Lucile M. (Lucy) Jones Oral History Interview

Interviewed by Heidi Aspaturian

2022 to 2023

Abstract

This ten-session oral history with seismologist Dr. Lucile (Lucy) Jones—visiting research associate at Caltech, former head of the Pasadena office of the United States Geological Survey (USGS), and founder of the Lucy Jones Center for Science and Society—offers an in-depth look at how a life in science became a mission to help build a more informed and resilient society. Widely recognized for her pioneering work in seismic risk analysis and public safety and for her ability to communicate fluently about both to diverse audiences, Jones has long been known to the public as "the earthquake lady," a distinction that, as she explains, she often views with distinctly mixed feelings.

Sessions 1–3 explore Jones' Southern California upbringing, including her early love of mathematics, science, and classical music, and her unorthodox academic path—majoring in Chinese language and literature at Brown University and then earning a PhD in geophysics at MIT under the guidance of future Crafoord laureate P. Molnar. With this background she became one of the first Western researchers to conduct earthquake studies in China after the normalization of U.S.-China relations in the late 1970s. She recalls her work with Chinese seismologists during a time of guarded liberalization, the impact of catastrophic quakes on Chinese society, the pitfalls of

earthquake forecasting efforts, and her newfound awareness, arising out of these experiences, that "earthquake prediction was not just a scientific problem, but a human and a social one."

Sessions 4–6 delve into her research on earthquake probabilities, particularly her use of statistical modeling to analyze foreshocks and aftershocks during California's 1987–1999 earthquake sequence. She discusses her collaborations with colleagues at the USGS and Caltech seismological laboratory, major advances and debates in seismology during this era, and her own growing prominence as both earthquake "explainer" and seismic safety advocate, and the challenges and opportunities it created, both personally and professionally. She also recounts joint research with her husband and fellow seismologist, Egill Hauksson, on seismic activity along the southern San Andreas and adjacent faults. These experiences culminated in her authorship of "Putting Down Roots in Earthquake Country," a guide to seismic awareness in California.

Sessions 7–9 cover Jones' tenure as scientist-in-charge of the Pasadena USGS and her move into public policy, working with government agencies at all levels to develop and promote hazard mitigation strategies. A highlight is her conception of the ShakeOut earthquake drill, now a global event. She talks in detail about her year as seismic safety advisor to the mayor of Los Angeles, which led to the Resilience by Design report—a comprehensive blueprint for bolstering the city's ability to withstand a catastrophic quake—and about her decision to establish the Lucy Jones Center, reflecting her commitment to "science activation," with the climate crisis as a key focus. A recurring theme is the potential tension between scientific integrity and public advocacy, and the challenges of working within and across both worlds.

In Session 10, Jones discusses her experience researching and writing her book The Big Ones, a popular science treatment of how societies throughout history have responded to natural disasters, the lessons they offer for meeting similar challenges today—and the lessons she herself learned through her explorations of how humanity has dealt, or failed to deal, with such events over two millennia.

Note to Readers

Oral history interviews provide valuable first-hand testimony of the past. The views and opinions expressed in them are those of the interviewees, who describe events based on their own recollections and from their own perspective. They do not necessarily reflect the views of the Caltech Archives and Special Collections or of the California Institute of Technology.

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Lucy Jones in 2018. *Photograph by Helen Berger*

Session 1, March 18, 2022

Family background in southern California & southern China; missionary & theological roots

Heidi Aspaturian: This is March 18th, 2022, interview session No. 1 with Dr. Lucy Jones. I usually start at the beginning and ask about your family background.

Lucy Jones: I actually was born in Santa Monica. My dad was an aerospace engineer and at the time he was at Point Mugu. Later he got moved up to Moffett Field in the Bay Area and then to Utah, and he said, "I'm not dealing with Utah!" He quit that job and came back to California. So I mostly grew up in Westchester in West LA, with an aerospace father who played math games with me.

Aspaturian: How about family history on your mother and father's side?

Jones: They're both interesting. Through my mother's side I'm a fourth generation Southern Californian. My great-grandparents met at a religious community in Orange County. You know, Anaheim means "blessed home."

Aspaturian: I did not know that.

JONES: They were not part of that one, but there were a lot of fundamentalist religious communities, sort of utopian communities, that formed in Southern California, especially Orange County. One great-grandparent was born here; the other came as a small child, and they grew up across the street from each other. Very fundamentalist. Then their daughter— my grandmother— rebelled against that and left home and went to UCLA, where she was in the second graduating class in Westwood. She probably graduated in 1922. She married my grandfather, whose family had settled Banning, and so we found that my great–great- grandparents and my great-grandparents were buried in the San Andreas Fault. [Laughter]

ASPATURIAN: How appropriate.

JONES: Yeah. There's an old cemetery up in Banning. The fault pushes up there, so there's kind of a plateau that looks over the valley, and it's really easy to dig there because the ground's all churned up. My mom used to take care of their graves. She was born in San Dimas and grew up on an orange orchard. Her father ended up working for LADWP [LA Department of Water and Power] and worked on the Colorado River aqueduct.

ASPATURIAN: He was an engineer for the aqueduct?

JONES: I don't know if he was a trained engineer. I realize I should have figured this out. He died before I was born, so I never knew him. My mother was born on the day of the San Francisquito Dam disaster.

ASPATURIAN: I'm not familiar with that.

JONES: That's a turning point in the history of the DWP. They built a big dam up in San Francisquito Canyon in the San Gabes [San Gabriel mountains], and they decided when it was under construction to make it taller; and they did, and it wasn't strong enough. Maybe a month after it was built, it broke. Something like 450 people died in the flood: It killed more people in California than any earthquake except 1906. It's said to have broken Mulholland. He died soon after. [William Mulholland was a civil engineer responsible for developing much of the infrastructure that brought water to the LA Basin in the 1930s. The San Francisquito Dam failed shortly after he inspected it and pronounced it safe. –*Ed.*]

ASPATURIAN: Oh, of course, *Chinatown*, the movie.

JONES: Yes, *Chinatown*. Since my grandfather worked for DWP, and my mother was born that day, I've wondered about the connection. I didn't realize it until after my mother was gone, so I never asked them about it. When we saw *Chinatown*, mom said that story of the man being the father of his granddaughter was true not of Mulholland but of his chief of staff, his right-hand man. That that's what she had heard growing up. So, yeah, I've got a lot of ties to Southern California. We marry our men and bring them home.

ASPATURIAN: Did your mother go to college?

Jones: She started college at the University of Tennessee. [*See also Session Two*] My grandparents—her parents—got separated, and my grandmother took a job as the assistant superintendent of education for Knoxville, Tennessee. She was quite a controlling woman and would not allow my mother to leave home for college.

Aspaturian: Interesting, considering her own history.

Jones: I know. The story about my grandmother was that she wasn't allowed to cut her hair growing up because of the family's fundamentalist religious beliefs. When she was at UCLA, she cut off her hair and passed out because of the weight coming off her head.

So that's the sort of stories we had about her. But she would not let my mother leave home; maybe because she knew what she'd done and didn't want to see my mom do it. So my mother started at the University of Tennessee, where she met my father, who was training as a fluid engineer.

Aspaturian: At the university.

Father's upbringing as "missionary kid" in prewar & WWII China

Jones: Well, no, at Northwestern. But he came down for work study on TVA, on the Tennessee Valley Authority. So, he was the child of missionaries.

Aspaturian: A lot of religion on both sides of the family.

Jones: Yes! Some embraced and some rejected. My paternal grandfather was a New Testament theologian at the University of Nanking [today Nanjing]. He and my paternal grandmother grew up in a farm town, Dodgeville, Wisconsin, which is now the home of Land's End [clothing company], and they went off to China in 1915. We have an oral history of my grandmother's where she says, "Francis came back from university and asked me to marry him and go to China. It's a long way to go, but it got me out of Dodgeville." So my dad and his two brothers and sister—

Aspaturian: They were all born in China?

Jones: Yeah, in southeast China in a town that they called Hinghwa, in the local dialect; in Mandarin it's XingHua. The region has its own dialect, basically its own language. My grandfather ran a high school there for fourteen years. When they came back to the States on sabbatical, he was working on his ThD.

Aspaturian: Theology degree.

Jones: Yes, Doctor of Theology. He got it in 1930, when my dad was a year old. So when they came back in 1930, they moved back to Nanking, and he took a professorship at the seminary at the University of Nanking.

Aspaturian: Did your father grow up fluent in Mandarin?

Jones: Only sort of, because they all lived in southern China, so the local dialects were different than Mandarin.

Aspaturian: Yes, they speak Cantonese down south.

Jones: There's like five different major ones. Fujian dialect, which is what's spoken in Taiwan; Xinghua is in Fujian province, but it's got its own version; and then there's Cantonese. In Nanking they would have mostly spoken Mandarin, but it would have been accented. Shanghai has another pretty separate dialect. But because it was the government, they would have been speaking mostly Mandarin around there. And he would have had an *amah*.

Aspaturian: A nanny.

Jones: A nanny. So they were poor; they were missionaries; but there was an *amah* for each child. They left when he was nine. They happened to go on sabbatical at the end of June 1937.

Aspaturian: Just as war [second Sino-Japanese War] broke out in the far east.

