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PRESS CONFERENCE

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THE AMERICAN PHYSICAL SOCIETY

AMERICAN PHYSICAL SOCIETY STUDY GROUP ON RADIONUCLIDE RELEASE FROM SEVERE ACCIDENTS AT NUCLEAR POWER PLANTS

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Room 1167
Nuclear Regulatory Commission
1717 H Street, N.W.
Washington, D.C.

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The press conference convened at 11:45 a.m.

PARTICIPANTS:

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Chairman
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Study Group on Radionuclide
Release from Severe Accidents
at Nuclear Power Plants

WILLIAM HAVENS
Secretary
American Nuclear Society

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P R O C E E D I N G S

MR. HAVENS: I apologize for the confusion for this press conference. I hope this microphone is operable -- I can't tell -- is it? All right, then we will have to speak in a lecturing voice.

THE PRESS: Will you talk into it anyway?

MR. HAVENS: It will sit there, I am not going to be bothered by it.

I am Bill Havens of the American Physical Society. The American Physical Society thought this press conference was being set up by the Nuclear Regulatory Commission staff. The Nuclear Regulatory Commission staff thought it was being set up by the American Physical Society, and consequently nobody did adequate preparation to set it up. I found out on Tuesday night that the NRC staff had expected the American Physical Society to do it, and it was rather late then to take into account the things that had to be done.

So, please, accept my apologies for the unfortunate physical arrangements we have here.

A brief history of this report is that about two years ago, the Nuclear Regulatory staff asked the American Physical Society to undertake a study of the "source term," mainly to establish validity of the scientific base in the methods of calculation of some of the factors which were important in the "source term" used in serious accidents in

nuclear reactor plants.

We assembled a group of these phycisists, as Dick Wilson said to the Commission in the previous meeting. I called him up in Geneva in the summer of 1983 now to ask him to be chairman of this group and he did -- I will confirm -- he did say, "Yes, but I'm sorry you made this telephone call."

He did agree to be the chairman of the group, and we immediately began assembling a group to study this particular report. There were a large number of people suggested as members of the study group. We have a selection process which has been published as to how this takes place, and the study group was assembled in September of 1983 when it held its first meeting at the Joseph Henrie Building of the National Academy of Sciences. The group has been working hard ever since.

Now, since Dick Wilson has been the one that has been closest to the operation, I think it's best for him to take over at this time.

MR. WILSON: Firstly, I would like to tell you who of the group is actually here in case you want to ask them some questions as well of, or instead of me.

Firstly, Dr. Araj, who has been the Executive Director of the group who has been full-time working on this and making sure I do the things I have to do at the right time, and trying to make sure all the things happen properly.

Dr. Allen, who is Chairman of the Chemistry Department at Brookhaven National Laboratories here; Peter Auer is not; David Boulware is not. Fred Finleyson from Aerospace Corporation is here; Professor Simon Goren of the Department of Chemical Engineering in Berkeley is here; Dr. Clark Ice, Professor Leon Lidofsky, Dr. Mary Shoaf, and Dr. Irving Spiewak. In addition, we have had a special consultant throughout much of our study, Dr. Robert Budnitz, and he was a member of the study partially because at the time much of this work started which we were reviewing, he was Director of Reactor Safety Research and was commissioning part of the research. So, he was advising us on some of the background, and of course he was also trying to find out for himself what he had done wrong.

(Laughter)

MR. WILSON: So, I don't know how many of you heard the presentation of the Commission on the report, but let me just summarize in a few brief words what we conclude. It's always hard to get a group of twelve physicists to conclude anything, but we generally concluded there are a significant number of -- pardon me, let me go back.

We have accepted what is now the general approach for looking at reactor safety. That is an approach which was brought up in Rasmusson's report in 1975, that if you try to imagine what can go wrong in a scenario of events and then try and follow that clue and first evaluate how probable it

might be and second, a group of such events can be and, secondly, you evaluate what the final consequences are.

We have been addressing the second of those, evaluating what the final consequences are in some of those accidents.

Now, in evaluating them, we have taken the group of sequences which other people have evaluated. We have looked at that just a little bit. We think they chose a sensible set to look at, and we find in a number of those sequences that was thought at one time to be quite a severe accident was much less severe, was thought to be less severe now than it used to be because of, finally, the containment vessel which surrounds the reactor is less likely to fail than we thought it was at the time of Rasmusson's report. That leaves time for all sorts of things to happen such as deposition of aerosols on the -- surfaces in the reactor so they will not be released in the environment if and when a containment finally breaks.

