes, Subcommittee on Energy Research and Development, B374 Rayburn House Office Building, Washington, D.C. 20515 (202/25-8056).

I appreciate your attention to this matter, and may I wish you all the best for your continued outstanding efforts in the future.

Sincerely,

Marilyn Lloyd

MARILYN LLOYD, Chairman
Subcommittee on Energy
Research and Development

ML:Lcl
Attachment

**Questions for Dr. Harold Furth**

1. The members of this Committee would appreciate learning of your views concerning future funding support for fusion energy R&D programs - especially do you foresee a significant shift in emphasis from one area, such as magnetic confinement programs, to other approaches.

Mrs. LLOYD. Thank you, Dr. Furth.

Dr. Ballinger, please proceed.

STATEMENT OF DR. RONALD G. BALLINGER, DEPARTMENT OF NUCLEAR ENGINEERING, DEPARTMENT OF MATERIALS SCIENCE AND ENGINEERING, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, CAMBRIDGE, MASSACHUSETTS

Dr. BALLINGER. Thank you, Madam Chairman, and members of this committee for the opportunity to come and make what will be the last comments, I believe. Since most of what I have to say has already been said quite forcefully, I think I will just probably try to conclude.

I should say first that I'm just one member of a team at MIT that includes people from the Plasma Fusion Center, the director of which is Ron Parker, who I'm sure you're familiar with, and the Department of Chemistry, and the head of that department, Mark Wrighton, is also somebody who I'm fairly sure you're familiar with, and that the team contains many members. The list of those members is in the back of my testimony. But the point is the team is composed of experts in the fields of physics, plasma physics, chemistry, electrochemistry, radiation detection, and all of the disciplines which we feel are essential in the verification of the University of Utah results.

Since the reports of these results, a number of—

Mrs. LLOYD. Excuse me, Dr. Ballinger. We have a vote. Although we have five minutes, before you get into your testimony, I think it might be prudent that we vote and then we'll be back as soon as we possibly can to resume your testimony.

[Whereupon, the committee was in recess.]

Mrs. LLOYD. We will resume our hearing. Dr. Ballinger, you may proceed with your testimony.

Dr. BALLINGER. I'm afraid, if we have another vote, that I'll be the only person here. The subcommittee is down to one.

Mrs. LLOYD. It does happen at the end of the day.

Dr. BALLINGER. I'll just continue where I left off.

Since the reports of these results, a number of teams around the world have been working, to say the least, at a feverish pace to try to duplicate the results. To my knowledge, however, with the possible exception of the people at Stanford, and in Europe and the USSR, which I have no personal knowledge, we have not had a single confirmation, scientific confirmation of either the reported neutron emissions from the experiment, nor the excess heat. I want to be careful when I say scientifically verified.

This is in spite of the fact that—at MIT, we are in that category, I should say. This is in spite of the fact that we and others are employing methods of radiation detection which are at least ten times more sensitive than the University of Utah experiments, and calorimetry, which is on the same order, probably ten times more sensitive than has been used originally.

In the scientific community, I should say that the soundness of experimental or theoretical research results—and I'm not saying anything new here—they're evaluated through peer review. For results such as those reported, whose potential impact on the scientific community and the world are so great, this review is absolutely essential. Unfortunately, for reasons which are not clear to me, this has not happened in this case, at least so far. The level of detail concerning the experimental procedures, conditions and results necessary for verification of the experiments at the University of Utah have not been forthcoming. At the same time, we've seen almost daily articles in the press, often conflicting with the facts, and they have raised public expectations, possibly for naught, that the energy problem has been solved. I think those of you who are as old as I am remember the "too cheap to meter" statement that was made about another source of energy not too long ago. And so we in the scientific community are left to attempt to reproduce or verify a potentially major scientific breakthrough while getting the experimental details from the Wall Street Journal.

I'm not singling out that publication. It happens to be the one that I read in the morning. I'm from Boston, and The Globe is probably here.

Experiments like this, which are conducted in haste on insufficient detail, coupled with premature release of results, have often resulted in retractions and embarrassment on the part of the scientific community. I guess we're all human.

The result of this unsatisfactory situation has been that a healthy skepticism and in some cases a distrust of the reported results has developed. We at MIT share this skepticism.