Jones: They were on a German ship going through the Indian Ocean when the Rape of Nanking began. A lot of their friends got caught up in that; a lot of Westerners in Nanking at the time went to Japanese concentration camps. But just by chance they missed it by two weeks.

Aspaturian: That's an amazing story.

Jones: It is. When they came back, they first went to Shanghai, where the seminary had re-formed. Shanghai was still an international community, so the Japanese didn't control it. It had been controlled by Westerners.

Aspaturian: Did they come back that same year, or was it '38?

Jones: A year later. 1938. My father spent two years at the Shanghai American School, but the seminary finally decided that they couldn't work with the Japanese; and so it dissolved and then re-formed in west China, and my father's family left to go there. By this point, my oldest uncle and my aunt were in college back in the States, but the next uncle, Uncle Phil, drove an old Dodge truck with my grandfather over "the Hump"—over the Himalayas—into west China to bring in supplies, and my dad and grandmother were flown in by the Flying Tigers. [The First American Volunteer Group (AVG) of the Republic of China, nicknamed the Flying Tigers, was formed to help oppose the Japanese invasion of China. *–Ed.*] A month after they left Shanghai, all the foreigners there were interned in concentration camps, so they missed internment twice.

Aspaturian: What province did they—

Jones: Sichuan. So the seminary reformed in Chengdu, but they were bombed a lot there by the Japanese, and the school was moved out to— I used to remember the name of the town; I think I've forgotten it.

Aspaturian: We can introduce it later.

Jones: Ren Zhou. My dad always struggled to say it. And so then he went to a Canadian school in west China, about 60 miles south of Chengdu, for another few years, but in '43, the school was bombed and shut down, and he was sent out to India to a British private school, which he hated. He lasted for six months, then went down to the mission board in Bombay: "I'm a missionary kid; I want to get back to the States."

Aspaturian: How old was he?

Jones: Fifteen.

Aspaturian: Fifteen, okay.

Jones: His parents were still in China, but he got put on an American troop ship and sent back to New York. Quite a story of arriving in New York, completely guileless, and some man at the train station offered to help him. What's the central station there—not Union Station.

Aspaturian: Penn Station.

Jones: Penn Station. He took him home and went, "I can't do this. Kid, I was trying to seduce you; you are too naive." [Laughter] He couldn't do it, and he got my dad back on the train down to where his older brother was living in D.C. Lots of good family stories, right?

Aspaturian: Yes. Good to capture them all.

More "good family stories": theologian, missionaries, Sinologists in OSS & CIA

Jones: And then dad went to school for a while in D.C. My grandparents left China a few months later. My grandfather got typhoid, and my grandmother had— something else went wrong. So the Flying Tigers flew them out, and they offered to take them all the way to Miami. It was like, "No, no, we've got to stop in India and pick up our son." Who had already left or the States months earlier. But they were able to find that out quickly enough to be able to get back to the plane. It took three days to fly from Chengdu to Miami in the middle of World War II.

And so my dad got back with his parents. They were in New York for a while. My grandfather was at Garrett [Garrett Evangelical Theological Seminary]; no, it was Union Theological Seminary in New York. Garrett's in Chicago, and that's where he got his master's. We realized that at the time they were there in '37, '38, it was with Bonhoeffer. I don't know if you've heard of him.

Aspaturian: Dietrich Bonhoeffer?

Jones: Yes. [Dietrich Bonhoeffer was a prominent German minister and theologian and outspoken anti-Nazi, who was executed by the regime in April 1945. –*Ed.*]

Aspaturian: Did he have classes with him?

Jones: They were both postdocs at the time. I don't know if he had classes with him, but he definitely seems to have had some interaction with him; I'm not sure to what extent. There's a lot more stories, but it would take up way more of this time. If you want me to, I can keep on going.

Aspaturian: If you have a couple of others that you think are worthy of recording, I'm perfectly happy to do that.

Jones: This one is actually an answer to your question, Did he speak Chinese? He had picked it up obviously as a kid. But he left Nanking when he was nine and went to Shanghai, which is a different dialect, and then he was in Sichuan, which is another completely different dialect. So he insisted he couldn't speak it. Years later, when I was studying Chinese and came back from Taiwan, we were at dinner having spareribs, and somehow my dad managed to cut a rib in a way that made it flip out, and there was barbecue sauce and the spare rib—

Aspaturian: Landed in your lap?

Jones: Landed in his lap, and he swore.

Aspaturian: In Chinese.

Jones: In Chinese. I won't repeat it here. I wouldn't say it in English, and I won't say it in Chinese. It was foul, and I was like, "Dad!" It was the first time that he realized that anybody understood Chinese. But I realized—you grow up the child of missionaries—you don't learn to swear in English.

Aspaturian: [Laughter]

Jones: So whenever he was really surprised, it would come out in Chinese. I also discovered over time that if I said something to him in Chinese where he didn't notice that I said it in Chinese, he could respond. So there was a deep level.

Aspaturian: It was ingrained.

Jones: It was in there. I'm sure if he had ever gone to study Chinese again, he would have spoken it well. Both his brothers did. Of course, they were older. Uncle Ed, who had already come back to the States and graduated from college in 1940 or something, went to work in the State Department as a China expert. Uncle Phil graduated from high school in China, I think, in '42. Later we realized he had Asperger's. Very highly functioning; he just didn't really care what anybody else thought. He took off with a Chinese army battalion and hiked up into Tibet catching panda bears. He spent six months basically wandering around western China in 1942.

Aspaturian: What did they do with the panda bears?

Jones: I don't know; I never did figure that out. And I'm not even completely sure about the panda bears. There's also a story about panda bears with his Dodge truck coming over "the Hump." Really, I'm not quite sure where all the panda bears come in; I know they were part of it. Anyway. Uncle Phil spent all this time wandering around China, gets back to the States in 1943—he had arrived a few months before my dad—and my poor aunt, the sister-in-law who had married Uncle Ed, goes down to meet Uncle Phil at Union Station in D.C. in January 1943, and he shows up in shorts and a pith helmet.

Aspaturian: But no panda bear.

Jones: But no panda bear! She was so embarrassed that she refused to go meet my dad when he came in because she wasn't at all sure what he was going to look like. [Laughter] Again, that's the family story that got told. But Uncle Phil ended up getting drafted and then when they discovered that he had a pretty decent knowledge of Chinese and had hiked all over western China, they sent him to the Monterey language school.

Aspaturian: Oh, yes. I'm familiar with that. My dad taught at the Naval Postgraduate School up there for a number of summers; he was a Sovietologist. But this is about you, so go on.

Jones: This is the same idea, right? Sinologist. Both my uncles ended up being very much in that. Uncle Phil was drafted into the OSS [Office of Strategic Services] and worked on planning the invasion of China—how to take China back from the Japanese, which didn't have to be used after the atomic bomb. And then he ended up in the CIA because he got recruited from there from the OSS.

Aspaturian: What a trajectory!

Jones: Yeah, all the family's pretty interesting that way. Uncle Phil would never talk much about his work; he was pretty careful about all of that stuff. He married a woman who had been a codebreaker for the NSA [National Security Agency]. They both lived at a boarding house for spooks in the early 1950s. But the only time I ever heard Phil talk about his work at all was when one of my cousins was talking about dropping the atomic bombs on Japan, and he rather vehemently said that they saved lives. So many more Chinese would have been killed in the retaking of China that it was actually a way to save lives. He felt that extremely strongly, and since he was actually planning the invasion, there's some data there.

Aspaturian: I imagine so.

Jones: So we have this family that's all connected to China. And both my uncles were Sinologists for the government—CIA and State Department. My aunt didn't marry; she ended up going back and becoming a missionary in Taiwan and then later in Hong Kong.

Aspaturian: It had a very strong pull on your family.

Father's engineering education & return to China; mother's background; parents' marriage

Jones: Out of the whole family, my dad was the only one who wasn't working in Chinese. He decides he wants to be an engineer because he felt that the way to help China was to build the Yangtze Dam. He saw all of the deaths each year as the river flooded. He apparently told his parents somewhat early on, "I'm going to be an engineer," and they're like, "Whatever that is; okay." Because this is a very humanities-focused family.

Aspaturian: Yes.

Jones: So he went off to college—I guess he managed to complete high school. He didn't actually take all the classes, but he got the degree when he was sixteen.

Aspaturian: Do you know what year this was, roughly?

Jones: 1944. He was born in 1928, so he was sixteen. And he enrolled at Northwestern in engineering.

Aspaturian: He decided on engineering just as the outcome of World War II focused enormous new attention on the sciences.

Jones: Yes, but he would have made this decision in about 1940.

Aspaturian: But I mean, his timing was good to go into the field.

Jones: Oh, yes. Absolutely. He had decided he was going in as a fluid engineer because he was going to work on the Yangtze Dam. And he went down to Tennessee to work on TVA for his work study. So he had started at Northwestern in '44 at sixteen, did four quarters, and in the summer of '45—he likes to tell the story—he was taking chemistry when the bomb was dropped, and suddenly eight elements were added to the periodic table.

Aspaturian: Suddenly it seemed really relevant.

Jones: And then being the practical engineering type, he calculated things out: The war was still on, GI benefits were still there, fighting had stopped. So he enlisted and spent eighteen months in the occupation army in Japan, being a clerk, a secretary, to a general. He then came back to the States and Northwestern in the winter of '47 and started doing work study with TVA.

