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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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RES SEMINAR:

WASH-1400 AND THE ORIGINS OF

PRA IN THE NUCLEAR INDUSTRY

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MONDAY,

NOVEMBER 9, 2015

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ROCKVILLE, MARYLAND

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The Seminar occurred in the Auditorium at the Nuclear Regulatory Commission, Two White Flint North, 11545 Rockville Pike, at 9:30 a.m., Stephen G. Burns, Chairman, presiding.

PRESENT:

MICHAEL WEBER, Director, Office of Nuclear Regulatory Research

ROBERT JAY BUDNITZ, Lawrence Berkeley National Laboratory, University of California

THOMAS WELLOCK, NRC Historian

ALSO PRESENT:

TERRY BROCK, Office of Nuclear Regulatory Research

JAMES CHEN, Office of Nuclear Regulatory Research

GENE EAGLE, Office of New Reactors

DEREK HALVERSON, Digital Instrumentation and Controls

BARBARA HAYES, Office of New Reactors

JOHN LANE, Office of Nuclear Regulatory Research

JEFF MITMAN, Office of Nuclear Reactor Regulation

PAUL REBSTOCK, Office of Nuclear Regulatory Research

NATHAN SIU, Office of Nuclear Regulatory Research

T-A-B-L-E O-F C-O-N-T-E-N-T-S

P-R-O-C-E-E-D-I-N-G-S

PARTICIPANT: Good morning and welcome to our seminar on the WASH-1400 study and the origins of PRA in the nuclear industry. Before we get started, there are a few housekeeping items we need to go over.

First, this session will be recorded, so we ask that you please turn off or silence your electronic devices and please minimize side conversations.

Please also be aware that the recorded contents of the session including any questions posted by the audience members will be preserved in accordance with NRC's record management program and moreover, subject to FOIA disclosures.

Please refrain from including any sensitive information, for example SUNSI, in any comments or questions. For folks on the webinar, please submit your questions and we will be reading them during the question and answer session.

I will now leave you with the Director of the Office of Nuclear Regulatory Research, Mike Weber. Thank you.

MR. WEBER: Well, good morning. It's great to see so many folks out here for our WASH-1400 seminar. Welcome to November. If you have not yet participated, you have a whole month of opportunities to practice active knowledge management as we work to accomplish the mission that the Congress set before us.

PRA, Probabilistic Risk Assessment. Everyone in this room should be familiar with what PRA is all about. Many of you deal with risk assessment as part of your everyday jobs, at least we hope so in a risk informed, performance based regulatory approach.

The NRC has a policy statement on the use of PRA. We have standards, we've got guidance, we've got methods, models, tools, and data. We have a host of PRA technical experts on our staff, thank goodness.

PRA today plays a major role in NRC's regulatory process and in many of the initiatives that improve the Agency's effectiveness as we work to accomplish our mission.

Can you imagine an NRC without PRA? Even before the NRC was created, government, industry, and academic visionaries thought it would be useful to have a new quantitative, probabilistic representation of the risk, but they didn't know how and whether it could be done.

It took the concerted efforts of Norm

Rasmussen at MIT, Saul Levine at the Atomic Energy Commission, and others and the NRC staff, ultimately the Reactor Safety Study Team to produce the document that we refer to today as WASH-1400 or NUREG-75-14 I believe.

NRC and team worked to develop this probabilistic assessment and it showed not only that PRA was possible but it also was capable of providing new, important, and actionable insights that could benefit reactor safety.

WASH-1400 was published in October 1975, 40 years ago. And the effort was begun in 1972 before the NRC was even created by, at that time, the Atomic Energy Commission.

But why did the Atomic Energy Commission undertake this effort? What prompted the idea? What did the AEC hope to accomplish by performing a PRA? What were the goals of WASH-1400? How was the study conducted and using what information?

What were the major challenges and how did they get overcome in order to complete WASH-1400? How were the results of this first-of-a-kind study received? The questions are numerable.

This morning, we are fortunate to hear presentations from two experts, two distinguished and entertaining speakers which I'm sure they will not let us down. Dr. Thom Wellock who's NRC's historian is in the process of writing a paper on the origins of risk assessment and WASH-1400.

Thom will begin by focusing on the origins of reactor PRA which can be traced back to the 1940s, and carry through to the beginning of the reactor safety study.

And at that point, Dr. Robert J. Budnitz will pick it up. Bob is a member of the scientific staff at the University of California Lawrence Berkeley Laboratory and the former director of the Office of Nuclear Regulatory Research at the NRC.

Bob also was a member of the Risk Assessment Review Group which was chaired by Hal Lewis and is affectionately referred to as "The Lewis Committee." Bob will pick up the story in the early years of the study and carry it through to the early 1980s addressing the performance, the reception, and the consequences of the study.

I think you'll find their talks fascinating and informative, thought provoking and very relevant to understanding how the NRC works today and what practices we have employed.

Please join me in welcoming Thom Wellock and Bob Budnitz.

DR. WELLOCK: How do I sound? Loud enough?

PARTICIPANT: No.

DR. WELLOCK: No? How's this, better? Still not? Higher? How about now? Okay. All right. When I first started working on this I talked to Nathan about this topic and he looked at what I had written and he said you're focusing too much on probabilities and not enough on the development of consequence studies and the like.

And I had to kind of address that. And you can see in my --

DR. BUDNITZ: It's not loud enough.

DR. WELLOCK: It's not loud enough? Is that --

MODERATOR: Loud enough?

DR. WELLOCK: Is it?

DR. BUDNITZ: You're fine they said.

DR. WELLOCK: Okay. You know, he wanted me to talk more about the development and study of consequence as opposed to probabilities. Why did I focus so much on the "P" in probabilistic risk assessment in my historical work?

And I wanted to say up front that's

largely what, the P is what led the development of the history, the emphasis that finally leads to the launching of WASH-1400.

In looking at this, consequences, probabilities, from an early period in time, right in the late 1940s the Atomic Energy Commission and people who were working on production reactors were pretty confident that they could get if not a ball park estimate of consequences, at least something that was in the same county as far as what was realistic.

Probabilities, they didn't feel like they were even in the same solar system as far as realistic estimates go. And so much of the hunt to try to develop this, a model really focused on probabilities over time, over the first 20 years or so, that was considered the major challenge that went on.

So, next slide, please. When I came to the NRC, I've only been here five years, didn't know much about this subject at all of course. And I took that class on reactor safety that a lot of people take which uses the NUREG perspectives on reactor safety, NUREG-CR 6052 or whatever it's called.

And I can remember learning basically a few things about the early history of reactor safety. A deterministic design was the dominate preferred approach, that fault tree methodology had largely emerged borrowed from aerospace industry, and also that WASH-1400 was really launched basically at request of Congress to the AEC, that the AEC was kind of a passive recipient of the idea of launching a risk assessment.

So my research kind of found -- these are not all wrong but I do want to modify them in a certain way. Next slide, please.

As I note here, it's what we see is that determinism certainly was there from the very beginning but the desire to develop some sort of probabilistic model really dates back to the late 1940s and basically a long-standing a frustration that they couldn't do it.

And so there were efforts in the 1950s and onward trying to come up with some sort of model that could help solve some of the problems that had developed with determinism that it couldn't address, questions that couldn't be dealt with.

I was also very surprised to find how much work had been done in the nuclear industry, especially in the 1960s that really was outside of the aerospace industry, development of decision trees and fault trees, event trees that really I didn't know about and was not discussed in the history.

And finally, in many ways the AEC had launched this study even before Congress requested it, largely because they had regulatory, technical, and really political reasons that all kind of came together in the early 1970s that convinced them despite their skepticism, that this is something that they needed to try.

Okay, so I'll start early on. Next slide, please. Seems like everything I do these days takes me back to Hanford in the late 1940s. Every time I do research I wind up back in the wind swept plains where I used to live in eastern Washington and Hanford.

Safety approach. From 1940s, the basic idea to keep a reactor safe was isolation. You can see it in this photo, the wide open plains. The idea that these reactors would be safe for the public by isolation, miles and miles of nothing stretching out endlessly.

And so the Hanford reservation was huge, right? And so through the 1940s that seemed to work well. By the late 1940s however the AEC had been formed in 1947. A committee called the Reactor Safeguard Committee was created which was really the forerunner of the Advisory Committee of Reactor

Safeguard, the main changes in 1953.

The reactor safeguard committee starts reviewing reactors throughout the AEC system. There's ones being established now in other locations. Argonne National Lab wants to create a test facility and there's debates about whether to put it in Chicago, Chicago, or somewhere out in the middle of nowhere, Idaho.

Brookhaven is, you know, going to build a reactor. So there's lots of reactor questions that are emerging, safety questions that have to be answered. And the Reactor Safeguard Committee is formed.

They look around and they look at Hanford reactors and they scare them. The reactors at Hanford seem to be getting more dangerous over time. They were developing safety problems that hadn't been anticipated, the graphite was expanding causing potential problems for scrams.

And so they find very little inherent safety in Hanford reactors. And at the same time they start developing a philosophy of safety which today we call, of course, defense in depth.

But you can see elements that kind of just lay out the idea of layers of defense even in their descriptions in the late 1940s, ideas of, you know, inherent physics in the reactor, barriers, and of course in the case of Hanford, isolation.

They felt that these things were necessary because the reactors themselves seemed very unstable and potentially could explode. Okay, next slide, please.

The person who leads the Reactor Safeguard Committee is Edward Teller. They take a very conservative approach to reactor safety in this period and, next slide please. This is basically how they wanted safety to be pursued at a reactor like Hanford which was considered the least safe in the AEC arsenal.

Other reactors, they thought, had more safety characteristics. But basically the exlcusion area of a reactor, exclusion area, not low-population zone, exclusion area, that distance was determined by point zero one times the square root of the power which if you had a 3,000 megawatt thermal reactor would be about 17 miles of just nobody, nothing.

You couldn't have anything going on in that 17 miles. So this was a very conservative approach of exclusion. Now that created problems at Hanford. Next slide, please.

They started taking this formula and applying it to the reactors. You can see the location of the reactors, these squares. And you can see these circles. That's applying the formula. This is 1951. They started applying the formula to the Hanford reactors.

This stair step line here is the borders of Hanford. So obviously they had a problem. The formula didn't work for Hanford. They were going to have to find some other means of safety.

And the Reactor Safeguard Committee starts to pressure the contractor at Hanford General Electric. By the way, DuPont designed the reactors during World War II and GE takes over after World War II is over as the main contractor.

And they start pressuring GE to find some way to get back to that happy place that they once were where they had really kind of assurance of safety. They kept, you know, their memos say please make the potential for sabotage impossible.