At the risk of becoming a bit too technical by my comments, I think I should comment a little bit on the source of at least our skepticism. As I mentioned earlier, the major results reported by the University of Utah are that there have been a generation of excess heat and the measurement of neutron radiation. By excess heat I mean—and I'm sure we're all aware by the end of the day—that there's been a measurement of more energy produced than has been input to the system.

From our standpoint, the key point of verification is the detection of neutron radiation. This has been reported in their published paper. From an engineering point of view, however, the important of excess heat is the critical component. On these two critical points, we have found that the results reported in the few available published documents are inconclusive or unclear.

For example, with respect to the detection of neutrons, the reported results are confusing. They either do not agree with or are not presented completely enough to show that they are consistent with what one would expect from the emission of neutrons from the deuterium fusion reaction. The gamma ray spectrum that is reported in the paper does not have the shape and intensity that demonstrates the increase in the number of detected neutrons above what's normal background. Further, the reported rate of neutron emission and level of tritium production are consistent with natural background. The results have, nevertheless, been reported as significant. These inconsistencies can only be resolved by a full disclosure of the details of the experimental measurement for examination by the scientific community. Until such time as this occurs, we feel that the data is really insufficient to demonstrate the presence of neutrons.

In conclusion, I feel that it's safe to say that the scientific community is excited, very excited, about the possibility of a significant advance in this area. From my standpoint as an electrochemist, it's one of the few times I've started talking to the physicists. It's a very, very, very welcome and it's an exciting topic. But it is, at the same time, skeptical of the results that have not been verified and frustrated by the methods by which the discovery has been handled, both in the scientific and nonscientific community.

Thank you.

[The prepared statement of Ronald G. Ballinger follows:]

# Comments on "Cold Fusion"

Testimony presented to

Committee on Science, Space, and Technology

U.S. House of Representatives

Washington, D. C.

by

Professor Ronald G. Ballinger

Department of Nuclear Engineering
Department of Materials Science and Engineering

Massachusetts Institute of Technology
Cambridge, Massachusetts

April 26, 1989

Mr. Chairman, Members of the Committee:

I am Ronald Ballinger, a faculty member of the Departments of Nuclear Engineering and Materials Science and Engineering at the Massachusetts Institute of Technology. I am very grateful for your invitation to convey my views related to the recent reports of the achievement of "cold fusion".

I am a member of an interdisciplinary team at MIT that is involved in an attempt to reproduce the reported "Cold Fusion" results of Professors Pons and Fleischmann of the University of Utah. The teams' principals include Dr. Ronald R. Parker, Director of MIT's Plasma Fusion Center; Professor Mark S. Wrighton, Head of the Chemistry Department; and myself. (A complete list of team members and areas of expertise is included). The team is composed of experts in the fields of physical metallurgy, electrochemistry, plasma physics, instrumentation, and radiation detection. The team has been involved in attempts to reproduce the results, reported by Professors Pons and Fleischmann since shortly after their results were released to the press and for publication in the *Journal of Electroanalytical Chemistry*.

As I am sure that you and the members of this committee are aware, any breakthrough in the area of energy production that has the potential to supply current and future energy needs in a non polluting manner must be given serious attention. Quite apart from its impact on basic science, the results recently reported by Professors Pons and Fleischmann, should they prove to be correct, represent such a breakthrough. The basic nature of their results have been described and discussed by earlier testimony before this committee. Basically, the team at the University of Utah has reported the fusion of Deuterium atoms in a palladium matrix at room temperature.

As evidence that "cold fusion" has taken place the production of excess heat and neutron radiation has been reported. The reported magnitude of both of these is such that their presence could be verified by other investigators.

Much more modest results have been reported by a team of investigators at Brigham Young University. We feel that it is important to distinguish between the BYU results, which are of scientific interest but of limited or no practical significance and those of the University of Utah which, should they prove correct have major implications for future energy production.