Aspaturian: You say he clerked for the general; was he on [Douglas] MacArthur's staff? That would fit.

Jones: I mean, were there other generals there?

Aspaturian: I'm sure MacArthur had junior generals, but he was the one in charge.

Jones: Well, he was the clerk to a general; that was the way I heard it. The main reason I heard it was because he then asked for family leave to go visit his parents in Nanking. That caused a certain amount of consternation among the staff. But he did it. He went over and visited his parents in, I want to say, '48.

ASPATURIAN: The Communists took over that year.

JONES: 1949. October 1st, '49, is considered the start of the People's Republic. And this was in '48.

ASPATURIAN: It must have been tense.

JONES: It was tense. His parents were in Nanking, which was Chiang Kai-shek's headquarters, and it was taken in September of '49. So sometime before then, he visited. Actually it must have been '47—by '48 he has to have been back at Northwestern because my parents were married in June of '49. So he was doing a quarter at Northwestern and a quarter at TVA to pay for school.

ASPATURIAN: And that's where he met your mother.

JONES: And he met my mother by going over to the University of Tennessee. So my [maternal] grandmother, brought up in this fundamentalist family, was an atheist. My mother rebelled against her by becoming an Episcopalian. My dad's family were Methodists. Take this with a grain of salt: He said he went to the Episcopalian group on campus instead of the Methodists because the Episcopalian girls looked nicer.

Anyway, so they met, and both of them kind of gave excuses for why they fell in love and got married relatively quickly. For my mother it was, "My mother can't hold onto me; it got me away from my mother," who had never let her live at school. The story from my aunt was that she came to visit, and dad told her about this wonderful woman and how much in love he was. She said, "I think I've heard this before," and he said, "No, you don't understand; she likes me back!" [Laughter] And then he wrote to his parents and told them he was getting married. At this point, he would have been twenty.

ASPATURIAN: Very young.

JONES: Very young. They had both just turned 21 when they got married. My grandmother wrote back, "You're too young," and my father wrote back, "You don't understand; I wasn't asking, I was telling you." So my grandparents did not come back from China for the wedding. My dad's siblings were all there. My mother's father came out from Oregon to give her away, and then dad got mumps before the wedding. Which they then had to postpone.

ASPATURIAN: I would think so.

JONES: By that time, my grandfather had gone back to Oregon, and my mother's friend, who was going to be her maid of honor, was off on her honeymoon, and so my dad's sister was then the maid of honor. We have all these family pictures of this, so you're seeing everybody then. And they moved up to Northwestern, and he finished his degree and went and did a master's degree at Berkeley.

Aspaturian: That's how they came back to California.

Jones: Not quite, actually. They spent a year there while he got his master's and while he was getting it, the Korean War broke out, and they went "oops," and he took a job with Sperry Rand in New York, and they moved to New York.

Grandparents' departure from China in 1951

By which point my grandparents had left China in '51, so they stayed through the revolution.

Aspaturian: That must have been quite an experience. Did they flee to Taiwan with the Nationalists?

Jones: No, no, no. They stayed, and they were helping people, but the way I heard it was that they came to feel that they were doing more harm than good. They weren't actually thrown out, but people were getting in trouble for associating with them.

We have some of the relics or artifacts that they bought from Chinese friends who could no longer keep things like ivory chopsticks. Paying for such things was a way to give their friends money as they were leaving. So I have this set of ivory chopsticks and various other pieces mostly because neither my brother nor sister particularly wanted to hold onto them. So my grandparents came back to the States, and my grandfather taught at Drew University in New Jersey; there's a seminary there.

Dad actually almost died. He had been injured in China with something falling on his back and damaging his kidney, and then when he was in the Army there was a flu outbreak, and they gave everybody sulfa drugs. He got kidney stones from that, and there was more damage to his kidney, and it had to be removed. The interesting thing: The Army took responsibility for it, and he was classified as a disabled veteran and got a small payment every year for life. This happened when my sister was a small child, so my sister stayed with my grandparents and my mom flew out to Chicago.

Aspaturian: This is your older sister, I'm guessing.

Recalls first earthquake at age two; other early memories

Jones: Yes. Her name's Susan. So they got through all that. Sperry Rand moved them to California in 1954. I was born in '55 when we lived above the ocean in Malibu. One of those old houses where the surf came up under the house.

Aspaturian: Sounds very lovely.

Jones: Apparently. I, of course, don't remember a thing about it.

Aspaturian: At that time, was your father at TRW? Had he started there?

Jones: He was still at Sperry Rand. He was stationed at Point Mugu; that's why we lived in Malibu. Then we moved up to Ventura, where I felt my first earthquake.

Aspaturian: Yes, you talk about that in several news articles that I read.

Jones: It's a nice line.

Aspaturian: I actually looked into it. The probable date I've got is March of 1957.

Jones: Yes. I think so.

Aspaturian: Maybe it was today. Do you remember?

Jones: No, I should look it up. That would make a great little post, wouldn't it? Sixty years ago.

Aspaturian: I found this in the online *Santa Barbara Earthquake Catalog*. "Sharp booming earthquake; the shake being most violent in Ventura and Oxnard." I thought that must be the quake. Since I believe you say you were two.

Jones: I think it must be. I know I was in Ventura, and by the time I was three we had moved up to Sunnyvale. So it's right around that time. It's a very odd thing because I have very few memories of that house, but I have a very clear picture of the living room into the hallway and my mother taking us into the hallway.

Aspaturian: One news story has it that it was your grandmother. I was going to ask you about that.

Jones: No, no, it was my mother. I just have this isolated memory of being on the floor, squished up with my brother and sister and my mother on top of us, covering us up. So it was strong enough to make her— she would have been in the Long Beach earthquake.

Aspaturian: In '33.

Jones: In '33. So, having grown up in California, she definitely knew earthquakes.

Aspaturian: Do you have any memories of the aftermath, or is it just one of these bright, isolated memories from very early childhood?

Jones: It's that very early childhood where it's this picture.

Aspaturian: It's a flash, and everything else is dark.

Jones: It's a flash. I can see the living room through the door there and hear the cat screaming. So we had Siamese cats—you know that special yowl they have?

Aspaturian: Oh, yes.

Jones: That's the memory I have—of that Siamese yowl and my mom making us stay down.

Aspaturian: Was that your earliest memory, do you think?

Jones: I have another memory from that house of being about to get a cold-water enema because I had a fever of 106. That's another traumatic memory— of being put in this bathtub of cold water as they were trying to cool me down.

Aspaturian: I can see why that would stay with you.

Jones: I actually had multiple episodes of ear infections; hospitalized a few times when it got my temperature too high, and I finally had my tonsils out when I was three to try and stop it. My adenoids blocked my eustachian tubes, and when they took them and my tonsils out, all of the ear infections stopped 'til they grew back.

Aspaturian: That's very rare, as I understand it.

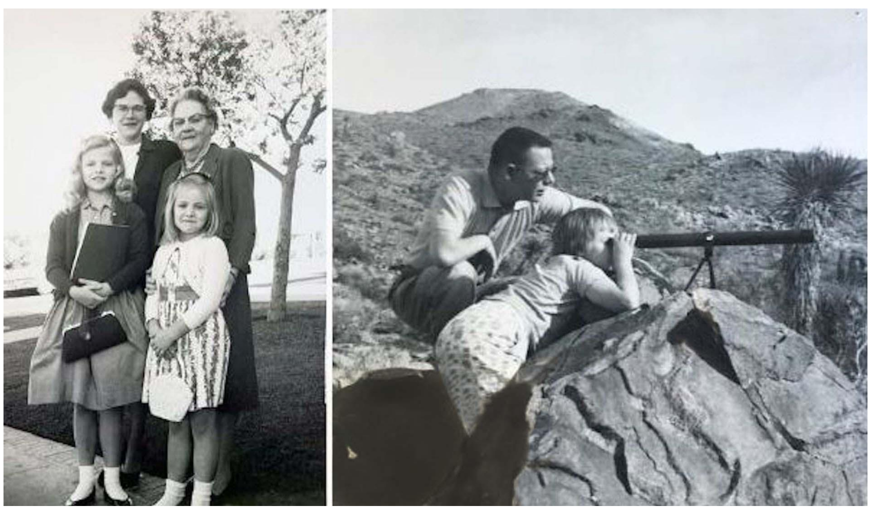
Jones: My mother's father had three sets of tonsils; so I had them taken out the second time when I was eleven, and that seemed to work.

Aspaturian: That did the trick—finally.

Jones: That did the trick. Basically, before we came back to LA the memories are much more isolated. When we were up there in Saratoga, I mostly remember my tonsillectomy and being in the hospital. Then we moved to Utah when I was four, and I have a few more memories out of there of the snow and such. And actually the nursery school which was at the Episcopal cathedral in Salt Lake City; I have quite a few memories of that place.

Upbringing in Westchester, CA; impact of father's work in aerospace & satellite development

And then we came back here and moved into a rented house in Westchester. My dad took a job there a company called Space Technology Laboratories, STL. TRW bought it up a few years later, so then he worked for TRW Systems Group.