You know, please come up with a fool proof safety device. They use the word fool proof safety device to ensure that these reactors are going to be safe.

GE kind of patiently tries to explain you can't really expect that to happen. But GE is under pressure from the Safeguard Committee to find some way of making these reactors which were vital in the middle of the Cold War, you're not going to shut them down, to keep operating and achieve some assurance of safety.

And so next slide, please. I was going through a Department of Energy database one time and found this memo that had the Evaluation of Probability of Disaster. I love the way they were so honest back then. They used words like disaster and catastrophe and explosion at random. You know, it just didn't bother them.

So the Evaluation of Probability of Disaster, 1953. And this memo which was submitted by the director of their statistics division came up with a proposal that they thought would help make things better, or at least reassure the Reactor Safeguards Committee that they would achieve some level of assurance other than isolation, some sort of deterministic method to get to safety.

And he wrote in a memo that the probability of a disaster, you know, could be studied through what he called accident chains. A disaster, they reason, was a culmination of small malfunctions and mistakes, as he put it.

Well, there had been no disasters. There had been incidents which in the absence of mechanical safety devices, and/or the alertness of other personnel, could have led to disasters.

A disaster will consist of a chain of events. It may be possible to evaluate more specifically the individual probabilities in the chain and then amalgamate these results to obtain the probability desired.

So all went well, right, and everything just worked out fine and they came up with a number, right? Well, of course they didn't. Eight months later they come back and they say never mind. It's too hard. We can't do this. And they listed a number of problems.

First they said it was almost impossible to imagine all the paths to failure. They also, the data that they had, the failure data that they had in their records just didn't, you know, wasn't in a suitable form to come up with any kind of probability estimate.

And the other problem was expertise. The statistic people didn't know the reactors and the reactor people didn't know statistics. So they couldn't get there.

Now keep in mind, GE is, you know, they're an electric company. Electrical engineering is one of the main sources of reliability engineering and probabilistic work.

So they don't give up. For the next ten years or so they work on developing better understanding, trying to work on, in smaller chunks than some sort of big, grand figure of merit, some big, large number, top line event.

They decide to work on probabilities for components and systems and work their way up in the hopes that if they do this over a period of time, eventually they could put this all together.

And so over that time you see them do an incredible amount of work. I mean, I wandered into this database and I didn't get out for, like, three weeks looking at all this information that they had.

So they continue to work on it. GE works, plugs away at this. But in 1964, one of their staff finally admits considerable effort has been expended over the past ten years trying to develop a failure model which would make use of minor incident statics which would, through appropriate combinations, culminate in a major type incident. These studies did not prove successful.

Next slide, please. So that failure, I mean, they do continue working on reliability and trying to develop the pieces of a probabilistic model. But ultimately by the early 1960s, they're not going to do it. They're going to need help elsewhere which I'll talk about in a second here.

And I do want to point out one other thing. I think what I also understood about this is that the Hanford experience is unique. Unlike the other reactor sites that the AEC had, Hanford as I noted had the least stable reactors and therefore the ones of most concern, the ones that really required very active shutdown systems, very fast acting.

And so they had three scram systems. They had systems to ensure that the pile stayed wet and didn't, you know, go through a recriticality accident. And all of this required a focus on reliability performance and probability statistics that they could eventually use and put together.

Other reactor locations didn't really pursue this because the ACRS wasn't beating on them every day saying how are you making these reactors safer because they were seen as being inherently safer.

So it's kind of interesting because GE, of course, goes on to build civilian nuclear power plants. DuPont does not, and they're managing the Savannah River plant. And so GE carries this attitude of probabilistic approaches into their civilian era in the 1960s.

In 1967 you can see GE advocating both to the AEC and to Congress that probabilistic methods need to be pursued with greater vigor and that they have a great deal of frustration in dealing with a kind of deterministic safety approach.

So this is something that they brought out of their experience at Hanford and tried to, they wanted to apply in the 1960s in greater, you know, with greater vigor to various engineering problems dealing with reactor safety.

Next slide. Okay. As I said, GE needs help, and they're going to have to look around to find it. They're not going to develop this, it's going to come elsewhere. So, trees. Next slide, please.

Decision trees, they really start to develop in the late 1950s. This is just a decision tree that was developed in 1959 in an article that was considered kind of, you know, kind of a founding article along these lines relating to biological patterns. Next slide, please.

That leads to other developments. This is one. That is in a business journal and it is about marketing strategies, whether to test the market or not to test the market. Sticking in probabilities on each branch of the tree and developing outwards.

Now, so this is in the water. This is beginning to develop. More to the point for reactors, next slide please, Bell Labs Fault Trees, 1962. They developed them for intercontinental ballistic missile development, the Minute Man Missile.

And the first repor that comes out is in 1962. And here you can see them explaining their concept through an explosion in a hot water heater. That's the image on the left. And on the right is the fault tree for that failure. This is the first, basically, reference I see in the public literature because of course, Minute Man, there's lots of still classified material.

This is, in the public literature this is the first time fault trees really kind of appear is in 1962. So the aerospace industry jumps into this with vigor, and this becomes a major component in improving the reliability and performance of minute man missiles, what Bell Labs develops in '62. Next slide, please.

And this one is from a, this fault tree is from a 1965 conference that's organized by Boeing. Boeing starts to take these ideas and apply it into, like, the 747 development. So fault trees are clearly something that develops out of the aerospace, airlines industry.

Nuclear however begins to take a very strong interest around 1965 and moving forward in trees in general, decision trees, fault trees. Next slide, please.

This one is one that was developed for the snap reactor which was a satellite reactor, the SNAP-10A. And this is 1965. As you can see, it branches out showing the possibility of failure of the satellite returning to orbit, whether it would burn up and then assigns odds. This is very, you know, obviously very rudimentary early on. Next slide.

Atomics International takes an idea of event sequencing and they apply it. This is the first model that I see that's actually applied to a civilian reactor accident. And this is also in 1965.

By the way, I know you can't see the little lines here but they do provide actual failure estimates here. Usually they're in the range of 10 to the minus 10, 10 to the minus 11, in that area. So these early estimates were opbimistic, obviously. Next slide.

GE, remember GE? They get involved very early in developing fault trees. This one is from a hazard summary report, 1967. And so they're answering questions submitted by the AEC. And obviously this is again a very simple one. But they're pushing the idea of using trees to solve engineering problems.

Next one. Then they apply it to seismic questions. This one they contracted with Holmes and Narver to develop. Next one. And this one is for fuel failure. So GE, by the end of the 1960s is heavily involved in using fault trees to analyze specific problems. Next slide.

The one on the left is Battelle Northwest, the one on the right, Westinghouse. For some reason they just didn't want to use fault tree nomenclature. I don't know why. But nonetheless, there's a great deal of interest in the nuclear industry in developing this approach, using it and applying it to specific problems.

Oh yes, next slide. And I have to show a photo of John Garrick because John Garrick was, in 1967 he completes his PhD dissertation which develops a fairly sophisticated fault tree approach for that time.

He develops one of the first codes, the SAFTE code, S-A-F-T-E, which was used both I believe in Idaho National Labs, but they also used it at Hanford to look at specific problems. Next slide.

This is actually from his dissertation. So clearly the state of the art is beginning to move forward, that you're getting developing methodologies that GE never really had in the 1950s are now coming along in the 1960s. And one more slide on this.

I can't leave out the British. The British take a strong interest in event trees. And this is a 1969 publication. And a guy named Michael Pew had developed these ideas for British regulation.

And certainly the Rasmussen reports borrowed some of these ideas from the British. So naturally with all this work, nuclear regulators in the Atomic Energy Commission love this approach, right? They just do. I mean, it's obvious.

Why would you say no to this? Well, they are probably the most reluctant entity out there when it comes to accepting the idea of using fault trees and risk to evaluate quantitative risk for reactors. Next slide.

Steve Hanauer. Partly I show this photo just so, I mean, don't you look at this photo and say my God, I'm glad that 70s fashion sense just went out of style? I had Steve's glasses back then. So, you know, I am not picking fun at him.

But Steve Hanauer. For those of you who don't know, Steve Hanauer was one of the, I mean, he was one of the leading intellectual lights on the regulatory side in the late 1960s, early 1970s. He had been on the Advisory Committee for Reactor Safeguards, PhD from University of Kentucky I believe.

And when people talk about Steve they usually use two words, brilliant and narcolepsy. The poor guy would fall asleep in the middle of meetings. Apparently he even fell asleep during his testimony during ECCS rulemaking hearings.

But what was startling about it is he would wake up in the middle of meetings, you know, after being asleep for a while, and he would ask the most trenchant question during the meeting. And so he was both brilliant and obviously kind of eccentric.

And yet Steve had, you know, a very penetrating intellect. He remained, as well, very skeptical of risk assessment for quite some time.

A British counterpart in 1969, he wrote to him we the AEC have not yet arrived at the point where probability analysis techniques give adequate assurance that failure modes are indeed considered adequate. That probabilistic models for severe accidents that correspond to actual failures will occur as predicted, and that we are also skeptical that adequate failure rate data are available for prediction.

If you notice, everything in that sentence talks about probabilities. They were fairly certain, as I said, that they could do consequences. But probabilistic work still seemed out there. Why were regulators so skeptical. Next slide.

You have to go back to 1957, WASH-740 which is the first major report done on the estimates of major consequences resulting from civilian nuclear power plants. Previous work had been done on production reactors, but civilian reactors, this is the first major one. And it predicted, as you probably note, fairly large consequences.

It was a very conservative, worst case kind of scenario. In this report they, you know, knowing that they had very large consequences predicted for the worst accident, WASH-740 tried to address probabilities.

And they basically confessed that what they could provide was an engineering estimate, an expert judgement. In fact, they even admit in the report that they had talked to some experts who absolutely refused to give a number because it would give credence to the whole idea that you could come up with a number and that ultimately they said, you know, they said something to the effect that we will likely never know what the probabilities are of a major accident. That's in 1957.

And with that, I think they hoped that this whole approach would go away. And it wouldn't, of course. Next slide.

Yes, the 1965 update. In 1964, Frank Pittman who was in charge of reactor, basically what would be considered reactor research today, although it was on the pro-development side not the regulatory side of the AEC, gets called before Congress.

Congress, the Joint Committee of Atomic Energy, the very powerful joint committee which really had incredible influence with the Atomic Energy Commission and really a kind of hammer lock control over legislation, hearings, over anything having to do with radiation.

They say to him we really hate the 1957

report. You know, it just gives us no end of heartburn. Critics are bringing it up all the time. Can't you revise this?