Since the reports of these results, a number of teams worldwide have been attempting to reproduce these results. To my knowledge, with the possible exception of the Stanford results and results from Europe and the USSR of which I have no personal knowledge, no team has been successful. As far as the results of attempts by the team at MIT are concerned, we have been thus far unable to scientifically verify any of these results. This is in spite of the fact that we are employing calorimitry and radiation detection methods of even greater sophistication and sensitivity than those of the University of Utah. Having said this I can assure you that these negative results have not been the results of a lack of effort. The MIT team has been, as I am sure is the case with other teams, laboring around the clock. However, we and the other teams have been handicapped by a lack of enough scientific detail to guarantee that we are actually duplicating these experiments.

In the scientific community the soundness of experimental or theoretical research results is evaluated through peer review and duplication. For results such as those reported, whose potential impact on the scientific community and the world are so great, this review process is absolutely essential. Unfortunately, for reasons that are not clear to me, this has not happened in this case - at least so far. The level of detail concerning the experimental procedures, conditions and results necessary for verification of the Pons and Fleischmann results have not been forthcoming. At the same time, almost daily articles in the press, often in conflict with the facts, have raised the public expectations, possibly for naught, that our energy problem has been "solved". We have heard the phrase "too cheap to meter" applied to other forms of electric energy production before. And so the scientific community has been left to attempt to reproduce and verify a potentially major scientific breakthrough while getting its experimental details from the *Wall Street Journal* and other news publications.

Experiments conducted in haste and based on insufficient detail coupled with premature release of results have often resulted in retractions and embarrassment on the part of the scientific community - caught in the heat of the moment. I guess we are all human.

The result of this unsatisfactory situation has been that a healthy skepticism and, in some cases, distrust of the reported results has developed. We at MIT share this skepticism.

At the risk of becoming too technical in my comments, I feel that I must be a bit more specific with regard to the source of this skepticism. As I mentioned earlier the major results, reported by the University of Utah group are that there has been a generation of excess heat and the measurement of neutron radiation. By excess heat I mean that there has been a measurement of more energy produced than has been supplied to the system. From our standpoint the key, from a verification point of view, is the detection of neutron radiation. From an engineering point of view, however, the importance of excess heat production is critical. On these two critical points we have found that the results reported in the few available published documents from the University of Utah are inconclusive or unclear. For example, with respect to the detection of neutrons, critical products of the fusion reaction, the reported results are confusing. They either do not agree with or are not presented completely enough to show that they are consistent with what one would expect from the emission of neutrons from the deuterium fusion reaction. Specifically, the reported γ-ray spectrum produced by neutron emission does not exhibit a shape and intensity that demonstrates an increase in the number of detected neutrons above normal background. Further, the reported rate of neutron emission and level of tritium production are consistent with natural background. The results have nevertheless been reported as "significant". Those inconsistencies can only be resolved by a full disclosure of the details of the experimental measurements for examination by the scientific community. Until such time as this occurs we feel that the data is insufficient to demonstrate the presence of neutrons.

As far as the issue of excess energy is concerned we are also faced with a confusing situation. While the presence of excess energy is documented in the *Journal of Analytical Electrochemistry* paper, the method by which this excess energy was determined is not clear. With metals, such as palladium, which act as hydrogen storage media and at the same time as catalysts for many chemical reactions, both situations which can result in discontinuous chemical energy releases, it is critical that a total energy balance over time be done. To us it is not clear that this has been the case. Until this issue is clarified we are unable to make a judgement concerning the excess energy issue.

In conclusion I feel that it is safe to say that the scientific community is (1) excited about the possibility of a significant advance in the area of fusion energy research, (2) but is, at the same time, skeptical of results that have not been verified to this point and (3) is very frustrated at the methods by which the discovery has been handled both in the scientific and non-scientific community.

Thank you.

# PROFESSOR RONALD GEORGE BALLINGER

Professor Ballinger is an Associate Professor at the Massachusetts Institute of Technology with a joint appointment in the Departments of Nuclear Engineering and Materials Science & Engineering. Professor Ballinger's areas of specialization are as follows: (1) Environmental effects on material behavior, (2) Physical metallurgical and electrochemical aspects of environmentally assisted cracking in aqueous systems, (3) Stress corrosion cracking and hydrogen embrittlement in Light Water Reactor systems, (4) The effect of radiation on aqueous chemistry and stress corrosion cracking, (5) Experimental fracture mechanics techniques and analytical methodology, and (6) Materials development for cryogenic applications. Professor Ballinger is the author of several papers in the above areas and is a member of several professional societies including the National Association of Corrosion Engineers, The American Society for Metals, The Electrochemical Society, The American Nuclear Society, and the American Society for Testing and Materials. Professor Ballinger is a member of the International Cyclic Crack Growth Review Group and the International Cooperative Working Group in Irradiation Assisted Stress Corrosion Cracking.