Lucy Jones, about age 8, with older sister, Susan, mom, Patricia, and paternal grandmother, Lucile Jones, and at right, age 9, with her father, Donald Jones, surveying Lucerne Valley in California's Mojave Desert. *Photos courtesy of Lucy Jones*

ASPATURIAN: I guess he had to give up his dream of damming the Yangtze successfully because of what happened in China.

Jones: But instead, he worked on the space project.

Aspaturian: Aerospace, yes.

Jones: And I realize that probably had a pretty big impact for me because there was a feeling that you were creating a new world, being part of putting up satellites. He would regularly go down to Cape Canaveral, which then became Cape Kennedy before it became Cape Canaveral again. He ended up developing a specialty in balancing spacecraft, how to determine their exact center of gravity because if you want to apply force to get them to go to a specific destination, you need to know exactly where their center of gravity is, and their angle of rotation, to be able to apply the force correctly. So if he hadn't done his job correctly, the satellite wouldn't go where it was supposed to go. He ended up developing a machine for spinning them and making these measurements, and so he was needed whenever they needed to send up satellites.

Aspaturian: I see; he was indispensable to that part of the program.

Jones: So yes, when the time came that he started thinking about retirement, they convinced him to go part-time, because "We aren't sure what we need you for, but we want to have you around."

Aspaturian: How early do you recall becoming aware that your dad was involved in the space program?

JONES: I can't remember not knowing; I think it was really, really early. I don't think it would have mattered before kindergarten; you aren't really aware of your family outside of your family until then. But basically from almost as long as I can remember. And growing up in Westchester, we had this sort of cohort of students with aerospace engineer fathers; we were all in the honors classes; most of us were in orchestra. I was actually just at a funeral for the last of the adults from that cohort. All the fathers were aerospace engineers, and all of the mothers were at home, at least at first. The funeral was for Pat Russell, who ended up becoming city council president.

ASPATURIAN: The name is familiar.

JONES: In '69 she ran for city council and won, and so I would have been fourteen. My sister dated Pat's son for most of high school. So it was just part of our lives; all our dads were aerospace engineers because in Westchester you had Hughes Aircraft to the north and TRW to the south, and several other smaller aerospace companies around the area. And so you had this whole group of dads—they were almost all dads.

ASPATURIAN: Yes, in those days, that's right.

JONES: And then as their kids, we all sort of hung out together.

Father's computer programming work reinforces early interest in math & science

Aspaturian: Were you always interested in the sciences?

Jones: I just knew I was really good at math.

Aspaturian: That's often how it starts.

Jones: Right. And that made dad really happy. He would play math games with us, and I was the one who always wanted to do the math games. My brother and my sister were more likely to go, "Yeah, I want to play outside."

Aspaturian: Were you the middle child?

Jones: Yes. My brother's younger; my sister's older. We were all good at math, but we all did this thing where whatever one of us was really good at, the rest of us avoided. So both my brother and sister tended to avoid math.

Aspaturian: They gravitated elsewhere.

Jones: My sister was the really popular one, and my brother was the really good musician.

And so I stopped playing piano. My brother was a year younger and once he took up music was better than me in like three months. So I was the one who was good at math—it was sort of like my identity within the family. Dad loved it. I never asked my siblings what they thought of going to visit him at work because I loved going to see his office. He would take us there relatively regularly, and we'd see some spacecraft that was getting ready to be launched. We'd go and see the big manufacturing bays where these satellites were assembled or go and see the computer. A computer where you programmed it with a paper tape.

Aspaturian: I've seen *Hidden Figures*, so yes.

Jones: I've told this story a certain amount about how my dad wanted to explain concepts to me. I've seen it written up as him explaining how to calculate prime numbers, but he was actually showing me computer programming about prime numbers.

Aspaturian: I had read that and wondered how one would just mentally calculate prime numbers, but now what you're saying makes a lot more sense.

Jones: He wrote two different programs. One was a DO loop, all in Fortran. [Laughter] He taught me basically the concept of a DO loop at this point. You take a number to test. You divide it by every other prime number smaller than its square root, and if any of the results are an integer, it is not a prime number. Then your DO loop would test, is this bigger than the square root of that number and get out of the DO loop and it would write it down as being a prime number.

Aspaturian: I can see why this would fascinate someone who was naturally drawn to mathematics.

Jones: And then he had a different program that just took every number between one and a hundred, did two DO loops, multiplied them past each other, and made a matrix by outputting X when you got the answer, and then all of the remaining zeros were the prime numbers. The point being that the second program took much less time. Computers then were so slow that it would churn out the first few prime numbers up to a 100 or 1000 pretty quickly, and then it would really slow down because it had to do all of these calculations. You can imagine how slow that must be compared to a computer now! It was just so elegant that you could get all these answers so much more effectively.

Aspaturian: Yes, the efficiency of the algorithm.

Jones: The efficiency; he loved it, and he showed it to me because I'm his daughter, so I must like this too. Because he thought I should, I did. I think that that sort of thing— it was a place where my dad shared his love with me by showing me this stuff.

Aspaturian: It was a bonding experience.

Jones: It was very much a bonding experience.

“Women can’t usually do math, but you’re my daughter; you can do it”

And I’m certain that that’s what carried me through times when I was still facing a lot of pressure that “women don’t do this.” I told a story at his funeral that there was some point at which I heard him talking about Jenny. Jenny was an engineer at TRW and the only woman in the group, and he was talking with my mom about her and said quite admiringly, “Jenny thinks like an engineer, not like a woman.” And I’m like, “Dad! What do women think like?”

Aspaturian: That’s interesting.

Jones: He was like, “Well, you know, women usually can’t do math.” And I’m like, *What?* And he says, “*But you’re my daughter.* You can do it.” So he was very much in that culture, but *I’m his daughter, I’m the exception*. And I was thinking about it in connection with Women’s Month this month, and how when you’re the only woman in the class, is that a message to you that you don’t belong or that you must be really good? And I took it as I must be really good, because that’s the way my dad had taken it. It’s actually interesting; my brother’s been scanning old family photos, so I’ve recently been looking at whole boxes of slides and pictures from our childhood. And noticing in how many of them while we’re out camping, I’m sort of clinging onto my dad.

ASPATURIAN: Well, you clearly had a lot in common. What were your parents' names?

JONES: Oh, Donald Jones. My mom was originally Patricia Lehman. She would tell us about long-time American roots; and that anyone with the name of L-E-H-M-A-N spelled with only one N, was a relative because some seven or eight generations back there was a Hans Lehman, who came over and had fourteen sons.

ASPATURIAN: Fourteen sons!

JONES: In Pennsylvania. Pennsylvania Dutch, named Lehman, almost undoubtedly a relative, and they came in 1737, I think. Something like that. The Lehmans settled all over Southern California, mostly out in Banning and Redlands. One of my mom's cousins put together the family history and tracked them back.

ASPATURIAN: Fourteen boys. I think today a geneticist might be very interested in him.

JONES: That's true. I don't think there were any girls. I'm not sure about that.

Early reading interests in fantasy & science fiction; teaches self LOTR "Elvish"

Aspaturian: What kinds of things do you recall liking to read when you were growing up?

Jones: Fantasy.

Aspaturian: Science fiction and fantasy or just fantasy?

Jones: Mostly, I read just about everything.

Aspaturian: How would you define fantasy?

Jones: So, [J.R.R.] Tolkien and Edith Nesbit.

Aspaturian: Of course.

Jones: I remember reading all of those. There were some other ones. I have a very clear memory of exactly where they were in the local library and going down and getting the next book. I read *The Hobbit* when I was in fourth or fifth grade, and then it was like, "Do you want to learn more about hobbits? Find *Lord of the Rings*." Aha! But they weren't in the children's section; they were in the adult section. So I went and I got *The Fellowship of the Ring*, the first of the three volumes.

Aspaturian: Oh, yes.

Jones: But there's actually two books within each one.

Aspaturian: That I don't remember.

Jones: I got through the first half of *Fellowship*, which ends with the line—I clearly know this all too well—"Frodo was alive but taken by the enemy." And it was so scary with the black riders coming after him. I put the book back; I couldn't finish it. I walked away and it was over a year—

Aspaturian: Before you went back.

Jones: And then I wouldn't reread the first section. [Laughter] I started reading again after that part and read the whole thing. I don't remember how many times I read them when I was a kid. I remember going with my boys when the movies came out, and they asked me something like "What part of *Lord of the Rings* do you like the best?" "Darling, you don't understand; I taught myself Elvish, which is really just the characters mostly, and we used to send notes to each other in Elvish in class." It was at that sort of level. I didn't get into science fiction until I was fourteen.

Aspaturian: I read somewhere that your dad introduced you to the *Foundation* trilogy.

Jones: He got the *Second Foundation* series as soon as the last book came out and said to me, "The heroine is a fourteen-year-old precocious girl like you," and I read it to figure out what he was saying about me because I wasn't sure what precocious meant.

Aspaturian: What was her name again? I can't remember. It ended with a "y," I think. [Arkady Darell]

Jones: I don't remember. So I read the *Second Foundation* series first and then went back and read the others and then discovered [Robert] Heinlein. I don't know if you've ever heard of *Analog*?

Aspaturian: No.

Jones: It was a science fiction magazine. So a lot of those classic stories were published there—Philip Dick, [Robert] Heinlein.

Aspaturian: Arthur C. Clarke?