And I wish Pittman was still alive because he sounds like a really smart guy. He says, he basically gently told them that's a bad idea. We don't have new data. So you're basically just going to run the same study with bigger reactors and it's going to be worse.

And he warns them so, you know, if you go off and do another study and it doesn't make it better, it might make it worse. Chet Hollifield, the Chairman of the Joint Committee, reasons otherwise.

Two months later he says, you know, you guys have done all this stuff, you put containment buildings around stuff, it's got to make it better. Go off and do a study.

So the AEC reluctantly goes off and does the update and they discover, lo and behold, if you take a worst case accident, it's worse, much worse because the reactors are bigger.

I mean, it was fairly simple. And so the AEC is stuck with a report they don't want with bad news. And so they said well, maybe we should go look at probabilities again.

They contract with a company that they had actually been funding some small studies on reliability through Holmes and Narver and Research Planning Corporation, both out in California.

And they asked them could you guys run some numbers and see if you could come up with a decent probability estimate for an accident. They come back and their estimate ranged from 10 to the minus eighth to 10 to the minus sixteenth, which is a range that's just mind boggling.

You know, it's just, 10 to the minus sixteenth is 700,000 times longer than the age of the universe. And so the AEC wisely looked at this and recognized that probabilities were not going to solve this problem, this report.

And they understood, really they understood why these numbers are coming out so high. No one could really estimate what, you know, they called it the time common mode accidents, the common cause accidents, but there were a number of technical problems that were making these numbers too optimistic.

So the AEC basically decides not to publish the report, to leave it. And they simply report back to the Joint Committee that our answer to you is that, you know, we've done enough preliminary work that we recognize that in some cases the consequences may be worse.

That may be good enough for your purposes since they were debating renewal of the Price Anderson Act. That was the point. At least that was the claimed point of why they were doing this.

So after 1965, the AEC had come away from this with a very bad taste in their mouth that probabilities were not going to be the answer for what they were doing. And this was in 1965.

So all that development work I showed you was going on, but the AEC still is very, very skeptical that this approach is going to yield anything that looks realistic.

So they, you have a strong degree of regulatory skepticism right up through 1970. Now that memo that I read you where Hanauer was writing to a British counterpart was in 1969. A year and a half later the AEC start down the road to WASH-1400. Why? Next slide.

The AEC is coming under increasing criticism. Everything I've talked about so far mostly focuses on technical issues. What's going to move them off the dime are a number of factors that really start pushing them to try to come up with a better answer to the question how dangerous are reactors.

And so first you have the anti-nuclear movement. By the late 1960s, early 1970s the anti-nuclear movement's gaining steam. There's increasing opposition to local power plants, there has been a controversy about low level emissions from power plants that featured AEC scientist John Gofman and Arthur Tamplin.

They were dissidents who came out critical of the AEC. And finally there's the ECCS controversy that's already starting to develop by 1970 and will result in hearings that are very controversial in 1972.

All of these things are making the AEC an agency that once was immune, more open to public criticism, that it wasn't paying enough attention to its regulatory focus as opposed to its promotional focus on reactors.

They're also coming under fire from other federal agencies, or at least criticism. NEPA is passed in 1970 and Environmental Policy Act and environmental impact statements lead the AEC down the path of having to estimate the impact of reactor accidents on the environment.

And the EPA wants them to start developing top event numbers. They refuse to do it for Class 9 accidents, the worst class as it was known then. So the AEC is having now to answer to other federal agencies.

And then of course new technical problems begin to emerge. ATWS. 1969, this emerges among the ACRS and is debated over the next several years. To the nuclear industry, they thought ATWS was just an academic exercise.

GE goes off and produces an estimate that says the changes of an accident through ATWS was about 10 to the minus 15. AEC thinks that number is way out there. But you don't have any kind of resolution to this.

So there's increasing discussion within the ACRS that the AEC needed to develop greater ability to develop fault trees and analysis on their own, independently of the industry.

So these things are percolating. None of this is necessarily going to result in a multi-million dollar study. We need one other item. Congress. Next slide.

As the Atomic Energy Commission is increasingly under criticism from anti-nuclear activists, those activists are also going to gain allies in Congress.

Senator Mike Gravel, a liberal Democrat from Alaska. Remember what I just said, a liberal Democrat from Alaska, an anti-Vietnam War proponent, excuse me, an opponent of the Vietnam War, he was also in many ways critical of the AEC because they were doing nuclear testing on the Aleutian Islands for thermonuclear test devices.

And so he was already critical along those lines. Picking up criticism of atomic energy wasn't that hard for him. He's very close to the anti-nuclear movement. And in 1970 he sends a letter to the AEC and says I would like to see the 1965 WASH-740 update.

This is before the age of FOIA, but you don't easily say no to a senator, even though in this case he was just a junior senator, freshman actually. This was his second year in Congress.

And so the AEC debates what to do. What do we do? Do we give it to him? That's not going to be a lot of fun. And so they say no. But they come back and they say well, we'll go off and do a new study, a study that will basically update WASH-740.

Now that commitment doesn't necessarily

turn into WASH-1400. Gravel doesn't have a huge amount of influence. I mean, any kind of study might have done. But they commit to him in late 1970 and in early 1971 they split the study.

The development side, the promotional side of the AEC is going to develop a study that's called WASH-1250 which kind of lays out the safety philosophy of the AEC. But the major consequence study is going to be taken over by the regulatory staff.

Now the regulatory staff is a pretty small entity at this time, a few hundred people. They don't have a lot of expertise. This is going to require something more. But they do commit in May of 1971 to a study that will deal with probabilities and consequences.

Total budget, $200,000. Basically 20 times less than they actually spent which tells you that whatever they thought this study was going to be, it probably wasn't the very ambitious study that we wind up with.

But nonetheless, May 1971. As I said, Gravel is not a major player in the Senate but he has to be answered. So we need one more item, the Joint Committee of Atomic Energy. Next slide.

Saul Levine. In the summer of 1971, Saul

Levine is on loan to the Joint Committee. And he very astutely reads their mood. It's not just the AEC that's under fire by this time. The Joint Committee itself is beginning to feel vulnerable.

The environmental movement had empowered Congressional committees that looked at the environment of course, that gave them an ability to start challenging the turf of the Joint Committee.

And so the Joint Committee is also feeling a little vulnerable that other Congressional committees may start horning in on their territory, start holding hearings on radiation questions.

And Levine says what you guys need to do is launch a study. This is just a couple months after the AEC's already kind of decided they're going to do one. But Levine gets the Joint Committee interested in a study that deals with consequences and probabilities and laying out the safety approach.

The idea is they can have a study done, they could hold hearings. And that would kind of take the wind out of the sails of any other committee that was thinking of doing something.

And so by the end of 1971 they're very interested in carrying out a study. And so that gives impetus not only to the development side study, but it also gives impetus to the regulatory staff to launch something in a much bigger way.

And next slide. Early 1972, March of 1972 Steve Hanauer brings in Norm Rasmussen to lead the study. Now there was actually a couple of people that they had tried to recruit in 1971, people had said no. That was part of the reason the study kind of lags in 1971, not much goes on.

But in March 1972, Rasmussen commits from MIT. And he and Hanauer basically sit down and kind of lay out the basic components. There was basically five areas that they were going to focus on, three focused on consequences which was considered actually the easier part of the study to do.

But the probabilities, that was the part that they discussed during their meeting as being the challenge. Rasmussen told Hanauer there may be significant lack of precision in the results we develop.

And Hanauer who wrote some of the most razor sharp memos in the federal government, some of you may remember Steve Hanauer, after Fukushima there was a news report, there were news reports about a memo written back in the early 1970s where an AEC staffer said we shouldn't license Mark I containments anymore. That was Steve.

So he spoke his mind. And after meeting with Rasmussen, after he gets the commitment he wrote down we may have to learn by trying. But the only way to really see if we could do this basically was to go off and do it.

That for all the work that had been done elsewhere, there was still questions that weren't resolved. How do you deal with common cause accidents, is there enough data out there, all these kind of questions that were still lingering that prevented people from getting a real number that they felt comfortable with.

He said do we dare undertake such a study until we really know how. Are we willing to be told the task is impossible of achievement with presently available resources? We want the whole package. Doing accident consequences without probabilities would be another WASH-740 with the risk still unquantified.

We might have to settle for that, but we want to try probabilities. And that basically lays out the strategy. If they can't get probabilities that they feel comfortable with, it was going to basically fall back to a report that was going to say, you know, we've made some advances. Here's areas that we need to work on.

That was kind of the thinking in 1972. But I think as Bob will talk about, you know, how the report develops, I'll leave that to him. By 1972, Rasmussen and the team get to the point where they start to feel comfortable that they can actually develop numbers that can be defended as something closely realistic without large error bands on either side.

And so next slide, please. So I'm going to turn this over to Bob. But I just want to leave a couple of points. It's launched in March '72. What we can see from the discussion I've just described, by 1972 there were good technical reasons to launch a study, there were increasingly regulatory questions that had to be answered such as things like ATWS but also there was a strong growing political need.

They needed an answer to be able to say that reactor accidents were, you know, incredibly unlikely in the range of airplane accidents or, famously later, compared to the risk of meteors.

And so in 1973 we have significant advances in methodology and enough so that the AEC starts discussing creating an analysis group formed around the Rasmussen group to be used to kind of, you know, expand their expertise in this area to be used for regulatory questions down the road.

Finally, I just want to say the part that really leads to a great deal of political controversy as you probably know is the Executive Summary. The Executive Summary in the end, the way it was formed for the final report is the area where they really try to compare the risks developed in the study to other risks.

They used natural catastrophes of course, but they also used airline accidents and the like, laid out in various graphs and the like.

In early 1974, Dixie Lee Ray, final slide, with her dogs. Dixie Lee Ray goes before Congress and provides the first estimates out of the report which were, as you can imagine, you know, the industry loved them. People who were promoters in nuclear power loved the numbers.

They were very, very unlikely compared to airline accidents and the like. They seemed to be very good. In fact, she used a comparison. She said that the chance of a major accident was basically the same as drawing a four of a kind in draw poker twice, which I think also kind of tells you that this kind of points to where the report's going to be controversial, kind of care in comparing numbers that have, you know, most people think have a lot of certainty in them compared to these numbers that are coming out of a brand new report with uncertainty in them is going to become a major point of political contention down the road and is going to lead to a lot of the questioning that creates problems for the study.

So there's a combination of, you know, tremendous advancements that come out of the WASH-1400 report but also some missteps. Okay, thank you.

DR. BUDNITZ: I think I'm going to talk from the podium.

DR. WELLOCK: Oh, you are? Okay.