DISCUSSION

Mrs. LLOYD. Thank you very much, Dr. Ballinger, as well as Dr. Furth.

You're researchers as well as members of faculties. What is the attitude among the nuclear engineering and the physics students towards these new developments at your institutions?

Dr. BALLINGER. I can tell you that within two hours of the reported results from Utah, a number of graduate students, primarily from the chemistry department at MIT, were high-tailing it over to the plasma fusion center to find a place to do these experiments. Those folks have been working 24 hours a day, seven days a week, for the last month. So the excitement on the part of the graduate students is extreme. That's an understatement, also.

I should say also that the excitement on the part of the faculty is also very much there.

Mrs. LLOYD. Of course, the main comment that you made, there has been no indication to indicate a deuterium fusion reaction. It's my understanding that most of this work has been done at relatively high energies and there's really no work to measure this rate at lower energies, at room temperature. So are there any characteristics of reaction at these low temperatures which might explain the fact that the measurements to date have not seen the large flux of neutrons? Could this be—

Dr. FURTH. Maybe I cold comment on that.

We are fortunate to have the experiment in new meson fusion, which is cold fusion certainly, which were mentioned by Dr. Jones a little while ago. That's about as cold as you can get. And in that case the deuterium-deuterium fusion reaction was perfectly normal, gave rise to exactly the product suspected, just as it does in so-called warm fusion. So it is not the coldness of the fusion that is changing the basic process of nuclear fusion here, if, indeed, it is being changed.

Mrs. LLOYD. Then the answer to my question is a big no.

Dr. FURTH. I'm sorry?

Mrs. LLOYD. The answer to my question is a big no, that this would not account for the fact that we have not seen the large fluxes of neutrons?

Dr. FURTH. That's right. What is being cited as the explanation is not the coldness but the presence of the palladium-titanium metal lattice, and the idea is that somehow that can interact with this d-d fusion process in such a way as to carry off the excess energy. But someone has compared this to setting off a hand grenade in a hayloft and expecting the hay to change the nature of the hand grenade explosion. People are having trouble seeing how this would work.

Mrs. LLOYD. At this point, would you characterize this then as a chemical reaction?

Dr. FURTH. I really can't tell what it is yet, because the experimental evidence that has been laid on the table simply is insufficient to be persuasive. My feeling, therefore, is I would like to urge vigorously both that more experiments be done and that more evidence, if it already exists, should be laid on the table so we can see it.

Mrs. LLOYD. What sort of peer review activities would you like to see?

Dr. FURTH. I think that one of the most productive activities at the moment is the collaboration of Utah U. with Los Alamos, and perhaps with other places, in the collaboration of Brigham Young with Los Alamos and perhaps other places, because one can get only so wise from reading articles, let alone from reading the daily press. The thing to do is to have the observers of these phenomena physically transport their palladium rods and their apparatuses into the environment of these large laboratories where really careful measurements can be made. I think that really would be a very effective way for the peers to get mixed in with the originators of these ideas.

A second point is I look forward very much to this meeting at the end of May in Santa Fe, where all the cold fusion optimists and skeptics will gather and thrash it out for an extended period. I think that should be very productive.

Mrs. LLOYD. But at this point, you characterize your response as wait and see?

Dr. FURTH. Yes. I would say, since I haven't seen anything that is truly persuasive, that truly proves the point, therefore, my attitude is to wait and see.

Mrs. LLOYD. Dr. Ballinger, does this characterize your philosophy at this point?

Dr. BALLINGER. Yes.

Mrs. LLOYD. Does the Stanford experiment meet the plain water or the heavy water test, Dr. Furth?

Dr. FURTH. We don't know yet. It again is a matter of seeing the particulars, and I wish I could fly off to San Diego to be there tonight and hear about it. But there will be many good people who will scrutinize what Dr. Huggins has to say and we'll see.