Jones: Arthur C. Clarke. Isaac Asimov. Ray Bradbury. There were lots of authors. So it would come every month. Sometimes it would contain a longer piece, like a novella, that was in sections in the magazine and then several short stories. My dad subscribed to it.

Aspaturian: So you read them as well.

Jones: Once I discovered the *Foundation* series and read through that, I started reading through my dad's collection of *Analog* magazines. I read some mystery novels, and I'd occasionally read other, more serious things, but I tended to use reading as my escape.

Excels in school "except for physical education"

ASPATURIAN: In school—well, of course you were with a lot of aerospace children, but were you kind of known as a math and science standout?

JONES: I was definitely. I mean, I never got a B. So.

ASPATURIAN: In anything.

JONES: Except for physical education. I got a C in that, and it always really bothered me that it brought down my grade point average. [Laughter] I started school in an Episcopal parish day school and went to public school in fourth grade.

ASPATURIAN: This was in Westchester?

JONES: All in Westchester. The priest was having an affair with a parishioner, and my parents got upset enough to take me out of the day school.

ASPATURIAN: Well, that's a religious education right there.

JONES: [Laughter] And also by this time my sister had graduated from the day school and was going to junior high school, and they discovered it was a perfectly fine school. So I got moved out to the public school in the middle of fourth grade. The teacher was really intimidated by how smart I was, and the school tried to move me into seventh grade.

Aspaturian: That's a big leap, fourth to seventh.

Jones: My parents wouldn't let them. But there was a program back then where they took the smartest kids from school and put them all into one class in a different school over at Paseo [today Paseo Del Rey Natural Science Magnet Elementary].

Aspaturian: So they not only tracked you; they put you in a different building.

Jones: Except a new principal had just come into my school and didn't believe in it, so she wouldn't send me over there. She wanted to just skip me to seventh grade, but she wouldn't send me to Paseo. So I actually remember at the end of all this being pretty miserable, arguing with the teacher that she had the definition of ordinal numbers wrong. I was right, but the teacher didn't appreciate it. All that. So that wasn't great.

Aspaturian: So you went through fifth and sixth grade mainstreamed at this Westchester school?

Jones: Right. I also started orchestra there—in fourth grade. And then I went into seventh grade at the Orville Wright Junior High School.

Aspaturian: Very appropriate.

Jones: It was Westchester, right? There was Airport Junior High and Orville Wright Junior High, because it was all surrounding

LAX. Then the kids from Paseo came in, and there were the honors classes.

"Music was always part of my life"; begins playing cello at nine

Aspaturian: In orchestra what was your instrument?

Jones: Cello. So, okay, here's the other part of my dad's family; my mom's, too. They were Welsh.

Aspaturian: I would guess from the last name.

Jones: And so, you know, the only question was which instrument you would be learning. My grandparents would come over for Thanksgiving or whatever, and grace would be sung a cappella in parts.

Aspaturian: Sounds lovely.

Jones: It was actually, except whenever I couldn't follow the alto line, I felt incompetent. So music was always just part of my life. I started the cello and cello lessons in fourth grade, and in junior high, the orchestra really became the center of my social group and also important musically: We had a *Mr. Holland's Opus* sort of inspiring teacher. There were a hundred kids in the orchestra, 150 in the senior band. And actually the first clarinetist in my class became the principal clarinetist at Hanover.

Aspaturian: That's impressive.

Jones: And the first flutist is the principal flutist of Cologne. And the principal second violinist was part of the original founding group of Turtle Island String Quartet. They're really pretty amazing. The first oboist became a studio musician. I was the first cellist, and we were all sort of this clique. It was, as I said, this social group, but we were also a group that played one of the Brandenburg concertos for the ninth-grade graduation. It was important; it was really fundamental to who I was.

Attends & graduates from American High School in Taipei, Taiwan

Then when I went to high school, I guess I was trying to take too many classes. I didn't have time to do the orchestra—it didn't have a very good reputation—and I was bored out of my mind. Somewhere around Christmas, I came home to my parents and said, "I'll finish out the year for you, but I'm not going back to that school afterwards."

Aspaturian: How old are you at that point?

Jones: Not quite fifteen. And their reaction was, "Okay, let's figure out what you're going to do." And I'm like, "*Really?* That was way crazy, what happened?"

Aspaturian: You were braced for an argument, and nothing came.

Jones: It turned out that one of my teachers had talked to them a couple weeks earlier, basically saying, "She's too smart to be here; this isn't going to work. You need to figure out some other way to be educating her." Actually, I went and took the SATs then.

Aspaturian: At that point? You were fifteen, sixteen?

Jones: I was in tenth grade, and my birthday's in February, so I guess I was fifteen by the time I took them. At that time, you could place into UCLA with just SAT scores. And so the default plan was for me to go to UCLA at fifteen. But my mom was really cool to the whole idea. So my uncle Phil at this point —

Aspaturian: The CIA guy.

Jones: The CIA guy was now in Vietnam, and his family—his wife and children—were in Taiwan in what was called a safe haven community, because, you know, you don't put kids in a war zone. There were these "safe haven" communities, and you could choose to live in Taiwan or Hong Kong or Tokyo, for example. So my mom wrote to my Aunt Jinny in Taiwan going, "Gaaahh!" And Aunt Jinny said, "Why don't you send her here?" I left then at fifteen and went and spent a year in Taiwan living with my aunt and uncle.

Aspaturian: Did you formally graduate from high school, or had you got your GED equivalency?

JONES: I graduated from the American school in Taipei. My aunt and uncle were only going to be there for another year. I looked at the requirements and realized that if I dropped P.E., which I hated anyway, and took an English class, I would have enough credits to graduate in Taiwan. So I went to the high school principal's office and said, "This is what I want to do," and he said, "Oh, no, no, you'll need more time than that." I told him, "You don't understand; I'm not going back to high school next year. The question is whether I have a degree or not." I talked them into it. So I graduated from the American School just after I turned sixteen.

"Loved" first in-depth exposure to Chinese culture

ASPATURIAN: What was that like for you? This must have been your first direct exposure to Chinese culture, despite everything in your family.

JONES: Right. I loved it. Being in Taiwan was really wonderful. I had a cousin, Steve, who was just a few months older than me; we hung out with the same people, and he also played viola. So when I was getting ready to go over there, he was like, "You gotta bring your cello, and we'll have the hard part of a string quartet." So we found two violinists and formed a string quartet. That experience actually shifted my music—discovering small ensemble music compared to orchestra. I just loved, and I can still remember, the pieces that we played there in concerts.

Aspaturian: Did you have much interaction with the Taiwanese themselves, or was it largely an American enclave?

Jones: Not a lot. It was mostly an American enclave. There's a big military base there. The CIA community there is pretty large. It was never called that. It was the "Army Technical Group"—civilian advisers to the military. We had our own street that was protected, and the street had its own bus. If the US government paid your school tuition, you got on that bus; if it didn't, you took a private bus. So I was going off to high school on this private bus because the family was paying my tuition, and my aunt was like, "This is really crazy," and she asked if I could ride the military bus. People realized there was a "CIA kid" standing out there, waiting for this other bus, and they didn't want that—that was a liability. But the only way to have me properly on the military bus was if my education was being paid for, so the CIA paid my tuition to make sure I didn't stand out alone on the street. My class was about 60 percent government-paid—these were mostly people associated with the war in Vietnam, who had their families in Asia. Probably 20 percent were non-government Americans—mainly corporate people and missionaries, I guess. And then 20 percent Chinese.

Aspaturian: Okay, so it wasn't all European. Or Caucasian.

Jones: Right. The son of the Spanish ambassador was there. There were some other nationalities. But the school was mostly dominated by the military kids. So it was a pretty different environment.

Aspaturian: And you were there for a year?

Jones: I was there for a year and then graduated. Actually, I did get a B—the one B I had in high school, and in physics, no less!

Taiwanese earthquakes & advanced calculus

Aspaturian: I'm trying to do the math in my head. You must have—

Jones: It was '71.

Aspaturian: I was going to say, you must have missed the San Fernando earthquake.

Jones: Yes, I was in Taiwan when that happened. I remember hearing the news on Armed Forces Network Taiwan and seeing the headline in *Stars and Stripes* newspaper: "Los Angeles Destroyed by Earthquake." We freaked out and tried to call my parents, and since it was right after the earthquake, we couldn't get a phone call through. It was February 9th, and February 13th was my birthday—I was turning sixteen. So my parents, having no trouble at all in the earthquake because they were in Westchester, were waiting for my birthday to call me. In between, I'm thinking that they were destroyed in the event.

I felt lots of earthquakes in Taiwan. It has about ten times as many as California. You would feel something every week or two. None of them were really big, but my aunt had this beautiful framed, solid-wood carving that had been my grandparents' hanging above the dining room table. That's quite a pendulum. So it was sort of our sign if you were asking, Was that an earthquake? You'd always go check the carving to see if it was moving.

Aspaturian: It was kind of your personal low-tech seismic reading.

Jones: [Laughter] Right, right. That's what it was.

Aspaturian: Did you have any foreshadowing experiencing these earthquakes that this might—

Jones: No. At that point, I know I'm really good at math, and actually Taiwan was part of the change in my thinking. I was taking precalculus that year, and I was ahead in math at that point, and I had an incredible math teacher, Mr. Do. I'm pretty sure, looking back, that he must have had political troubles because he was a brilliant man who should have been a university professor, and he was teaching high school. I don't know what the issues were.