DR. BUDNITZ: I'm going to talk from here. Okay, great. You don't know it but I can't see you because of the lights. And you can hear that I'm hoarse. I got up yesterday morning hoarse. Nothing else wrong with me. I'm going to try to talk through it. I'm sure I will.

I'm going to start right where he left off, but I'm going to back up just a little to talk about regulation because as all of you know because it's still in place, the way then as now, the center of regulating severe accidents in the NRC as it was in the ACE is what we call design basis accidents.

And I can list a bunch of them, but I'll just mention two or three. I mean, obviously there's a large break LOCA. There's the largest earthquake at the site that is contemplated. There's the total loss of AC power, a bunch of them.

And those design basis accidents were the center of the original AEC analysis and assurance that reactors were going to be safe enough to run. But I want to point out something that I know and I assume you know which is the design basis accident is not an accident at all.

Each one of those is merely what we call an initiator for an accident. The loss of total offsite power is an initiator. The large break LOCA is an initiator.

The way the original AEC regulators were put in place, and by the way, this is with the total concurrence of industry working at the time, was for every design basis accident, equipment and procedures were put in place to make sure that when it happened, this, this, this, this, and this happened. And then when they happened, the reactors were going to reach a safe state without a core damage accident.

And you know, for example, for the large break LOCA there's the accumulators and, all right I don't have to go into it but you can understand. And the assurance that was had, that people had was that if all of that equipment worked fine, after the design basis accident, why you would reach a safe state for anything like that and also for anything that was lesser than that. Okay?

Now that approach which was the, and by the way, there are human errors too but then of course there was procedures and the like. And the belief at the time, and I was just starting in this business in the early '70s, was that if you had a design basis accident and all that stuff worked, things were safe.

The probability that things wouldn't be okay, that you would reach a core damage state, was thought to be incredible. The word incredible was used all the time. It was used in the industry's literature, it was used in the AEC's literature, it was used in the university community. The word was it was incredible.

I want you to remember that word because I'm going to come back to it. Now the belief that it was incredible meant that if it was incredible to reach a core damage state, it was surely incredible to have a large release because that was, well you can follow that through.

And the general idea then was that with that in place, you didn't have to worry. Along come Rasmussen and Levine in this study and they set out to do the following, and this is really, you got to think about it for a minute.

Norm and Saul, I'll use them, first names. I knew them very well. I love Saul like an uncle. Norm and Saul and their team set out and said you know what, we are going to identify for each design basis accident all the things that might happen because that equipment I just told you about might not always work because if it always works it's safe.

You get a design base accident which, initiator, it's an initiating event. We're going to work out all the different accidents that might happen because that subsequent equipment might not work because the probability it works isn't 100 percent. It's whatever it is.

And furthermore, they identified a bunch of initiators which were less than the design basis accidents which were qualitatively different than the design basis accidents.

We now understand, we've understood for

the longest time, that not all LOCAs are enveloped by the large LOCA, the double-ended guillotine break. Not even all large LOCAs are enveloped by that.

Now here's the key. Okay? Norm and Saul and their team said to themselves we are going to identify every accident sequence that you can think of, I mean that matters. Every one.

And for each one, we're going to tell you through the analysis what the probability is of that accident sequence reaching that undesired end state which is core damage. And then thereafter we're going to work out for the various end states which ones of those led to a release inside the containment, which one of those led to release outside the containment, and then where it went offsite.

So we are in the end, they said to ourselves, the team, I wasn't part of it, we're going to identify every single accident sequence that matters and we're going to work out not only its probability because without the probabilities you don't know where you are because they're all the same without probabilities.

Some of them have high probabilities, some have low, some have really low. And we're going to work out the consequences, the consequences being core damage or some release into the containment or some release from the containment. And obviously in the end it goes somewhere offsite.

The safety community said to themselves and said to Saul and Norm and the team go for it, but we don't think that's possible. That was the prior thinking at the time. Could I have the first slide? I only have one slide.

And this is a chronology just to lead you through. And the first line says the early objections, before they even launched it. People said to them and people said to each other you can't do that. There are too many of them, it's too complicated and even if you identified them, you could never work out all those probabilities because we just don't have the data to support it.

And so they just didn't believe it. Now I want to point out that the Reactor Safety Study, WASH-1400 was limited to internal events at full power. They didn't do earthquakes, they didn't do other external hazards, they didn't do fires.

So it was just internal events. But I mean, that's a huge scope, but that's what they did. So there's some stuff they didn't do. Some important stuff they didn't do. But nevertheless, believe it or not, when they published the draft and later the -- the draft in '74. I had the privilege of reviewing the draft at that time, and then later the final in '75.

They actually demonstrated that it was not only possible, but they pulled it off. Go read it. They did two reactors, Surry, PWR, Peach Bottom, BWR, Mark-I. They did in fact identify every important accident sequence.

If you look back now 40 years later, there are not important ones they didn't capture, internal events. Okay? They worked out the frequency of every one of those using these techniques. The frequencies are more or less right.

I mean, they had uncertainties and we know a lot more. They worked out the end states, core damage for some of them, some of them led to release into the containment, some led to offsite.

They worked out the consequences, radiological consequences. And when you look back at those, there's some places where they didn't get it right because there weren't enough data or not enough experiments.

But more or less 40 years later you can look back at that study of those two reactors and they more or less got it right. And when you think about it, it's not a miracle. It's a tour de force, a major intellectual accomplishment.

I can't think of anything else in our field that is as major a step forward or a breakthrough as that in the whole history going all the way back. It's just a major, major accomplishment.

Now crucially, in order to do this right it has to be a realistic analysis, a realistic estimate of the probabilities of initiating event, a realistic estimate of the probabilities of failure of the subsequent stuff, a realistic estimate of the probabilities of human error in the control room or somewhere that then aggravates the accident, a realistic estimate of the consequences.

Now some of those things we now know aren't right because of other newer information, but more or less they got it right. And when you think about that major intellectual accomplishment, it wasn't appreciated at the time, only later as the field matured was that appreciated.

But that was a crucial, crucial breakthrough. The second crucial breakthrough was they insisted that you couldn't do this without quantifying the uncertainties, the uncertainties in the probabilities, the frequencies, and the uncertainties in the consequences.

And they tried to make as best they could an honest estimate of those uncertainties which were large, but nevertheless not very different from what we have now. They underestimated it in some ways.

But let me just now go to the early insights, and this is really important. For those two reactors, the core damage frequency for Surry and Peach Bottom internal events they found was a few times ten minus five per year.

They found the probability of a large release was less than ten percent given the contingent probability, given that. They found that most core damage accidents actually stopped inside the vessel.

They found that most releases were small and only some of them were large even if you got a release. And they identified the importance of structures and components and systems and so on that mattered.

They identified for the first time to everybody's mind that small LOCAs were really much more important than people had thought, certain transients were important, loss of offsite power. By the way, blackout they identified as being important, for the first time really understood that.

And that large LOCA wasn't as important as people had thought which is where much of the effort had gone. Now it's really important to understand the reaction. Many in the industry thought this study couldn't possibly be right because those frequencies were way too high.

They had been thinking and told each other and they had published all over the place that large accidents with these things were incredible. They weren't incredible. It's a few times ten minus five.

If you're running 500 of them which was the prediction at the time, we only ended up with 100, but if you're running 500 of them you can work out that you're going to get one of these every century or two.

The anti-nuclear people thought they couldn't possibly be right because they had been telling themselves that every core damage accident was going to contaminate the size of the State of Pennsylvania. Do you remember that? Or if you're too young to remember it I'm telling you.

That it turns out not so. There are none that do that, and almost none of them contaminate anything. Okay? The anti-nuclear people thought it couldn't possibly be right. They were convinced that the study had been dishonest and was hiding stuff and they wrote it and they testified before it and they sent it to Congress and they wrote it in their own newsletters.

They were convinced that the people were so pro-nuclear that they had obviously been intellectually dishonest, and they said so. Couldn't possibly be true.

The NRC, the new NRC, the staff, it was the same staff, the regulatory staff, concluded that this showed that plants were safe enough. Dixie Lee Ray said so herself and Schlesinger said so later.

But they basically sat back and watched as the reaction took place. Now crucially, one of the first insights of the study that Saul Levine himself who had been a regulator for 15 years wrote down was that he found that some of the regulatory things were inadequate and some things were overkill.

He wrote that down early on. And those insights by the way remain with us. For example, something that was overkill was we had allowed outage times and action statements at some of the plants that you didn't need to shut down after three hours when three days would be enough when something failed because you could show it through the study that it wasn't so important. That sort of thing.

So there was a whole lot of stuff going around inside the regulatory staff in which regulations were thought to be overkill. We should, you know, regulations were thought to be inadequate. The anti-nuclear people were saying what they were saying.

And that went on for the first couple years. But to his credit, by the way, Herb Kouts was the first director of the Office of Research, 1975. But about 18 months later he retired. Saul Levine became the director of research. I was his deputy.

And Saul, to his credit, launched a whole bunch of studies I'm going to tell you about in a minute, to fill in and help understand the places where the study itself had shown that there was not enough information. I'll talk about those in a minute.

Those were launched by Saul Levine as the director. Of course he had been the study director with Norm of the study itself. And it was launched by him in the days before he had to ask anybody's permission.

I can tell you that if he had to get a user request from NRR, they would have said no we don't want it, we don't need it, we don't care about it. Everybody, almost everybody in NRR, I'm talking about the intellectual giants of the time and they were wonderful, important people, didn't think that that research was with doing.

But Saul had the authority. It was only five percent of the budget by the way, five percent of the research budget went into this. I'll tell you what they were in a minute.

But launched a whole lot of research that took five years to play out, and a lot of it didn't play out until after the accident at three mile island.

Well two years passed and there was a lot of turmoil. It was going nowhere. The commissioners themselves, '77, decided to put together an expert panel of a half a dozen experts to spend a year and opine and give the commission back a report about what this methodology really could be used for in regulation and where its limitations were and why.

It was called the Risk Assessment Review Group, the Lewis Committee. I was privileged to be one of its members. And we spent a whole year and we basically heard from everybody. I mean, everybody came out of the woodwork.

Every month we spent two days having people come and tell us this, tell us that, tell us the other stuff, all the stuff I was just mentioning. But it was also a highly technical review.

And in the end, the Lewis Committee, it was published in the fall of '78, the Lewis Committee strongly endorsed the use of these methods in regulation, but it also was very blunt about how although the methods were terrific and the insights were important, the executive summary was way overstating what you could get from this.

Crucially, there was only two reactors. Right? That was enough. Crucially it didn't have earthquakes and fires which we now know are half the risk at many of our plants. It didn't have shutdown.