I have so far, in response to my mentioning this control experiment with light water in Dallas, have gotten at least three different inputs; namely, Professor Huggins sees heavy water heating and not light water. There's another professor from Drexel who sees light water heating but not heavy water, and there's a lot of people who don't see either one producing more heat than it should. So I'm in a state of confusion.

As I mentioned earlier, I sort of have the feeling, when I hear Dr. Fleischmann say that he'd rather not talk about light water fusion, that there is something there that he's not talking about, and I wouldn't be surprised if he's wiser than any of us about what happens and look forward to some time when his patent attorneys or whoever will let him talk about it.

Mrs. LLOYD. Thank you very much.

Mr. Schiff.

Mr. SCHIFF. Thank you, Madam Chair.

Gentlemen, I don't want to belabor a previous set of questions I asked, but again, I think you're aware we're between being advised to make a great investment into this type of research that's done at the University of Utah, and what I think you referred to, Dr. Ballinger, as a heavy skepticism. The problem for us is which way to go. We're advised, on the one hand, that we should move immediately before other nations take advantage of the commercial and scientific applicability of this process, and we're advised that going too far too fast in one direction could backfire on us.

So I would be grateful simply for your opinion. If you were us, what would you do now?

Dr. FURTH. Do you want to speak first?

Dr. BALLINGER. I think if I were you, I would try to find a way to provide the kind of funding that people that have very innovative ideas—and I think this is one of them and must, so far at least, can do something quickly to either prove or disprove a particular theory, but that that freedom should only go so far. There needs to be technical, scientific verification of the results before you make a major commitment of funds.

I think that once you get verification, that there is a very serious problem in this country about the way we get things, in effect, to market. So I think that a lot of the points made by earlier speakers are very, very valid.

I, for one, have not had a problem in getting the amount of funds that I need to do what one would consider to be "offbeat" or wild type of experiments, so I haven't had the experience that other people have. But I've certainly heard people talk about it.

Dr. FURTH. If I could comment, also, I very much sympathize with the idea that you don't want the situation where something good has been discovered scientifically and it runs on for months and years on end, as one polishes up the scientific theory, and one lets technological opportunity slip by. So certainly, I totally agree those should be telescoped.

But I think, before you launch on anything ambitious, you really should know whether it's for real or not. And in this case, you don't know it. I don't know it; maybe nobody knows it. This could lead to a very severe embarrassment, and from seen from a scientific point of view, the scientific community is not thrilled at contemplating that, and I would think the Congress would not, either. So I think prudence dictates that if you wish to accelerate the process, what you would do is really turn on the screws on this verification business, and whatever money is lacking, I'm sure it can be found to get on with it.

It isn't even very expensive, nor need it take very long. As I mentioned in my testimony, things you could do would only take a week. So I think there is where the pressure belongs. The business of making big commitments to things, to projects which are based on phenomena which we don't know yet to be real, I think that isn't speediness; that is haste.

Mr. SCHIFF. Of course, the testimony of the second panel was that in other countries they're willing to take that gamble, and I know if I sound like I'm contradicting myself, it's because we're hearing up here contradictory advice.

The consultant to the University of Utah has stated, or at least implied, that in Japan, at least, after reading a newspaper article, they were all set to invest a significant amount of assets into this research now.

Dr. FURTH. Yes.

Mr. SCHIFF. Do you believe that is, in fact, occurring, if you know, Dr. Furth?

Dr. FURTH. I'm sure your means of discovering this are better than mine. I have no idea. And I'm sure your judgment as to whether that's wise or not, and whether we should imitate it, is also very good. And so me just rest with my description of how I see the fact.

Mr. SCHIFF. I can only hope that we politicians ultimately come to the right conclusion for this scientific community.

Thank you, Madam Chair.

Mrs. LLOYD. Thank you very much, Mr. Schiff.

I want to thank the witnesses for being here, the participants. This has been a long hearing, but it's been a good hearing.

I would also like to thank the staff for the excellent manner in which they put our hearing together. To all of you, I express my appreciation.

If there's no further comments, the committee stands adjourned.