Aspaturian: You think that perhaps he ran afoul of the *Kuomintang* in some way. [Also *Guomindang*, the Chinese Nationalist Party, which established an authoritarian government on Taiwan after fleeing to the island following the Communist takeover of mainland China. –*Ed*].

Jones: Yes. Within about a week of my arriving, he asked me to stay after class and told me that I was too smart to just be doing the regular class and that he wanted me to do my current class in one semester, and then we would do calculus in the second semester. He put it as my filial duty to my family to make the appropriate use of my abilities.

Aspaturian: Was this a new concept for you, hearing it said this way?

Jones: Hearing it said that way, yes. Given my relationship with my father, it resonated. So he took me through the first college semester of calculus in the second half of the school year. I would sit in the back of the class and work at my own speed and go and ask him for help whenever I had questions. I also discovered later that the cultural perception that women don't do math is a Western-culture perception. It's really not there in China. This wasn't a factor with him at all.

Aspaturian: He just saw your abilities, and that was that.

Jones: Right.

"The teacher accused me of cheating because girls didn't get scores like that"

Aspaturian: I have a note here about how when you were at high school in Westchester, there was a science aptitude test.

Jones: Oh, yeah. This was the year that was making me want to leave. You were required to take this guidance class to help you decide what career you were going to do. I remember thinking, *I have to do this instead of orchestra?*

You had to write a report on three different careers that you were considering. My sister rather famously did them on being a housewife, a whore, and a nun. Because she said that those were the only options open to women.

Aspaturian: And she knew so much about all three at that time, right?

Jones: [Laughter] Yes, she got in trouble for that one. So they had this aptitude test for science and math, and I scored a perfect score, and the teacher accused me of cheating because girls didn't get scores like that.

Aspaturian: Was the teacher male or female?

Jones: Female.

Aspaturian: She called you in and accused you of cheating?

Jones: She did it in front of the whole class.

Aspaturian: What did she say?

Jones: "How did you cheat on this? Girls don't get scores like this; how did you cheat?" And she made me take it again, in front of her. I got a perfect score again, and she did back off a bit.

Aspaturian: Did you tell your parents?

Jones: Yeah.

Aspaturian: And what did they do?

Jones: I don't remember.

Aspaturian: I can't imagine your father taking this in stride.

Jones: Except by that point, I showed her up, right? So then when she asked me what I was going to do with this ability, I said I was going to be a nuclear physicist. Which sounded like the hardest thing I could say.

Aspaturian: In those days, yes. Did she apologize?

Jones: No. What did I do? I left the school.

Aspaturian: And you went to Taiwan.

Jones: I went to Taiwan. That was part of the reason; maybe it was the trigger. I don't remember it particularly as the trigger. I hadn't really put the two together—I wonder if it was.

Circumstances surrounding decision to attend Brown University

So then I graduated from the American School at sixteen, but by then it was sort of too late to really do college applications. I had only applied to Stanford, and I didn't get in. I decided to go back home and spend a year working and then apply to college.

Aspaturian: Were you upset when you didn't get into Stanford?

Jones: Yes. Well, there was a mistake on a grade, and I couldn't work my way around it. So I decided to get a job, and— who would hire a sixteen-year-old? At that time, that was pretty hard. I ended up getting a half-time job at the high school that I used to go to, as an assistant in the math department.

Aspaturian: At the Westchester High School?

Jones: Yes. I was basically a TA [teaching assistant]. I did some tutoring, and I graded tests and made copies on a mimeograph machine of the different tests.

Aspaturian: Was it hard to leave Taiwan? It sounds like you really flourished there.

Jones: I think so. The other thing that happened was that there was a program in Taipei called the Inter-University Program for Chinese Language Studies. It was administered by Stanford, so it was usually called the Stanford Center. [In 1997 the IUP moved to Tsinghua University in Beijing, and the Taipei program was renamed the International Chinese Language Program. –*Ed.*]

The head of it for a year or two, or maybe just for a year, was the head of the Chinese department at Brown. And he was an old friend of my aunt and uncle; they'd both been in Taiwan in the 1950s. Their daughter was a freshman, finishing ninth grade, so she was maybe a year or two younger than me. So this was a family that I got to know well. When my aunt and uncle were getting sent back to the States, I got a tutoring job where I could make a certain amount of money, and it ended up that I stayed for another six weeks with the Wrenns. So I did put off going back.

Aspaturian: This was your first association, I would guess, with the Chinese language program in connection with Brown University?

Jones: I had actually decided to not study Chinese in Taiwan, because when would I ever use it? So I did French at the Taipei American School, keeping up with my high school French classes. But then Jimmy Wrenn, who was a wonderful person, actually sent me the application to Brown. I didn't ask for it; he sent it to me. I'd never heard of Brown.

Aspaturian: Brown had only gone coed a couple of years earlier when it merged with Pembroke, right?

Jones: Right. It went coed in '71, so it was about the time I was in Taiwan. And it was sort of second-tier Ivy League, and here in California, we'd never heard of it. You knew about Harvard, Yale and Princeton, but you didn't know about the other Ivies. I discovered it because Jimmy told me about it and then literally had the application sent to my house in California. I also retook the SATs, and I took the National Merit exam.

Aspaturian: The NMSQT.

Jones: So I was a finalist, and I said I wanted to study physics. That was in the spring of my senior year in Taiwan, and then I came back to California. Caltech was trying to recruit women at this point, but what happened was they sent the letter to Taiwan, and somehow on the way over or on the way back, it got sent by sea mail. It took like three months to reach me, and when I got it, it was open. It was clear it had been read by the censors in Taiwan. I saved the letter for a long time. I actually was looking for it recently, but I think I tossed it at some point. It said something like, "We want you to know we now have 22 women."

Aspaturian: "You could be one too."

Jones: Yeah, right. It was because I put down physics on the National Merit test.

Aspaturian: Sure.

Jones: I did apply to Radcliffe, and it really bothered me that I couldn't apply to Harvard.

Aspaturian: They hadn't merged yet.

Jones: They hadn't merged. And then Santa Cruz was my back-up UC.

Aspaturian: That's where my daughter went.

Jones: That's where my son went. My sister went in '69 and then my son Niels went in 2008. When I was applying, it was also the really interesting one. It was very difficult to get into. Now it's one of the easy ones to get into.

Aspaturian: Not as easy as it used to be.

Jones: Well, none of them are. So I got into both Radcliffe and Brown, and I was then working in my old high school's math department, and the chair of the math department—an avuncular sort of guy who would give me advice—said, "What are you thinking about?" I told him I was sort of thinking about Brown, because even though Harvard's Harvard, it wasn't really Harvard; it was Radcliffe. I said I already knew the Wrenns at Brown, which made it a lot easier to go east, and he gave me the advice that I really did need to go to Radcliffe because there was a better class of men to marry there. And that should be the most important factor for a young woman going to college.

Aspaturian: What did you think when you heard this? Do you remember?

Jones: Yeah: "I think I'm going to Brown." [Laughter] You know, maybe that was part of it, too. Getting through when it was harder, when there were fewer women. You had to not think what people thought about you.

Aspaturian: Yes, you had to follow your own drummer.

Jones: And it's my bit of the autistic spectrum, which runs through my family, that I could cut that concern out—that I didn't care that much about what people felt. I mean, obviously I was working for my dad's admiration, but the fact that others that I didn't care about would have thought one way or the other never sort of made it into my brain. I can say that looking back. At the time—

Aspaturian: You just didn't think about it.

Jones: That's right. It did definitely piss me off that I should be considering marriage as my primary goal.

Aspaturian: That your MRS was more important than your BA or BS.

Jones: Yes.

Aspaturian: How did your parents react? Did they have an opinion?

Jones: My dad thought I should go to a UC. Why spend all that extra money? It didn't give you anything.

Aspaturian: It seems that Berkeley would have been an obvious choice for you, even through Santa Cruz was the one—

Jones: I didn't even apply to Berkeley. I didn't want to consider it. But they were willing to have me go east to an Ivy, and I actually got a pretty good scholarship package. There's that agreement between the Ivies where they offer the same financial aid, so that your out-of-pocket cost is the same. I know it's been argued about, and I'm not sure whether it's been removed. It was the going to Radcliffe, not Harvard, part that bothered me. The other part was that I knew the Wrenns would be at Brown, and also that Brown had just adopted the open curriculum.

Aspaturian: Yes, I'm familiar with it.

Jones: It was called the new curriculum at that point, so there were no distribution requirements.

Aspaturian: Not that different from Santa Cruz in those days, actually.

Jones: And that's me finding my own way, and maybe that's the ongoing theme: I wanted to find my own path. My mom was supportive of whatever I wanted to do. And my dad was sort of like, "I don't understand why you're wasting money, but okay." One of the funny things, through, was that once I accepted Brown and rejected Radcliffe, I got mailed the whole package from Brown. Kids nowadays just get emails, but that big fat envelope arrived, right?

Aspaturian: Yes, I remember those envelopes. If they were thick, you knew that it was probably good news.