Way overstated it. And the Lewis Committee, as I said, I was on it, also said that although they struggled to estimate the uncertainties in the study, they had underestimated them by a good deal.

The Commission thought about that for three months. And in early '79, just three months later or so, the Commission wrote an opinion piece or whatever you call it, I can't remember what it's called.

PARTICIPANT: A policy statement.

DR. BUDNITZ: A policy statement, thank you, which in retrospect was very harmful. Except the accident occurred two months later and it turned around. But it was very harmful.

The Commission bailed out on the Executive Summary, and that's what got the press, and that's what got Congress involved. Congress called them up there and they had testimony and all that stuff. And they bailed out on the Executive Summary.

And everybody said the Executive Summary is no good, the report's no good. And the Lewis Committee said that the core damage frequency aggregated numbers were unreliable because of uncertainties but the whole, all these other insights were important. It said that.

And it should be used. Now that was going on in January of '79 and I was at the time the deputy director of research. Saul retired later that year and I became the director.

And while all of that reaction was going on as you know perfectly well if you read the history, at the end of march we had the accident at TMI.

Changed everything.

But in the meantime before then, the Office of Research had launched a whole bunch of studies. They launched reactor safety study PRAs on four reactors, the NRC launched them. They launched the Seismic Safety Margins Research Program at Livermore which basically developed the methodology for seismic PRA.

They launched a crucial study on human reliability and human error rates at Sandia that Al Swain published only a couple years later. They launched methods for fires at Sandia and so on. They launched something that became the Fault Tree Handbook.

They wrote a bunch of computer codes to computerize, make things doable. And EPRI, to its credit, launched a whole major study on gathering data, a whole lot of reliability data, the sort of data that you need to do this better.

And EPRI launched a whole study on HRA, human reliability analysis also. This is all before Three Mile Island. Then all of a sudden, and the Germans launched a study just like WASH-1400 at Biblis under Adolf Birkhofer just to do it over, and their results weren't very different when they came out.

But then all of a sudden we had the accident at Three Mile Island. And I was on the staff, and it couldn't have been two days later the Commissioners called in Saul Levine and they asked him gee, is that sequence that happened the day before yesterday in the reactor safety study?

And the answer is yes and no. It wasn't because they didn't study that reactor which is specific. It was a BMW, it was just different. Broadly yes because it was a, as I suppose you know, it's a feedwater transient with a stuck open relief valve.

But in detail no because there was no way that sequence could have identified those odd events in the control room in which they turned off the pumps because they misread the pressurizer and stuff like that. They just wouldn't have identified that.

So probably not was the answer at the end. But of course we couldn't answer that in the second day because we didn't know the sequence. Now we know and we know that probably not.

Could PRA have been helpful? Probably so in alerting people to small LOCA, but probably not in detail unless they had studied those reactors. So six months after the accident, the Agency issued the TMI

Lessons Learned Report which is very much like your Near Term Task Force.

Couple hundred recommendations, they were prioritized A, B, and C. Not very many of them had the benefits of PRA insights despite all the effort to try to bring that to bear. It was really hard to do, and there was a lot of resistance in the NRR staff to using those insights to help prioritize.

I actually signed off on that report, I was in the Office of Research. But not very much PRA in that TMI action plan. However, I want to remind you about the accident.

Three Mile Island was a small LOCA. WASH-1400 said small LOCAs are the most important thing. Three Mile Island was full of human error probabilities. Human errors that had, right?

Three Mile Island was in fact retained in the vessel. WASH-1400 said most would be despite the previous thinking. Three Mile Island said that well, I won't list too much of that. I just want to move on.

But there was a crucial thing that happened thereafter. While the NRC's research in PRA that had been launched before the accident was going on, just at that time they also launch, the industry itself launches a study, a parallel study, to understand severe accident phenomenology much better.

The industry called this the IDCOR Program. The Agency, we called ours the Severe Accident Research Program. It went on five or six years. Spent a few hundred million dollars to understand the phenomena of severe accidents and core damage and aerosols and chemistry in a much better way.

And that was a really important thing because although we didn't say so to anybody, all of that research was aimed toward supporting Level 2 PRA. It was done to support Level 2 PRA. Okay?

Now crucially, and this is a crucial point about the leadership, and I just want to make the point. Saul Levine was the intellectual leader on the staff, and all those research things I told you about were launched when Saul was there.

I was only the director for less than a year, and then I left. And there wasn't anybody at the top of this Agency for at least a half a dozen years that cared much about PRA at all.

And not much new research in PRA was launched during those half a dozen years until the Chernobyl accident which was in '86 and then things changed again.

And the reason for that was just about the time I was leaving, this idiotic, I use that word in the most strong, I can't think of a better name for it, this idiotic idea that you can't launch anything in RES without a user request became policy.

And you know what? With some courage, the leadership in RES could've, but they didn't have the courage either. All right, I'm not naming any names, it just was so.

And not much was launched in those half a dozen years that was new, although the stuff that was launched was going on and it was producing really important stuff like the Fault Tree Handbook and like Swain's handbook and like a whole lot of stuff that led up to the PRA Procedures Guide which was an industry-NRC joint effort in 1983.

Joe Murphy did a feedwater study after the accident. He looked at all the Westinghouse PWRs and he analyzed the probability that the aux feed water, excuse me, the non-electric aux feed water, the turbine driven systems would be there when you called for them.

And all of those met regulations. And the contingent probability that they would be there when you wanted them differed by two orders of magnitude from the best to the worst one. These are the Westinghouse steam driven aux feed systems.

Two orders of magnitude and they all met regulation. And people in regulation said oh, we didn't realize that our regulations, the outcome of them produced such a wide variation. A big insight.

Just at that time, the industry began to launch its own PRAs. Zion/Indian Point with Garrick, the EPRI launched the arconee PRA in 1980. The industry was starting to do their own PRAs because they wanted some understanding on their side. And that went on for the first four or five years after the accident.

Not much new research at the NRC in PRA, but the research that had been launched was yielding all these useful reports, insights and so on. And crucially, all that time, EPRI was gathering data.

Now the next crucial insight came from Zion and Indian Point. The NRC had launched the SSMRP program, the Seismic Safety Margins Research Program at Livermore in 1978, '77. And they did a PRA on a reactor, a seismic PRA.

But at Zion and Indian Point, Garrick and his PLG team did a seismic PRA in Zion and a seismic

PRA at Indian Point. The first really competent seismic PRAs were done in the industry side. And they also did fire PRAs for the first time.

That was not an NRC initiative, although the NRC developed the data and some of the methods. It was an industry initiative. And that was all going on in the early '80.

But on the regulatory side there was very great reluctance to do anything with regulation. If you go and look at the regulatory initiatives in that period, I'm talking about between the accident and 1985, there are three or four things that had PRA insights that mattered.

There was the PRA insights that supported the revision to Appendix J which is containment testing. It's important but it's pretty unimportant when you think about it. There's the pressurized thermal shock rule, 50 part 61 that had some PRA insights.

The ATWS rule of 1984 was written with PRA insights but that's bologna. They did it all without it and then they backfitted it just because they were under pressure to show that PRA told them something. Okay?

The NRC was developing a strong capability in the labs, Idaho, Brookhaven, Sandia, Oak Ridge. And in the NRC staff itself there were groups that the NRC was supporting that did some really important stuff. And I said at Livermore with the seismic stuff.

And at Battelle Columbus and at PN&L for the Level 2 PRA work, and at Sandia Level 3. Dave Aldridge and his colleagues. So all that capability was being put together, but on the regulatory staff not much was going on at all in which the regulatory staff said gee, here's an insight. Maybe we ought to think about whether this regulation needs to be modified, needs some strengthening or perhaps it's just too much of a burden, doesn't produce much.

And that was all going on. There was one initiative going on at the same time on safety goals but I'm going to come back to it. So by the mid 1980s, that work I mentioned was going on, but not much on the regulatory side.

And then one day, I remember it well, we had the accident at Chernobyl, 1986. It was not our reactor. I was nothing like our reactor. It couldn't have happened here, everybody knew that. But it changed the perspective because it caused everybody to focus on what is the real probability of a big release like that at our place.

And that changed everything. People at NRR started to pay attention to these things, people in the inspectorate. By the way, the inspectorate was its own office at that time. People in standards development, the Office of Research got more money to do more stuff.

The Chernobyl accident in 1986 changed the perspective inside this agency and the future came thereafter. I'm going to talk about that future briefly just in a minute.

But before I do, I want to talk just briefly about safety goals. If you read the safety goals, 1986, it's transparent that nothing like that could have been written down never mind adopted but for PRA because in order to show, you got to do a Level 3 PRA.

Otherwise, how do you know what the prompt for that, how do you know what the latent fatalities are, how do you know what the probabilities are, this, you know, ten to the minus three.

That big initiative which was led by Dave Okeren in the ACRS and later on convinced the staff and the industry was possible, in fact enabled, by the advent of these techniques, even with their uncertainties. And the reason it flew, if you don't mind my saying candidly, is because every reactor could meet them with ease.

So it wasn't a problem. Okay? You got to think about that for a minute. In 1986, they could all meet, and by the way, this is still true, they can all meet them. They're licensed. And that enabled the political environment for that to be adopted.

Now just a couple more things, I'm almost done. Just about a year after the accident, a little bit before, the Agency launched a new office called the AEOD, the Office for the Analysis and Evaluation of Operational Data. It was a separate office for a while before it was absorbed into research.

And it started to understand operational events. And to it's credit, it used PRA methods to undersand the importance of them from the start. Okay? That's really important for you to understand. From the start, this was 1980.

A few years later the ASEP, the Accident Sequence Evaluation, the precursor program, the Accident Sequence, precursor is what -- What does ASEP mean? I can't remember.

Anyway, it was the precursor program, was launched, in which if you had a big event that stopped short the contingent probability of getting to the bad end point was worked out, and sometimes the contingent probability was 10 to the minus 3, but sometimes it was 10 to the minus 2, and sometimes 10 to the minus 5, and that, of course, used PRA methods right from the start.

That was launched initially because AEOD had the data and EPRI wanted it and everybody thought it was useful and that was launched in the mid-'80s.

Now by the late '80s, and this is sort of, I am ending here, by the late '80s and after Chernobyl the agency started to worry that perhaps the regulatory scheme that we had wasn't adequate but they didn't quite know where, so they launched the IPE Program, the Individual Plan Evaluation Program, in which every plant was charged to go and examine their plant and try to identify vulnerabilities.

Later on, a couple years later, it was the IPE for external events, it came later, but the IPE Program launched with generic letter ADA-20 so it had to have been 1988. It came along and all the plants did a PRA, that was 1988.