[Whereupon, at 3:40 p.m., the committee adjourned.]

# APPENDIX

**OPENING STATEMENT**
**BY U.S. REP. JERRY COSTELLO**
**COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY**
**APRIL 26, 1989**
**"COLD FUSION"**

Many of us were surprised at the recent announcement by Doctors Pons and Fleischmann that they had discovered "cold fusion" in their laboratory at the University of Utah.

I know that many of my constituents have expressed both confusion and amazement at this development, still unsure of its implications or what it actually means for science and scientific development. I was pleased that the subcommittee on Energy, Reserch and Development recently redirected $5 million into the Basic Energy Sciences program to study cold fusion.

Certainly, the importance of such a discovery if confirmed would have great implications for energy resources in the United States. I look forward to hearing the testimony knowing that we could be on the brink of a new development in scientific technology.

Lawrence Berkeley Laboratory
University of California Berkeley, California 94720
Applied Science Division (415) 486-5001 • FTS 451-5001

April 24, 1989

Robert Liimantainen
Energy R&D Subcommittee
Rayburn House Office Building
Room B374
Washington, D.C. 20515

Dear Bob:

Please find enclosed a copy of my written testimony for the hearing on cold fusion, to be held on April 26, 1989. If you need any additional information, please let me know.

I hope we can get together soon, either here in Berkeley, or in Washington. Best wishes.

Sincerely yours,

Elton

Elton J. Cairns
Director, Applied Science Division

# ELECTROCHEMICALLY-INDUCED COLD FUSION

Testimony Prepared for the House Committee
on Science, Technology and Space

Elton J. Cairns
Lawrence Berkeley Laboratory, and
University of California
Berkeley, California, 94720

April 26, 1989

## Summary

Recent claims of electrochemically-induced fusion and the production of large amounts of excess heat in a simple palladium/heavy water/platinum cell have triggered world-wide efforts to confirm the claims. If either claim is confirmed, new approaches to fusion and/or energy production will be possible. This could change the energy R&D plans for the United States. The essential next step is confirmation (or refutation) of the reports. This may require several months of effort.

Thank you for the opportunity to provide written testimony on the subject of electrochemically-induced cold fusion. I am Elton J. Cairns, Director of the Applied Science Division of Lawrence Berkeley Laboratory (LBL), Professor of Chemical Engineering at the University of California at Berkeley, and President-elect of the Electrochemical Society. This testimony represents my own views, and does not necessarily represent those of LBL, the University of California, or the Electrochemical Society.

Since the announcement of electrochemically-induced cold fusion by Pons and Fleischmann (at the University of Utah) on March 23, 1989, the scientific community has been attempting to learn more about the experiments behind this remarkable claim, and the related work by Jones and coworkers (at Brigham Young University.) Both groups have used very simple electrochemical cells comprised of a palladium or titanium electrode, a platinum electrode, and a heavy-water electrolyte ($D_2O$). Palladium and titanium have a special capacity for absorbing large amounts of deuterium, which is produced electrochemically in the cell from the $D_2O$. The fusion reaction is said to occur in the deuterium-containing palladium or titanium electrode. Preprints of the Fleischmann and Pons paper (*J. Electroanal. Chem.* 261, 301 (1989)), and the Jones *et al* paper (submitted to *Nature*) have been made available to the scientific community. Basically there are two types of claims:

1. Evidence of nuclear fusion reactions

$$d + d \rightarrow {}^{3}He + n$$

$$d + d \rightarrow t + p$$

   Both of the above groups claim to see evidence of nuclear fusion reactions through detection of neutrons and/or tritium. The rate of formation of the products of these reactions is very low (in the range of one pair of deuterons per second.)

2. Evidence of large amounts of "excess" heat, which cannot be accounted for by any known chemistry (nuclear or otherwise.) This observation was reported by the Fleischmann-Pons group, but not by the Jones group. The rate of heat release was reported to be three and more times the rate of energy input to the cell, and at least a million times the rate corresponding to the neutron production.