On the other hand, again it's very hard to get a group of physicists to make any sweeping generalization even when they say there is conservation of energy then someone comes out who say its a continued creation theory of the universe and if you look over a time scale of terms of nine years, even that doesn't work.

So, likewise, we are unwilling to say that in all

circumstances the source terms, the amount of radioactivity, will be less than what was predicted before in the reactor safety study, and in fact there is some data which suggests under some circumstances it might be slightly more. There is some more research still in progress to understand that latter question and we believe that research should continue, and we believe that the funding should be available to continue both here and in the rest of the world.

Now, we did not address on those particular circumstances how probable those things might be, we just addressed the consequences themselves. Now, I leave it open to ask any questions.

MR. HAVENS: We will have questions from the audience. Yes?

QUESTION: If the source term doesn't get out of the plant, how does that affect the environmental qualification of equipment in the plant that would be used to mitigate an accident; have you looked at that?

MR. WILSON: We didn't look at it, but it's a fairly general point that whereas if you inhale radioactivity and get radioactive iodine in your thyroid, it can upset what you are doing considerably. Radioactive iodine deposited on the casing of an electric motor does not stop it working.

MR. HAVENS: All right, is there another question?

QUESTION: How much of the analysis is based on

experiments or events like Three Mile Island, and how much of this modeling is empirical?

MR. WILSON: Well, I think the first thing, Three Mile Island had a very important indirect effect on the whole process of understanding the source terms. But in detailed analysis, it has no effect at all. And let me explain that.

Firstly, at the time of Three Mile Island, the methodology of studying reactor safety, which goes back to Rasmusson, was not generally accepted and used. That was a tragic error because if it had been used, the differences of Three Mile Island and the Rasmusson study would have been understood and perhaps that accident could have been avoided.

So, that was feature number one that happened as a result of Three Mile Island, people suddenly realized that they had not been doing the job as well as they should have done.

Secondly, at Three Mile Island, that accident -- although it was most severe in some ways it has taken place -- very little radioactive iodine was released.

Now, the reason why radioactive iodine is dangerous is because when you breathe it in, it tends to stay in the body; whereas if radioactive krypton and xenon, when you breathe them in, you breathe them out again.

At the same time it was found out that the radioactive iodine has a tendency to stay on surfaces and so on.

So, when it was noticed that not much radioactive iodine was released and it was noticed by all sorts of people simultaneously, the question is, how general is that conclusion? Would that apply to all sorts of other reactor accidents as well?

And that started the line of research which leads to our present review here today. If an accident happened fairly similar to Three Mile Island when there is lots of water still around and it is terminated, there is no question -- I don't think the committee discussed this -- but there is not much doubt that the radioactive iodine will not be released just as it wasn't at Three Mile Island.

However, we are talking here about accidents which would progress to much more severe circumstances in Three Mile Island -- the hypothetical in the sense that none haven taken place, I hope it will remain hypothetical because other people's calculations which we didn't review suggest they will take place, if at all, only very rarely. They might probably never take place in the next hundred-thousand years. So, they are by definition more severe than happened at Three Mile Island.

And then the experience what happened at Three Mile Island doesn't directly apply to what we are discussing.

QUESTION: Speaking just as an individual, I wanted you to respond to the question that Chairman Palladino asked,

I guess. On the basis of the information you have seen, do you think the size of the evacuation zones should or can be changed at this point?

MR. WILSON: Well, I would like to -- the emergency planning criteria of the NRC are based on a very large number of issues. There have been various members of the committee who have been involved with that in various ways.

I would like to stress in answer to that, I think almost everybody in the committee would like to stress emergency planning -- and you mention evacuation zones.

I think most members of the committee would agree -- though I haven't really polled them -- that that stress on evacuation zone is a stress on the wrong thing. The stress we would depend on -- and I personally would not go beyond what was said to the committee, a report which I wrote for the Governor or Massachusetts in 1980, if an accident occurs, the most important emergency planning criteria is to have someone prepared and able promptly to measure if and when any radioactive iodine is released and we would now add any radioactive lanthanum is released.

And if people are not prepared to do that, they are not prepared to decide whether or not an evacuation should take place. So, my personal conviction is that evacuation is easy. I have seen two million people leave one square-mile area in the City of London within one and-a-half hours, and

that is done every day.

QUESTION: How? What's the occasion?

MR. WILSON: You mostly walk to the nearest railroad station and catch -- there are a number of commuter trains which are going out of there. The City of London has two million people working in it, and a hundred-thousand people spend the night there.

QUESTION: Have you ever ridden the Long Island Expressway?