I'm about to end my story there, because the agency itself had launched NUREG-1150 in 1987, finally published in 1990, and the main purpose there was we had learned so much about the Level 2 PRA consequence stuff that everybody knew that the WASH-1400 estimates of consequences in the vessel were too high, and NUREG-1150 in fact showed that, that in fact there were much lower consequences than had been thought before, and we understood that because of the phenomena.

But, and this is a crucial point, by 1988, '89, and '90, although there was a lot going on, finally, and here it is today, 25 years later it's still going on, the maturity was still spotty.

Let me describe. Some of the IPEs were great, but some them weren't. The staff didn't do much of a review and, in fact, many of the IPEs that the utilities did just, they put them on the shelf and didn't use them, although a lot of them they did use.

A lot of them have been on that shelf for all that time. Some of the utilities launched in-house PRA groups, but not very many of them, but I think by 1991 or '92 there were five or six.

Not many of them had internal PRA groups, most of them used contractors for the IPE and in the staff, although PRA expertise was starting to develop, I think that by the end of the 1980s there were probably six, seven, or eight people on the staff who you would consider real PRA experts, well, great, but not 25.

Now part of the cause as I said was that the agency's leadership wasn't gung-ho until everything changed after Chernobyl and the user request system that I mentioned had something to do with it.

But by 1990, and I'm about to end my story, with NUREG-1150 the bright future was still a few years off but a whole lot of things were just budding at that time, and now I'm going to stop with that.

For example, the first shutdown PRAs were being launched. The reactor oversight process, which is now risk-informed, hadn't matured but the thinking behind it had been worked on, okay.

The maintenance rule which came along later was being thought about carefully by the agency, by the industry, and by the labs. 50.69, which came along a lot later having to do with the classification of equipment, came along a lot later.

And, of course, I don't have to tell you that at the end of the decade of the '80s was when the effort to put together a methodology standard for PRA started with ASME and the American Nuclear Society.

But the major lesson to me is the following, it takes a long, long time. Just think about it, I'm going to say it again. It takes a long, long time.

Why did it take a long, long time? There was a lot of work in the community of people doing the work, but trying to convince people who were fat and happy with where they were was very, very slow, spotty, and irregular.

And I want to tell you, it's now 2015 and that is still true. Less so, but it's still true. There are still some places in regulation, there is still some standards out there, codes and standards, national codes and standards that don't have thinking this way.

Certainly some of the industry do, a lot of them do, but not all of them, I can tell you, and in this agency the same.

So my major lesson is between when it took off in 1990, and the study was 15 years, it's now 25 years thereafter, there was a lot of parallel work, a lot of people doing different stuff, but to really penetrate takes a long, long time. Thank you.

(Applause)

MODERATOR: Thank you to Dr. Budnitz and

Dr. Wellock for their presentations. Now we are going to open the floor to any questions. We've got two microphones on the aisles and also anyone who has questions on the webinar please submit them and we'll read them.

DR. BUDNITZ: You got to stand up and ask a question.

(Laughter)

MODERATOR: No questions from the audience here?

DR. BUDNITZ: Of course.

MODERATOR: Are you still thinking, processing the information? Anything from the webinar?

DR. BUDNITZ: I'm surprised.

MODERATOR: You answered all their questions.

DR. BUDNITZ: There couldn't have been.

MODERATOR: You covered everything.

DR. BUDNITZ: Couldn't have been.

MODERATOR: There we go. Jose?

AUDIENCE PARTICIPANT: I have a question for Dr. Budnitz?

DR. BUDNITZ: Call me Bob, Jose.

AUDIENCE PARTICIPANT: Yes, Bob, yes. The question is you refer that some of these calculations have to be realistic. What do you mean by realistic? What is the meaning of realistic?

DR. BUDNITZ: Well what Norm and Saul and the team set out to do to was nowhere would they make a choice of analysis method or data that wasn't, that was conservative, that was realistic, unless they made a conservative choice which still could screen something out. That was okay.

But every accident sequence they identified they attempted to use realistic frequencies, realistic failure modes, and realistic human error rates.

And in the Level 2 analysis, which was led by Bob Ritzman and Rich Denning at Battelle-Columbus, they tried to use realistic understanding of the chemistry and the physics of the aerosols and the airborne contaminants in the containment.

And the offsite analysis, which was originally led, I can't remember who led it originally, but it ended up being led by Ian Wall, which was a Level 3 PRA, they attempted to use realistic analyses of radiological dispersion in the environment, realistic settling velocities, and realistic uptakes in the human.

So they did everything they could to make it as realistic as they could. In retrospect they did a pretty good job, although in some places they had a lot of judgement and they didn't get it all right, okay, but it was intended to be and in retrospect was realistic.

Everything we do in PRA is supposed to be realistic unless you can make a conservative assumption just to screen, okay.

MODERATOR: Okay, let's see. Dan?

MR. HUDSON: Well, I'd like to start -- My name is Dan Hudson in Research and I'd like to thank you both for your remarks today, and, Bob, if I can I'm going to put you on the spot here with your background in PRA and some of the candid remarks you have shared about the Office of Research and the user need system.

I'm curious, if you were the Director of the Office of the Research today what kinds of projects would you be funding to advance the state of the art in PRA?

DR. BUDNITZ: Oh, I'm not sure it's the state of art PRA that's the issue, although there is some of that.

There are a few things like that, but

mostly it's trying to understand using PRA insights each of our regulations, not just by itself, but a scheme of regulation in a given area, to see if we can make them, bring them into the modern era.

And I'll just give you -- That's a good opening, let me describe. The key to PRA that Rasmussen and Levine understood at the beginning was we are going to identify accident sequences.

But every analysis they did was centered around an accident sequence, okay. The NRC doesn't regulate accident sequences, okay. I'll just give you the seismic example.

The NRC picks a design basis earthquake, remember this it's an initiating event, and then they make sure that everything that has to respond meets the industry codes, and if each item meets the codes you get your license, okay.

Now some of those are overkill, some are about right, but there isn't -- By the way, nowadays with REG Guide 1208 you pick the design basis accident in a probabilistic way, which wasn't true before, before it wasn't but now we do for the new ones, but there is no recognition of accident sequences in any of our regulations.

I'll just -- Let me go on, just one more thing. If you look at the seismic PRAs, and I have about 40 of them on my shelf, that's a lot of what I do, more or less half of the important accident sequences consist only of seismic failures.

After the earthquake it fails this, it fails this, it fails this, it fails this, and you get a core damage. The other half, somewhere along the way in that sequence is a human error or a non-seismic failure.

You know, the diesel doesn't start, it's not a seismic failure, it's just, you know, one time in 300 it doesn't start, something like that. Half the accident sequences that matter in the seismic PRA have non-seismic failures and human errors and nothing the agency's regulations recognizes that at all.

It's absent in our regulations at all, except we regulate human error and we regulate the reliability of diesels, but not in the context of the accident sequences that matter after earthquakes. Why? You just don't do it.

Is that sensible? Well, I want to tell you they are really, really safe, so that's a success. On the other hand, the regulations are out-of-date and they are inefficient, okay.

The reason they are inefficient is some stuff is overkill, some stuff is just barely, and there is no way to sort that out. Now I'm going to give you the promotional side, all right.

I went to an engineer who knows about the costs of the two double reactor sites that are being built today, you know as Vogtle and Summer, as you know, and asked how much does it cost to build those reactors for seismic compared if they were in Southern Florida where we don't have any seismic, you know, motion, and it's hard to estimate, but it's probably 10 percent or 15 percent.

Ten percent of Vogtle and Summer, two units is a $1 billion. If we could save a tenth of that that's only $100 million. We have regulations that haven't been revisited in which those costs are embedded and if we thought about them carefully maybe we could make the regulations just as strong as we need to, but take out some of that stuff we don't need.

And there may even be some places, by the way, that we have to strengthen, but without the use of these methods how can you know? Well I know, but the regulations don't know, okay.

It's just one example, there is a lot of them. You can think about them, too, okay.

MR. HUDSON: Thank you.

MODERATOR: Yes. Let me read one question that I got from the webinar. What is the next step for PRA in your view and is there any gap that has not been addressed?

DR. BUDNITZ: Yes. The ASME/ANS Committee has just produced a standard for PRA for low-par shutdown states for shutdown states, okay.

The number of our hundred units that have a shutdown PRA is about this many, it's less than ten. I'm not sure what the number is exactly, maybe Jeff Mitman in the back can tell us, but it's less than ten.

We know that those risks are not negligible. They are not as big as par, but they are not negligible. So we got to -- The industry has to do those, okay.

We know that seismic PRA produces risks at many of our plants that are important, not dominant, but they are important. We're just now, just now going to do seismic PRAs in about a little less than half the plants.

You know which half, the ones that have this extra energy. Well, I won't go into it unless you want me to. The other half aren't going to do them. Why? You tell me why, I don't know why. I actually know why, but I don't want to go there.

The agency put 2150 on the shelf, that's blunt, it's probably an exaggeration by a factor by 1.1, but if you don't mind my saying, the agency put 2150 on the shelf, all right.

Heck, the agency put Recommendation 1 on the shelf, okay. There is a lot still in front of us and we have the methods, we have the knowledge, and we have the people inside the agency and outside, okay.

So that's Bob Budnitz's personal view and it couldn't be more blunt.

(Laughter)

DR. BUDNITZ: Okay.

MR. LANE: Bob, hi, this is John Lane from the Office of Research.

DR. BUDNITZ: Hi.

MR. LANE: I have just a comment about in the mid-'80s one of the early implementations I think of the results of the PRA was the filtered, the hardened vent, rather, for the Mark-1 containment.

DR. BUDNITZ: Yes, yes, yes, yes.

MR. LANE: Was Steve Hanauer instrumental in pushing for the hardened vent initiative which was around 1987 if I am not mistaken?

DR. BUDNITZ: I can't remember. I do

remember something though about it. Bob Bernero started it in the BWR, he was the BWR Branch Chief, and then he went over to NMSS, he became the Director of the Division of Risk and Research and in that thinking in 1986 and '87 right after Chernobyl Bob Bernero, the late Bob Bernero, he died last year, what a loss.

By the way, Steve died a couple years, or three years ago, too, what a loss. Bob Bernero was in the center of that I remember, and I can't remember whether Steve was, okay.

MR. LANE: That seems like one of the early, that was one of the earliest implementations because it did look at the transient with the loss of decay heat.

DR. BUDNITZ: That's fair, although it was the late '80s and that was just about when I was ending my discussion. It was one of the things that came along in the late '80s, as did pressurized thermal shock, came a little earlier. That's fair.

MR. LANE: The other, you mentioned the ASEP, what that meant, that was the Accident Sequence Evaluation Project.