Many laboratories around the world have been seeking to confirm the two results listed above. Special sessions have been held at scientific meetings to learn more about the work and to discuss it. Some groups have reported confirmation of each of the above claims, but no details are available yet. Some of the earlier confirmations have already been retracted. All of this leads to great confusion and uncertainty about the scientific validity and reproducibility of the results. Essentially all of the National Laboratories are attempting to confirm the experiments, but no confirmation has been announced. Experiments are underway at LBL, but no confirmation has been achieved yet. Part of our work at LBL is devoted to a careful energy balance, related to the second claim listed above.

If either or both of the claims are conclusively verified, this would indicate a very important scientific achievement. If only the first claim is verified, this would constitute evidence for nuclear fusion in a crystal lattice under very mild conditions, compared to those used in the fusion research program. This fusion in a crystal lattice would open up a new approach to fusion research which could prove to be very important. There would be no immediate practical benefit from this result, however.

If the second claim (large amounts of excess heat) is verified, then there could be much more near-term benefit. One would expect that inexpensive heat could be produced for many low-temperature applications, such as heating buildings, providing hot water, etc. If the excess heat could be produced at higher temperatures (as a result of additional research), then many more applications could prove feasible, such as the production of inexpensive electrical energy, and high-grade thermal energy for industrial processes. Since palladium is a precious metal (much like platinum), it probably is not feasible to rely on the use of palladium in large amounts. Therefore, research would be needed on the use of other deuterium-absorbing metals, such as titanium, vanadium, iron, niobium, and others.

With all of the claims, activity, and confusion, what should be done next? There should be careful, well-planned, complete experiments that have as their objective the confirmation of the two effects already claimed: (*i*) the fusion of deuterium in palladium to produce neutrons and/or tritium, and (*ii*) the production of significant amounts of excess heat (over and above the energy input and over and above the rate of neutron or tritium production). The planning and execution of such experiments are in progress. Once the claims have been verified, or properly refuted, then plans can be made to follow up on whatever positive results may emerge.

It is still too early to have any idea of what the outcome will be. We should not alter the funding of existing energy R&D efforts yet. We <u>should</u> place a high priority on high-quality confirmation efforts. These efforts could require from one to several more months. If confirmation of deuterium fusion in palladium is achieved, we have a new approach to fusion research. If confirmation of the excess thermal energy is achieved, we have a new area of energy R&D to pursue, with possible near-term benefits. Either one of these is an exciting development, worthy of an appropriate federally-funded program.

Thank you for the opportunity to submit my views to the Committee.

COMMITTEE ON SCIENCE, SPACE AND TECHNOLOGY
U.S. House of Representatives
April 26, 1989

Hearing on Recent Developments in Fusion Energy Research

Testimony for the Record
by

Texas A&M University/Texas Engineering Experiment Station

We appreciate the opportunity to submit written testimony for the record and commend Chairman Roe for the timeliness of this hearing.

Texas A&M University researchers have been actively pursuing investigations of anomalous heat generation by electrochemical decomposition of heavy water with palladium cathodes. Two research groups, using different calorimetric techniques, have shown that when heavy water is electrolyzed using a palladium cathode and a platinum anode, excess heat is generated. The rate of excess heat generation reached 15 $W/cm^3$, which is comparable to the value reported by Fleischman and Pons. Texas A&M University has also carried out the crucial experiments to confirm that there is no excess heat generation with palladium in normal water nor with platinum in heavy water. Experiments performed with a unique precision microcalorimeter, which were continuously recorded on chart paper, showed for the first time, that palladium and heavy water are crucial for producing the excess heat. Experiments are in progress to determine the critical parameters that govern the excess heat generation.

While we are yet at the threshold of recent scientific developments, it is important to keep in mind that once the press leaves the laboratories, the scientists will still be there doing research. In order for experimental data, such as we have now, to reach a stage when it can be called a technology; and then be able to apply that technology to practical energy use, a sustained commitment is necessary.

Researchers at Texas A&M University/Texas Engineering Experiment Station include: A.J. Appleby, Y.J. Kim, O.J. Murphy and S. Srinivasan (Center for Electrochemical Systems and Hydrogen Research); C.R. Martin and J.O'M. Bockris (Department of Chemistry); and B. Gammon and K. Marsh (Thermodynamics Center).

Again, Mr. Chairman, we thank you for this opportunity and should you wish to submit written questions for the record, we would be happy to answer them.

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