Jones: And so I filled it all out and sent it back to Brown. Six weeks later I got a letter from the Harvard health center saying, "We haven't gotten your health questionnaire back yet; here's another one in case you lost it." And then I got another letter from a Radcliffe dorm and then the Harvard marching band. We joked about it—how it was like Harvard didn't know how to deal with someone rejecting them; they couldn't believe it. And there was the temptation. Because one of the other things I discovered when I said "I'm going to Brown" is that nobody'd ever heard of it. You said "Harvard," everybody knew it—"Oh, you got into Harvard!"

Aspaturian: Right, instant name recognition.

Jones: And it did make me think, "Oh, maybe I could change my mind now." I'm so glad that I didn't. I went on to Brown, and the Wrenns were an incredibly important part of my being there as people I could talk to the entire time I was at Brown.

Aspaturian: I think we'll stop there.

Jones: You got me to college.

Aspaturian: Yes.

Session 2, March 23, 2022

Recap of mother's education & professional career

Aspaturian: You had wanted to address a question from last time.

Jones: Just that you had asked me about my mother having gone to college. I had said that she started, then left when she married my father, because she wanted to get out of Tennessee anyway. She had been a math major and then didn't end up finishing. She followed him to Northwestern and then to his master's program at Berkeley and then became pregnant, but eventually she did go back and get her degree in psychology—one year before I got my undergraduate degree. She was quite proud that she got her degree before any of her kids.

Aspaturian: Where did she receive the degree from?

Jones: UCLA. Her mother had graduated from UCLA; my mom did; my brother did; and my sister got her MBA at UCLA. But my mother ended up having a heart attack a couple of years later, so she never got actively involved in a career—a paid career, at least. She ended up becoming a spiritual director, doing a lot of counseling.

Aspaturian: Very interesting trajectory.

Jones: I sometimes think about it: My mom's a spiritual director; my dad's an aerospace engineer; and I'm out there giving earthquake information for comfort.

Religious influences: Episcopal Church; Social Gospel

Aspaturian: This leads into a question I was thinking about after our first interview, which was, Were you raised in a family with kind of an ethos of service?

Jones: Oh, absolutely. My dad became an engineer to help the Chinese people, and my mother brought us up in the Episcopal Church.

Aspaturian: Ah so, the Social Gospel.

Jones: Very much the Social Gospel, and actually my dad's father had a cousin who was murdered by Ford, during the labor problems in the 1930s. This was not something I knew growing up; we found out about it later, and my cousin actually researched it. But absolutely, service to others was part of my family upbringing, as an ethical, religious, spiritual obligation. So, yes. [LJ subsequently added: "Our relative was Lewis Bradford and he died in 1937. It was called an accident at the time. My cousin, Steve Jones, researched it and with the information he gathered, the Dade County coroner offered to open a murder investigation, but we didn't pursue it. Steve wrote a musical about it

Aspaturian: Did you have religious exposure growing up?

Jones: I grew up in the Episcopal Church, yes, and I still go. The Episcopal Church is a funny one because it used to be called the Republican Party at Prayer.

Aspaturian: [Laughter]

Jones: I mean, it was the church of the elite for a long time, but it also has become an extremely social activist church. Our priest when I was growing up was arrested in the antiwar demonstrations. My mother got arrested with him in a demonstration where they were celebrating a requiem mass for the Vietnamese war dead on the steps of the federal building in Westwood. She was quite proud of that. So, yeah, that's the framework; it was quite liberal, quite to the left as a matter of religious obligation.

Aspaturian: I wondered about that, considering the path that your career took and what you had to say about your family's missionary tradition and religious background.

Jones: It's a mixed bag with the family. Because of my mother's religious beliefs, my dad stayed with her church. He was also the baby of the family, sort of the easygoing one. His two older brothers completely left the church in a rebellion against their parents, and their sister became a missionary.

Arrival at Brown; Rhode Island a culture shock “in a bunch of ways”

Aspaturian: So, let us pick up where we left off. You had been admitted to Brown, and you had arrived on campus.

Jones: It was a big deal. I had taken the train to the East Coast once as a kid. We went out for a family reunion, and that was my only exposure to the East Coast. Like I said, part of the attraction of Brown was that the Wrenns were there, and Professor Jimmy Wrenn was head of the Chinese department, so he was somebody I knew, and his daughter was a friend of mine. The whole family were friends with my aunt and uncle. I actually drove out. My parents drove to the Grand Tetons, where we met my uncle. We moved my bags into his car and kept on going across the country. So I drove out the first time to my uncle’s home in Maryland and then took the train up to Brown, and the Wrenns picked me up. I spent a couple of days with them before going to the dorm. And they were good friends to me through the whole time. When I’d get stressed or something, there was an open invitation to walk over to their house. I could study there for finals. They’d give me a pot of tea and these incredible chocolate chip cookies that Jimmy would bake.

Aspaturian: Was Providence [Rhode Island] any sort of culture shock after Southern California?

Jones: In a bunch of ways. One was just that when you grow up in California in the 1960s, you always had fresh vegetables. In Providence at that time, you didn't have fresh vegetables in the winter. The whole food thing was a bit of a shock. Providence was also a big stronghold of the American Mafia

Aspaturian: Somehow I did not know that.

Jones: The Mafia owned every Italian restaurant in the city, and there was sort of this presence that people living there were aware of that just had me shocked. Then too, a lot of my Brown classmates had gone to prep schools and were mostly from New York. My roommate was from New Jersey—suburban New Jersey, outside of New York. So it was a pretty different culture. But it was also a place where *everybody* was smart. In the LA public schools I felt I was an outlier, and even partly at the Taipei American School, where there were plenty of smart kids, but rather than my little clique of aerospace engineer kids in LA, it was mostly military kids, and top military, or the family wouldn't have been stationed there.

And then to go on to Brown where everybody or some significant percentage of the class had been valedictorians or salutatorians, it was like, "Oh." In that sense it was just a relief not to have to think about how people thought of me because I was smart.

Considers math or physics major; settles on Chinese language & literature; joins Renaissance music group

ASPATURIAN: Did you enter Brown with the idea of focusing on Chinese studies?

JONES: No. I knew I was good in math, and as I said last time, I had this math teacher in Taiwan who pushed me ahead into calculus. I also did calculus with my dad., so I knew I was really good at it. I also knew that I didn't want to be a theoretical mathematician, so I listed applied math as my prospective major, because it used math, and I liked that [laughter], but without really having any idea at that point what applied mathematics was. That first year I took physics, a calculus class, first-year Chinese, and introduction to counterpoint, which is musical theory. So that was expressing the range of subjects that I was thinking about.

ASPATURIAN: That is a tough curriculum. Counterpoint might have been the toughest part of it.

JONES: Nah, the counterpoint was fun. The Chinese— I didn't study Chinese when I was in Taiwan, but I realized that I should have. Thinking about what I had picked up by hearing my father swear in Chinese and having spent time in Taiwan, and realizing I could have put all these pieces together—and why hadn't I studied it to do that? Also it was another connection with the Wrenns because Jimmy Wrenn was teaching me, and it was comfortable in that sense. And since Brown didn't have distribution requirements, I could really just pick whatever I wanted.

I initially said I would study applied math, and then when I started really looking at taking classes, I realized that maybe physics was closer to what I was thinking—I was taking the physics course for majors. That convinced me that I really liked science as opposed to math. At that point, the Chinese was really for my personal desire to learn a language rather than thinking it was going to be my major.

The counterpoint class ended up being the only music class I took—I was trying to decide what to do with music. I discovered I didn't like orchestra so much, and orchestra is a big commitment. I loved the ensemble thing, but how do you find that when you're not a music major? Then, in this counterpoint class I was taking, they let us know they were starting a Renaissance music group. This was at the very beginning of what's now called historically informed performance of early music—recognizing that what had become standard classical performance was not how things were played in the seventeenth and eighteenth centuries—and the start of some academic effort to try and recover how it was really interpreted and played originally.

So Brown was starting this group, and they had just hired a professor for it. I also played recorder, because everybody in my family played recorders, and we always had them for camping trips or whatever, and then you also learned to play whatever other instrument you chose. So I thought, "Well, I could take my recorder, and I'd at least have some music and maybe learn about it in the Renaissance. I went to this opening meeting and —"You're a cellist?!" We just bought a viola da gamba, which is the predecessor of the cello; would you like lessons? We'll give you lessons even though you're not a music major." They gave me the instrument.

So all the way through, after that first semester, I was taught by one of the founders of the Boston Camerata [an early-music ensemble] and New York Consort of Viols. She lived near Brown. I had three years—not my junior year, which was spent abroad—learning how to play the viola da gamba. That's the instrument I've kept with, and I played in that Renaissance group the whole time that I was at Brown. And then—I think this is sort of my schizophrenia—I started to think, Did I want to be in the foreign service? In Taiwan I had seen foreign service officers associated with the US embassy, and studying Chinese got me thinking about that.

Aspaturian: When you say Chinese, I assume we're talking Mandarin.

Jones: Yes. The second year of Chinese was actually two classes —one really focusing on the written characters and the other emphasizing speaking. If you were going to be a major, you could take both. So my sophomore year was physics, math, and Chinese, along with the Renaissance music group.

Aspaturian: You like to challenge yourself, clearly.