DR. BUDNITZ: Right, right, right, right, Accident Sequence Evaluation Program, it was the precursor program, that's right.

MR. LANE: And the other issue I just wanted to mention, on the IPE Program it's been criticized a little bit, but actually it was a pretty robust review that was done.

DR. BUDNITZ: Yes, it was. Yes, it was.

MR. LANE: The issue was really we were trying to drag the industry into doing their first PRA essentially across the board, and so we were just looking for vulnerabilities and so it was kind of a very high-level approach, but the actual in-house review was pretty thorough.

We had three very qualified contractor teams working with us and most of the active PRA people in the Office of Research were involved one way or another.

So it was a good effort and I think it's still referred to, surprisingly enough the IPE results are referenced occasionally even still.

DR. BUDNITZ: That's fair, but let me comment about the IPEEE. The seismic parts of the IPEEE, the fragilities work was great, but about half the plants decided they would go cheap and they did the systems part of the seismic PRA in-house rather than use the contractor they had used, because the IPE was all done with contractors, almost all the system stuff.

In about half of those systems analyses, you can go read it in that NUREG, I can't remember it's number, but half of those systems analyses in the IPEEE for seismic weren't very good at all, okay.

On the other hand, the fragility work was great and that was really where the vulnerability search was most fruitful, that's fair. By the way, then there is the fire part.

A lot of the IPEEE on fire used the five methodology, which was great if it was a brand new plant, but it wasn't so good if it was an old one that had vulnerabilities because the vulnerability search was spotty.

So there was a lot there, but it was, it left a lot to be desired in retrospect. That's fair. I mean that's fair. I mean I don't want to be unfair to them.

AUDIENCE PARTICIPANT: On Steve Hanauer, I thought he had left the agency by that time?

DR. BUDNITZ: Steve?

AUDIENCE PARTICIPANT: Yes.

DR. BUDNITZ: I'm not sure when Steve left. I don't know. Steve went to DOE later.

AUDIENCE PARTICIPANT: Okay.

DR. BROCK: Hello, Terry Brock from Research. Great presentations, thanks for the historical insight.

I was wondering if you could elaborate a little bit on the NRC Safety Goal Policy, specifically the quantitative health objectives where the latent cancer fatality and the prompt fatality was set at a 0.1 percent of the background.

Do you have any historical information on why that particular fraction of a percentage was picked? Were there other numbers that were picked? Either one of you that could answer that?

DR. BUDNITZ: The ACRS held hearing after hearing on that that illuminated all the opinions, but I don't remember why the Commission ended up where they did or how, but I do remember something that I said, which was where they ended up the reactors, the operating reactors were happy because they could all meet it, and that's for sure and that is so.

DR. BROCK: I guess I have some homework to do.

(Laughter)

DR. BUDNITZ: Yes. There's a whole historical --

DR. BROCK: I mean it's not based on first principles, it's more of a judgement --

DR. BUDNITZ: No, no, there was no --

DR. BROCK: -- of a low risk and --

DR. BUDNITZ: How safe is safe enough is always a judgement. There is an extended record there which has been retained.

DR. BROCK: Yes.

DR. BUDNITZ: I'm not sure.

DR. BROCK: Okay, thanks.

MS. HAYES: So, Barbara Hayes, NRO. I was wondering if either of you could share some comments about external events associated with flooding and PRA developments forward looking, backward looking, on that issue?

DR. BUDNITZ: Boy, you asked the right guy about that. In response to 2.1 if you are in trouble with seismic you've got to do a seismic PRA. If you are in trouble with flooding you don't have to do a flooding PRA.

This agency backed out on that. This was only a year ago. I was not happy and I expressed it inside, I'm one of your consultants, so I said that inside, but I don't mind saying it.

The idea you can't do a flooding PRA

hazard study is nuts. Of course you can if the state of knowledge has uncertainty it has uncertainty, but the state of knowledge is what it is.

And, therefore, I think the flooding PRA insights are still in front of us even after all this other work is done, okay. By the way, there is a terrific staff working on that.

I mean Fernando, who I saw this morning, and Shelby Pence (phonetic), these are top people, but the work coming into them isn't going to be as probabilistic either on the hazard side or on the response side as I think could have been done, all right. We're talking about river flooding and so on.

MR. REBSTOCK: Thank you. I am Paul Rebstock in the Office of Research, I am in Instrumentation and Controls. The question of regulating two accident sequences is kind of interesting to me.

DR. BUDNITZ: I agree.

MR. REBSTOCK: I see two divergent, or two slightly different viewpoints on it. For one thing, plants have a set of safety analyses that demonstrate that the plant is going to be safe.

That safety analysis is based on a set of scenarios that somebody picked out at some time. The nature of controls and of the electronic equipment that's used to do the reactor control and also to do the protection, SFAS and reactor trip system and so on, is very, is changing quite a bit.

And it seems to me that even the non-safety-related equipment has the capability of putting the plant into a condition that's not the same as what was traditionally analyzed for accident sequences.

DR. BUDNITZ: Yes.

MR. REBSTOCK: Another thing that happens is that you have a safety analyses that says that if this variable gets to this point and you take action then everything is going to be okay.

If it gets beyond that point we don't know what's going to happen because we didn't analyze it there. But a lot of times the accident analysis has a lot of conservatism in it and so there is room for the accident, for the issue to actually go beyond that limit. You just don't know how much you can tolerate.

I'm just wondering what do you think of those two, I'd like to hear thoughts on this?

DR. BUDNITZ: Well, you know, Tom Wellock spoke about the earliest philosophy, you started with Hanford. The earliest philosophy was we're going to make everything strong and we're going to add margin all over.

And that philosophy has prevailed right to this day, and we're talking about 60 years later, and that's the sort of thing you are reflecting on, too.

I am convinced myself that these plants are adequately safe for me, but that doesn't mean the system is as rational as it could be, okay, because that approach, which he talked about, they did it at that time because they, if you don't mind my saying they were ignorant of the details but they wanted to proceed so they added all this extra stuff.

That has remained with defense-in-depth, meaning there is, more than one thing has to fail and that sort of thing, that has remained the appropriate hallmark of regulation, and I think it's so.

I don't think you should regulate with accident sequences. I think you should regulate the way you are, but I think you should use the insights to see where those regulations don't have as much margin as you'd like and where there is so much margin that you can relax them and still be okay, that's a different point.

So I think what he described was the original philosophy is in my view valid and should remain because I don't see how a designer can design accident sequences.

A designer has to design a pump, or a designer has to design a sheer wall, or a designer has to make sure that a system, the spray removes the chemicals or whatever.

So you need that specificity for the designer, but the regulations need to be formulated in light of that information, and that's a different point than regulating accident sequences, okay.

MR. REBSTOCK: Thank you.

DR. BUDNITZ: Does that help?

MR. REBSTOCK: Yes, thank you.

DR. BUDNITZ: I think that original approach was not only the only thing they could do but it remained so.

MR. SIU: I'll take it. Tom, I guess I was surprised to hear my name up front. This is Nathan Siu, Office of Research.

DR. BUDNITZ: What's your name, Nathan?

(Laughter)

MR. SIU: So just getting back to the history aspect, and, Tom, you talked about these incredible ranges of very small numbers resulting from some of the early estimates and you pointed out how we got our technology from the aerospace industry.

Now given knowledge of the technology, and the fault tree analysis I'm talking about, given the knowledge of that technology and knowing that treatment of common mode failure at the time was probably primitive, if they did anything at all, I'd imagine that the aerospace guys also were coming up with very small numbers making very large ranges.

Have you in your readings come across criticisms of that and how they dealt with it, were they actually using these results in their decision-making processes?

DR. WELLOCK: Well I mean the story line is, of course, that aerospace, NASA, eventually started backing away from use of fault trees and the like because they were unimpressed with the numbers.

They found that they were too unreliable and that was, you know, that was an argument made during the period when WASH-1400 was controversial. Why are you using this methodology when NASA has abandoned it is the story that is told.

So, yes, and, of course, the question then comes after the Challenger disaster, why isn't NASA using it, you know, and so -- But at the time, no, I mean NASA argued that their technology was too unique, that it changed too much between launches to actually make valid use of it over a long period of time.

So typically though NASA was held up as the exemplar, you know, as the wise, and this is among critics of WASH-1400, they held up NASA as being the logical approach and that WASH-1400 was flawed as a result.

So, no, in this period typically NASA was not under criticism, that comes later.

DR. BUDNITZ: Right.

MR. EAGLE: Gene Eagle, Instrument and Controls. I just wondered at this point in time if you would like to make any, share some of your experience or ideas concerning Fukushima and the advance and any probabilities and things that, just, you know, if you would care to make any comments on that?

DR. BUDNITZ: No.

MR. EAGLE: Thank you.

DR. BUDNITZ: No.

DR. WELLOCK: I'll stick with the history.

DR. BUDNITZ: No. Well there is one thing that you ought to know. As best we can tell if there hadn't been a tsunami the reactors would be running today because they did survive the earthquake with almost no damage after offsite power. We know that.

So if you are in the seismic community you'd think that that's a triumph until the water came, and that's everybody's, worth understanding, okay, because it was in fact way beyond design basis, okay.

MODERATOR: We've got a question from the webinar, or, actually we got a couple. You discussed reluctance to adopt PRA even today in the NRC.

What steps do you think would work best to push PRA into the codes and regulations? Any insights on that?

DR. BUDNITZ: Gee.

MODERATOR: Because we've got a comment here, the seminars on the WASH-1400.

DR. BUDNITZ: Hmm?

MODERATOR: Just got a comment. The seminars on WASH-1400, just joking around.

DR. BUDNITZ: Gee, that's a hard -- Well I said five minutes ago the insights from PRA can help us understand which of our regulations are fine as they are and which could require some updating, both technically and also in terms of the margins one way and the other, okay.

You know, to me that's apparent all over the place. By the way, just to give you the example, the new regime under NFPA-805, which many of our reactors are in the process of adopting, isn't perfect, but it's way better than the old regime and it's an example.

But it's only one step in a long-term process I think. I mean that's just one example.

MODERATOR: I've got another question. Can you provide a comparison about the industry's perception of the NRC in the '70s and '80s versus today?

DR. BUDNITZ: It's hard.

DR. WELLOCK: Well the constant theme through the 1960s and certainly through the early 1970s was excessive conservatism. We have to find a way to remove that from the regulatory process.

And that was, keep in mind that was a view not only in the industry, but among other nations as well, the Japanese, the British, the Canadians, were critical of our kind of design-basis accident, or what they called a maximum-credible accident.