(Laughter)

MR. WILSON: Yes. I know the Long Island Expressway is the longest parking lot in the world, but also having been there after a summer holiday on a Sunday, it's almost stationary. But remember, go there at 2 o'clock on Monday morning and the traffic is gone.

QUESTION: What was that second element that you mentioned? You mentioned the two elements, two releases. You mentioned radioactive iodine, and what was the second one?

MR. WILSON: Well, lanthanum -- one particular element that's worth thinking about.

QUESTION: How do you spell that?

MR. WILSON: L-a-n-t-h-a-n-u-m.

MR. HAVENS: There is a question.

QUESTION: Dr. Wilson, I believe you told the NRC

that you thought that those source terms that you thought could be lowered or should be lowered were in the most probable accident sequences, but you also said the committee die focus on probabilities.

MR. WILSON: Correct.

QUESTION: Could you specify the differences?

MR. WILSON: Well, there are people who as a group have done a lot of study and a large amount of emphasis in the last ten years since publication of the Reactor Safety Study, the Rasmusson, has been on calculating going through these various sequences, evaluating how probable the calculations are, how good these calculations are; how one can improve reactors to make the probabilities smaller -- which is very important, and there have been particular studies on this question.

The numbers of those studies, outputs of those studies, are available to us. We did not discuss the output of those studies and do not want to say we believe in any particular numbers stated by them.

But, if we take the most probable sequences, for example, what these people say are the most probable things to happen, we now look at the consequences, those are the sequences. Some of those sequences are those for which we think the consequences for release of radioactive iodine in particular are going to be less than was previously assumed.

Now, we are not saying that those calculations are right. On the other hanc, if you'll push me to the wall and say, could they be really way off, we wouldn't say so either. They think they are quite reasonable re-evaluations, though we are not either endorsing them nor saying they are wrong.

QUESTION: Well, the bottom line is, do you think they are safer then the study said ten years ago, the plants are safer?

MR. WILSON: I don't think we particularly want to ask that question. But when Joseph Hendrie was Chairman of the Commission he did say that when he looked in the mirror in the morning when he was shaving and failed to go to the telephone and order the shut-down of every nuclear power plant in the country, he was publicly declaring nuclear power plants are safe.

I think no member -- I have not sent a telegram to the Commission to ask them to shut down nuclear power plants or to propose any licensing. I don't think any member of the study group has either.

QUESTION: At the same time, is the bottom line that you don't think that the current regulations -- there is not enough research available at the moment to support changing the current regulations?

MR. WILSON: No, we don't say that either. We explicitly don't say that. We take no position on supporting

it because the Commission have a much bigger job in front of them than we address. The things which they consider when addressing the regulations are the fact they've got a nuclear power plant out there working, or a nuclear power plant out there ready to work and someone screaming that if it doesn't work, they will be wasting billions of dollars.

Those are issues which we did not address. You can easily say, "Look, you can always wait to know the answers." Well, you may wait forever. We don't address that question. So, it could well be -- we say that we believe that some more work could be done on the source term to make the answers more reliable. The source term is one input into those questions.

We also made the statement to the Commission, "Don't expect the source terms to become more reliable within three months, that's not the time scale we are talking about." There is talk about a time scale of two years to five years when our report is going to be out. It will be published in News Among Physics at the end of this year. It will be read by physicists all over the world --

It will be digested, feedback will come over a period of two to five years. That is the time when more detailed information is available. If the Nuclear Regulatory Commission feel they have to re-look at the regulations in a shorter time frame than that, well, they have to do so and

to use whatever data is available to them, including what we have in this report.

QUESTION: Well, if you were on the Commission, would you want those peer reviews completed in five years?

MR. HAVENS: Please, let somebody else have a question. Right here?

QUESTION: Where, at this point, are the more critical unknowns in terms of possible major variances in source term calculation?

MR. WILSON: Well, I think there are two primary things. If you imagine a very severe accident, somewhat toward the end of that accident progression you imagine a molten core, melting through the reactor pressure vessel and falling to the concrete floor.

It's quite obvious, the farther you get down the chain of events in the reactor, the more uncertain everything gets. And in this particular thing that is the part of the accident sequences which have been undertood least.

Now, we believe that it is possible to understand it more than it is now understandable. It is certainly true that in the world there are a lot of things you will never understand, and we are not just recommending research for research's sake. I mean, the standard recommendation of any group of scientists, "Give us another $500 million and we will find some answers for you."

We are trying to be a little more specific than that. We say, "Some answers are available, we believe, in a finite period of -- the work will be over the two years; the understanding of it will be over four, five years on those questions and they would very much improve our knowledge."

And, of course, they might tell us that in some of the operation and design features we are not doing the best possible thing.

QUESTION: You said there were two things.