Typically I think the perception was is that the AEC was the most conservative entity out there I would say through the, certainly through the 1970s.

MR. MITMAN: Jeff Mitman with the NRR.

DR. BUDNITZ: Hi, Jeff.

MR. MITMAN: How much is WASH-1400 and PRA penetrated into the university, particularly the nuclear engineering programs?

DR. BUDNITZ: Well, there's hardly a nuclear engineering program out there that doesn't have a course on PRA that the kids take, excuse me, that the graduate students take.

And in a few of them you can actually write a dissertation under somebody, but for a lot of them that's as far as it goes, they've all been exposed to it, okay, which is great, okay.

What's happened in nuclear engineering in the country is something that I suppose everybody knows, is that we have less than half as many nuclear engineering graduate programs as we had 20 years, 25 years ago.

On the other hand, they are over-subscribed. I mean in Berkeley where I am the number of people wanting to, you know, undergraduate and applying to graduate programs in the U.S. is vastly greater than they can take, or the can accommodate, and they are almost all Americans, and so that's good thing.

But I don't think, Jeff, that there is as much PRA as I think the future 20 years, 25 years, hence is going to need from everybody, almost everybody, understanding this stuff.

I don't know if you've thought about it, too. We have training programs, as you know, you know perfectly well, to train early but mature professionals in PRA at our plants.

EPRI does them, INPO does some of it, there's that sort of thing.

DR. WELLOCK: Let me throw in one comment, I know you are asking about engineering programs, but you did ask about how universities look at WASH-1400 and PRA.

Outside of technical fields the view of WASH-1400 and risk assessment in general is almost universally negative, that WASH-1400 was -- The way you told the story that the Executive Summary was kind of pasted over the entire report discrediting the whole thing, that view still lives.

If you look at most accounts of WASH-1400 that go right up through Fukushima it is universally regarded I would say in the social sciences in a negative light with just, I can think of a few exceptions, but not many.

AUDIENCE PARTICIPANT: Most of your

discussion has been focused on large light water reactors and the ones we currently have operating are being built.

How does the work that's been done and the body of knowledge apply to the SMRs on the smaller still light water reactors, and beyond that to the non-light water reactors that we may be focusing on?

DR. BUDNITZ: Well that's an easy question to answer. Every SMR designer and vendor, every one, has a PRA in-house even in the earliest stages which they then keep mature and update as detail comes in. Each of them do that.

It helps them understand where reliability and redundancy and diversity matter and where enough is enough and it also helps them prioritize which accident sequences are the things that they have to keep track of and be sure are coped with.

So they are absolutely all being, and not just the water reactors, but the non-water reactors as well. I know that for a fact, I've talked to those people, and they're all using it.

Now there is something you should know. The ASME/ANS PRA Committee that I co-chair with Rick Grantham we wrote a standard years ago for light-water reactors at power and it's been out there for a long,

long time, I hope you know about it.

It's endorsed in Reg Guide 1-200. We have recently, recently, two years ago, issued a standard for non-LWR PRA that would apply to gas reactors, sodium fast reactors, lead reactors, and the like, and it's now being used in a half a dozen places.

We issued it for trial use because we want people to give us feedback about it, it's now being used in half a dozen places, three or four of them overseas and two or three of them here.

So these methods are being used in the earliest design stages for thinking through where the liability, redundancy, and accident sequences generally matter most, okay.

It's not required by regulation in the earliest stages, but they are sure doing it, okay.

MODERATOR: Another question from the participants in the webinar. How involved was the industry and the public on the development of guidance and regulations as the stakeholders in the 1980s?

DR. BUDNITZ: Oh, not very much at all. The only involvement outside the industry itself were the, what I'll call the anti-nuclear people, and they had some influence, to their credit, because they had some insights that were important, but it's not very much.

MODERATOR: Okay.

DR. BUDNITZ: I mean anybody else want to comment about that? I just don't think that was true.

AUDIENCE PARTICIPANT: How about the risk of software as it's being implemented? Digital I&C, it's a problem we are wrestling with today in terms of 50.59 and assessing changes.

They want to adopt mods that go digital and install software and they have to assess no more than a minimal increase and the probability of failure and, frankly, the state of the art in terms of determining what the likelihood of a software failure is, it can't be determined, it can't be calculated.

Do you have any assessment in terms of advance reactors and how that technology is and how that's incorporated into PRA?

DR. BUDNITZ: I myself don't know much about that except that, clearly, we've all been going slow, and appropriately slow, with the operators that are running. I just don't know much about that.

AUDIENCE PARTICIPANT: Okay.

DR. BUDNITZ: Anybody else?

MODERATOR: Another question from the webinar? I understand you to say that Rasmussen came up with probabilities for accident sequences but had high levels of uncertainty.

How can one use uncertainty results when they vary by many orders of magnitude when the bounding values cover both extremes?

DR. BUDNITZ: Well they don't -- They are not many orders of magnitude, but they are big enough, so you have to take decisions accounting for them, okay.

Okay, I've got an example for you, all right. I want you, everybody here to estimate my height, and I bet if you did the range would be plus or minus four inches, maybe plus or minus two or three.

But there isn't anybody in the room that has any concern that I can go through that door standing up, right. But if I was 6'10", I was really 6'10" and you -- Excuse me, if I was 7'3" and you asked me that question and you tried to estimate, we wouldn't have as much confidence would we?

We can make decisions in the face of uncertainty, we do it every day, but we have to understand what the context is of those decisions and I think in that context, we faced from the beginning with WASH-1400 major uncertainties and yet very important robust insights despite them, and that remains true today.

And one of the most important things that we learned in WASH-1400 itself is a whole bunch of accident sequences don't matter to anybody. You know, they're all 10 to the minus 8 and we don't have to worry about them.

And that turns out to be as robust today as it was and that's really, really important, okay. I mean so, you know, I don't' have any problem making decisions in light of uncertainty, you just have to stare it in the face.

MODERATOR: Thank you.

DR. WELLOCK: I was --

MODERATOR: Oh, sorry, Tom. Were you going to add anything, Tom?

DR. WELLOCK: Actually I wanted to ask Bob a question about an event I had once asked you about on the phone. It goes back to this question of uncertainty.

One of the key things that influenced the Commission's Policy Statement about WASH-1400 came during Harold Lewis's presentation.

DR. BUDNITZ: I remember, I was there.

DR. WELLOCK: Yes, which is why I want to ask. He said that he thought that the error bands were so large that you couldn't use WASH-1400 numbers in a relative or an absolute sense.

DR. BUDNITZ: I know. And after he said that Lowenstein (phonetic), Kautz (phonetic), and I jumped all over him in the break.

DR. WELLOCK: Yes, because that became a major point of debate within the Commission afterwards.

DR. BUDNITZ: Yes, it was. Yes, it was. Yes, in fact we didn't need that. If you don't mind my saying, I don't think that in retrospect when he read what he had said in the transcript that he agreed with what he had said, but it became kind of a calling card for people that thought that it was useless.

The fact is, sure the uncertainties are large, and they were larger then, but like I said we've got a whole bunch of accident sequences we know we don't have to worry about. That was really important by itself.

And we learned that small LOCAs were important, didn't we? People in this agency, a lot of them didn't, they though you cut the large LOCA it's okay. We learned that wasn't so.

So a lot of it is not the numbers, but

it's the configurations and, yes, I do remember that, okay.

MR. HALVERSON: I may make my question a little bit more specific since you mentioned you didn't know much about software. Derek Halverson, Digital Instrumentation and Control.

DR. BUDNITZ: Say that again. Say it again.

MR. HALVERSON: Okay, I'll get to my question. What do you think when you start seeing odds that are extremely low, at 10 to the minus something you just start becoming uncomfortable?

DR. BUDNITZ: Yes. Well some things really are low, okay, and you can't get me to say I don't believe numbers at 10 to the minus 12. I can give you some things at 10 to the minus 12 that are really true, but you have to be skeptical.

By the way, my famous 10 to the minus 12 is what's the probability that an egg you ate this morning had salmonella? I can't remember this in detail, but I think it's 10 to the minus 12. You can go work it out, all right.

We have 3 times 10 to the 8th people in the country, we eat one egg a day, that's 10 to the 11th, all right, but there is more than that because of the eggs that the industry uses in cake and salmonella is pretty rare.

The probability that the egg you ate this has salmonella is 10 to the minus 11, 10 to the minus 12, and that's absolutely robust. So there are some numbers I believe and others that you really have to be careful with, all right.

MR. HALVERSON: Thanks.

(Off microphone comment)

DR. BUDNITZ: Well it's the egg on the table, okay.

MODERATOR: So we got one last question from the webinar and then we'll go to James and we can call it a day. It seems like it is quite labor intensive to create each different PRA model.

DR. BUDNITZ: Yes.

MODERATOR: Parentheses, shutdown, operating, seismic, fire, and future unknown accident. What would be the ideal situation, or is there an idea of the situation for making it easier to describe these actual systems in PRA models? Is there some technology you wish would be invented?

DR. BUDNITZ: No, because they are all different. Now they're not all different, if you had a Mark-1 that had never been studied you could take half a dozen of the Mark-1 PRAs and you'd get a big jump, okay.

But the fact is they're all different. Why are they all different? Because the AE side of these reactors were designed and constructed all differently, we all know that, and that's where a lot of the differences are.

So there is no shortcut. I had mentioned before, I chair with Rick Grantham the ASME/ANS Committee that develops and maintains the standards for PRA and to meet those standards is not cheap.

We have thought long and hard about what we might do to see if there was an easier way to meet the standard and still have a PRA whose attributes are what you want, and it's not easy.

On the other hand, it pays for itself. Everybody I know who has got a PRA that they use all the time will tell you it pays for itself over and over and over.

You know, you can do a whole PRA and keep it maintained for years with a couple days outage and, right, we know that, and so that's the bottom line. You can't get something for cheap, although there are some efficiencies if you are smart.

MR. CHEN: James Chen, Office of Research.

Bob, you mentioned about the Office of AEOD, I think, that do the event inspection later was absorbed by research.

DR. BUDNITZ: Yes. That was right after TMI, yes.

MR. CHEN: Yes. And today the NRC doing the special inspection in Auckland, the inspection team already has a research staff participating, could you talk about what's the rationale that if want to append on the Office of the AEOD become part of research here and then the NRC's inspecting the way we do it as today we do it now?

DR. BUDNITZ: No, I don't think I can comment on that. I can explain why. I have got something in my day job that's in the middle of that and I don't want to get in the middle of that, okay, and it's not NRC supported work, but I just -- okay, if you don't mind.

MODERATOR: Thank you very much. I want to thank Bob and Tom for taking the time to come here.

(Applause)

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