MR. WILSON: Oh, the other feature is that a lot of the work is done on computed codes and computer calculations. Those computer codes have taken a long time to develop. Some of them in our view -- I mean, most of us on the study group, several of us were professors, we have graduate students. We have the task of understanding the computer codes our graduate students have written without actually having to go into detail. And we demand that our graduate students give us a copy of the output.

We demand we want a test program. We demand all sorts of different features -- that process has only just begun -- to really make sure that all the calculations are doing what they think they are.

For example, one of the crucial computer codes only became generally available in mid-November and we just think they have to be exercised and looked at by lots of people to

make sure, for example, that everybody understands what he is doing. It may well be that someone is using the computer code in the wrong way and coming out with an answer which is inappropriate. You have to have a little bit of time follow from that.

QUESTION: Could you describe, Professor Wilson, for us some of the types of research that you have in mind, do they involve tests of the containment vessel, for example?

MR. WILSON: Well, I think we didn't discuss in detail tests of containment vessel. We believe those were in progress. We had the report from people who test containment vessels and we believed that should be looked at by the Commission on an on-going basis to be sure that every containment vessel comes up to what the calculations are. There have been one or two tests to destruction of one or two typical containment vessels.

Of course, what an individual containment vessel at Indian Point or Pilgrim Nuclear Power Plant in Massachusetts -- which is one I am particularly interested in because it's about twenty miles away -- how that is strong, that of course has to be carefully gone into.

And we are aware that there are quality assurance programs but we haven't discussed them.

The tests we are particularly concerned with on the core concrete interaction which are tests which are still

in progress both in Karlsruhe, in Germany and in Sandia down in Albuquerque, in which they try and simulate the molten core falling onto the concrete and see what happens.

The trouble is, the simulations are rather hard. Some of the crucial things about them haven't yet been measured and at the moment there is not complete agreement between the German results and the Sandia results though there have been explanations, possible explanations for this, and we think that should be just carried through to a more complete conclusion.

QUESTION: Well, I can understand, where, you know, realizing that iodine is not a stable gas, it seems to me that the crucial difference -- is what happens to the containment itself. Is it going to remain intact?

MR. WILSON: That is the crucial difference that everybody has called attention to.

QUESTION: And one of the big unknowns, still, is the core-concrete interaction, what happens if the core hits the concrete.

MR. WILSON: Yes. But it would not matter what happens in the core-concrete interaction if the containment still stays intact for a long period because what is released in the core-concrete interaction, something is released even though it may not be radioactive, will firstly be released into the containment vessel, and if that remains intact for a long

period, that gives time for deposition of all the aerosols which are produced and they will deposit out on all the surfaces of the reactor. And that's one of the crucial things of that time.

Now, we are not sure, the computer calculations are still -- are the things that tell us what the detailed amount of deposition and the detailed time scale. We are not saying we completely trust the detailed numbers on that yet, but we do know if containment lasts for an order of, say, ten hours, that is a very important improvement in reactor safety.

MR. HAVENS: Yes?

QUESTION: Does that suggest that smaller containments might not be as safe as larger containments, PWRs or ice condensers?

MR. WILSON: The easiest to analyze and understand are the pressurized water reactors with large dry containments because it is a static understanding.

We are a little worried about the PWRs with ice condenser containments. We are not saying they are wrong, we are saying they are a little harder to understand because some of the processes of the ice condenser have not been completely understood yet.

As far as the stress on the containment, that depends on, is the steam taken out. And we sort of believe that happens

over an adequate time scale. We are not completely -- we haven't looked at the question of the hydrogen will not be taken out, there might be an explosive atmosphere inside -- I say it's not taken out by the ice condenser, it has to be taken out by other devices of which there are other devices whose efficacy has to be looked at, which was not a question for us to address and we didn't address it carefully.

But the other issue is, it's quite conceivable that the pressurized water with ice condensers aren't better because the ice condenser might act to reduce -- in itself be a filter for some of these fission products.

Unfortunately, there have been no tests on that on which we can test this assumption, and that is a question. One of our recommendations is that tests of that sort proceed because that could make one believe that ice condenser containments, they might turn out to be even better than the others, we don't know.

QUESTION: One more thing on core-concrete inter-reaction. Is there another answer on that than just the China Syndrome?

MR. WILSON: The China Syndrome, remember, is the melting of the core through the concrete base mat. I think at the moment that's a question slightly in dispute. But it depends what you mean by the China Syndrome.

Our new friends in China, at the time it was first

invented they weren't our friends. We can be reassured that at no time will the core -- everyone agrees the core will not go that far down and that has been agreed for a long time.

So, the question, will it go through the base, the concrete base mat and get out into the earth below it, that is still a matter of disagreement. Some people think it never can, some people, it will.

There is one very important thing. The China Syndrome has always been thought of, particularly in view of a particular Hollywood movie, as being the worst that can happen. I think a lot of people believe -- let me in fact make a statement. It has comparatively modest effects on immediate public health.

That is to say because if the material goes into the earth which is an excellent filter for airborne radioactivity which is the air surrounding it. We are much more worried about the possibility of the containment breaking, spreading into the air, rather than the core melting into the ground and then having an excellent filter from there to the environment.

QUESTION: I thought the ANS source term study had basically ruled out certain types of catastrophic explosions as major initial rupturing agents for the containment vessel. Does your study also take that attitude?

MR. WILSON: Well, there are two types of catastrophic

things for rupture that have been suggested. One is a possible hydrogen explosion, hydrogen which comes from the zirconium. We all know that hydrogen was produced in Three Mile Island, and that came as a result of the hot steam interacting with zirconium in the fuel cladding. The zirconium grabbing the oxygen and leaving the hydrogen free.

Now, that hydrogen then goes into the containment vessel and can ignite, as it did about 1:30 on March 28, 1979, and produce a pressure spike.

The question is, could that be dangerous enough, big enough to break the reactor vessel. In dry containment, I think it is generally accepted it will not because there is enough steam around that would inhibit the exposure -- although it might give a burn like happened at Three Mile Island, it will not be complete enough to stress the containment.

For the ice condenser containments it's a question of worry, and if there was no mitigation effect to take out the hydrogen, that would leave an explosive atmosphere, then, inside the containment vessel after such an accident.

However, there are such devices to take away the hydrogen. We didn't evaluate their effectiveness but merely accept it and so we don't make any particular statement on that particular matter. We don't say the American Nuclear Society is wrong, we just say we haven't looked at that

particular point.

MR. HAVENS: Yes?

QUESTION: How do your conclusions compare to the Stone & Webster report of a year or so ago? It's about the best engineering firm and it came out with a study saying that the threat of contamination from a nuclear accident wasn't as great as everybody thought it was.

MR. WILSON: Well, it's always easy to say the threat is not as great as everybody thought it was and briefly define "everybody" first.

(Laughter)

MR. WILSON: There are a very large number of people who thought at the time the Rasmusson's report came out, there were two groups of people, one who thought reactors were safe and the other who thought it was a fraud.

There were one or two people in the middle saying, "It emphasizes a very nice methodology for discussing reactor safety and making them safe, and operating them safely and making them safer."

I think I and most of the committee belong to that third group which wasn't very commonly introduced. So, we don't normally discuss reactors are safer than we thought they were. We do have the statement in there that for some of the sequences which are discussed by Rasmusson there was less radionuclide release than Rasmusson said, and we say that

unequivocally in the statement.

We do not go into detail on the probability of those sequences. I think, though, we do refer to the people who want to calculate such probabilities, we have repudiated them. And of course, the total safety is a product of the probability in some sense it's a probability in the consequences.

QUESTION: Do the conclusions of your report have any implications for changes in the regulations by the Commission?

MR. WILSON: Indirectly, yes. And of course, it's for them to decide exactly what because their regulations are based on all sorts of things.

If I was to give you a report on the number, the probability of severe automobile accidents of airplanes colliding with school buses in the State of Montana, that would clearly have a great impact on whether one should have speed limits on school buses.

However, it no one in setting the speed limit goes directly into those accident sequences in setting it since one of the inputs to what he does is by no means the only input and it should not be the only input.

For example, if we said -- most of the regulations, in fact, are not based on any of this methodology at all, they are based on much older discussions. Maybe they should be. That's up to the Commission to decide.

MR. HAVENS: Are there any further questions?

QUESTION: Yes, I have one. I wanted to ask that one question, if you were on the Commission, before you started looking at the regulations, would you rather wait to see for the three to five years so peer review can come in, and you publish this report, or would you feel confident, based on what you know about the source term research in the last five years to say we can start taking a look at those things now?

MR. WILSON: Well, when most people ask hypothetical questions like that, the hypothetical thing is flattering, if I was on the Commission. I just hope I'm never going to be on the Commission --

(Laughter)

MR. WILSON: They have a set of decisions to face which I just -- they have to cope with and they have to cope with a set of committees up on the Hill which I hope not to have to cope with. I'd just rather not address that particular question.

MR. HAVENS: Well, if there are no further questions, thank you very much. We stand adjourned.

(Whereupon, at 12:20 p.m., the press conference was closed.)