Jones: I think—in a way, I was obsessing. It's interesting; we now recognize that the autistic spectrum has a strong streak throughout my family, and I can look at this as sort of my bit of the autistic spectrum— I would obsess on things. Chinese is different enough, and it was hard enough that you either gave up or became obsessed. That's the way I like to say it: If I was going to do something, I was going to do it well. Sort of like Caltech, right? We only do the things we know that we can do well; we don't touch the rest of it. That was sort of my personal philosophy, the way I did life.

It also meant that I sort of had two camps. I had my friends from the Chinese classes, and then I had my friends from the science classes. It's funny how you end up not really knowing the other part of it. I remember running into someone at the physics building. I knew him as a musician, and he knew me as a Chinese studies student, and we were like, "What are *you* doing *here*?" I didn't realize that he was majoring in engineering, and he didn't realize that I was majoring in physics. But that was also a big part of Brown; that's why I went there.

Aspaturian: Did you discern any cross linkages between your studies in Chinese and, say, your studies in science? One tends to think of Mandarin as perhaps a more spatially oriented language in the way it's written.

Jones: Well, no, I'd say that's more artistic rather than spatial.

Aspaturian: More artistic, okay.

Tapei junior year abroad

Jones: I remember that my junior year abroad in Taipei, I actually studied with someone teaching calligraphy, trying to learn the artistic side of it. Not bad, but I thought, "Okay, I should figure out how to use Mandarin if I'm going to be a scientist." So when I went over to Taiwan in my junior year, I went to the Inter-University program for Chinese Language Studies [*see also Session One*], administered by Stanford. It was a consortium of something like seven universities, and the center was at the National Taiwan University. And the courses were all-around, starting with basic language capability, and then there were literature classes. I thought, "How do I get science into this?"

So I came up with the idea that in Taiwan I would audit second-year classical mechanics, which I had just taken back in the States, and now take it in Chinese and get the language part. But the textbook was the same American textbook that I had used at Brown; it was in English, not Chinese, and the technical terms were all in English. So I didn't continue with that; I just did Chinese that year. So, I really saw Chinese studies and science early on as separate things. I was just really interested in Chinese. It sort of caught up with all my family stories, and I had wanted to be able to go back to Taiwan, but it was still in some sense more of an intellectual exercise. I found physics more interesting than literature analysis.

Aspaturian: So you had already thought in terms of pursuing advanced studies in science while you were majoring in this very different subject?

Jones: Yeah, yeah. I was thinking that I was probably going to become some sort of scientist. The other thing I thought about, as I said, was joining that up with the foreign service. To be a science specialist in the foreign service is actually a big advantage.

Aspaturian: Yes, I'd imagine.

"Geophysicists get to play in the mountains and get paid for it"

Jones: So I was thinking of doing that. I really hadn't settled in. It was more that I was good at physics rather than I really liked it so much. So for my second year, there was nothing but physics and Chinese. But, at the end of my sophomore year, just before I left for Taiwan, I went to a brunch in my dorm. And invited to the brunch was a couple who both were geophysics professors, Terry and Jan Tullis. I happened to sit near them, and we ended up talking for a couple of hours. They were both in rock mechanics—mechanics is a physics term for how rocks behave under stress. And they were telling me, "You know, physics jobs are all making bombs. Geophysicists get to play in the mountains and get paid for it." There's another piece of family history that we really didn't touch on before—that growing up in California, our summer vacations were always camping and backpacking up in the Sierras.

It wasn't that I liked geology per se; I hiked through all of that granite for years without getting too enamored of it. But the idea that geophysicists get to play in the mountains and get paid for it sounded really good. They got me interested enough that I decided that when I came back from Taiwan, I would take a class in geology. I had to take two physics classes to make up for what I'd missed there, and then I didn't want to give up the Chinese, so I also took a Chinese literature class. The geology class was a basic introduction to geology. I went into it thinking, Do I want the foreign service, or do I want to go on in some kind of science—what do I want to do? And then I read the 900-page geology textbook in the first week because it was so fascinating. I couldn't put it down.

Aspaturian: Do you recall who wrote it?

Jones: Oh, yeah, *Earth* by Raymond Siever and Frank Press. Press had been at Caltech, and at that time was the chair of the geology department at MIT. And it was sort of this—"Ohh!"

Aspaturian: It spoke to you.

Jones: "*This* is what I want to be doing." Not quantum mechanics or trying to search for the edge of the universe, but the application of physics to interesting problems, to problems affecting humans, right? I used to say that I went into this field to predict earthquakes and to save the part of humanity that was silly enough to live near the San Andreas Fault. I was fascinated by it. So my second semester, I took a geology class with Terry and William Chapple, and they said I should go up and visit at MIT. They knew people there and set up an introduction for me to go and talk to someone there to see if this would be something that I would want. That was sometime in the winter of '76.

"Why don't we get you working on foreshocks & if China ever opens up . . ."

And Peter Molnar ended up being the guy at MIT that I talked with while I was there. He was an assistant professor at the time. He's become very famous on his own since then [*see also Session Three*], but he was hired basically because MIT wanted his girlfriend, Tanya Atwater; she's the creator of a lot of plate tectonics models for California. And he really wanted me to come. He talked to me about all the things they were doing, and then because I was fluent in Chinese, they were telling me, "There was just this earthquake in China with over 500 foreshocks. Why don't we get you working on foreshocks, and then if China ever opens up . . ." This was before Mao died. [Mao Zedong, "Chairman Mao," was the founder of the People's Republic of China—PRC—which he led as chairman of the

Chinese Communist Party from 1949 until his death in 1976. –*Ed.*]

ASPATURIAN: Mao died in '76, I think.

JONES: Yes, July 29th. No, that is the date of the Tangshan earthquake; Mao died in September.

ASPATURIAN: Maybe the shock killed him.

JONES: There's a whole story about that, too. But it was just like, "Oh, my heavens"; and this whole vision opened up to me that I hadn't really thought about. I mean, I guess I was thinking about doing a PhD. The end of my freshman year at Brown, my grandmother had died.

ASPATURIAN: Your mother's mother?

JONES: No, my father's mother. The other Lucile Jones—I was named after her. She had bone cancer. It was pretty awful. I had flown back to California the minute my last final was done, because she was dying. I got on a plane that afternoon and got into Claremont where they were living; it was very shocking because I think she weighed like 80 pounds by that point. I went to say goodbye to her, and she was struggling to be able to talk, and in fact the last thing she ended up saying to me was "You have your grandfather's brains; don't waste them."

ASPATURIAN: Her husband's brains.

JONES: Yes. And he was Dr. Jones, so that sort of stuck with me, and I was thinking I should be doing something with that. Obviously, it was not dominating my thoughts because I still took the foreign service exam. Then when I discovered geology, it was like, "Oh, this is it. This is how it all fits together."

ASPATURIAN: I want to step back to your Chinese studies for one moment. In 1972, of course, Nixon went to China. It was a bit of an opening. Did any of this impinge on your studies or affect your thinking? Particularly when you were in Taiwan.

JONES: Well, I was in Taiwan in high school when the ping pong diplomacy started.

ASPATURIAN: Yes, I remember.

JONES: The Taiwanese government was really upset. So there was quite a bit of tension at the time. There's a story here. The relationship of the American children living in Taiwan to the Chinese legal system there was challenging to the Taiwanese, because they were dependent upon American support. So, if an American kid got into trouble, they usually just sent the family back to the States; Taiwan's government didn't want to get involved in prosecuting an American minor. It was usually around drugs; there was a lot of marijuana at the time. But there was someone in our class who had a girlfriend. They slept together; her father found out and—

ASPATURIAN: Did not approve.

Jones: Did not approve. This was 1971, and he wanted the boy prosecuted for statutory rape. And so the family got sent back to the United States.

Aspaturian: This was a military family?

Jones: Military family, yeah, and that meant that the father's career just got sidelined and that punishment was going to be sufficient. Well, obviously, the boy's really upset. So from the airport, just before they get on the plane, he calls in a bomb scare to the American School. We—the kids—all knew what was happening. His friends knew that he was going to do this, and the word went around among the kids. But obviously not among the administration. So suddenly we were evacuated. My string quartet was playing, and we put down our instruments and left, and then they wouldn't let us get back in to get our instruments and the word went around to everyone about what was going on. There's a big American Air Force base down island in Taichung, and there was a bomb squad there that was on the American School campus within half an hour. Which then showed the Taiwanese that the Americans didn't really trust them, because they also had a bomb squad on call ready to go. This happened right after the ping pong diplomacy.

Aspaturian: The Taiwanese government was not happy.

Jones: The Taiwanese government was not happy at all about any of this. So it's something I remember very clearly. But the US government didn't officially recognize China until 1979.

Publishes *Nature* paper on earthquake foreshocks as college senior; admitted to MIT for graduate study in geophysics

I was back in Taiwan in '75 when Chiang Kai-shek died. So I was there for his funeral and all of that. But at that time, the mainland was still Red China. It was still behind the Bamboo Curtain—all those sorts of things where there just wasn't interaction. But in '76, so just as I'm applying to MIT, the National Academy of Sciences had established a Committee for Scholarly Communication with the People's Republic of China, and they sent a delegation of earthquake people there in the late spring or summer of '76 because of the Haicheng earthquake the year before, trying to understand what was going on with all its foreshocks. This committee included Peter Molnar, and I guess he knew he was going there when he got me to start working on foreshocks. So while I'm still a senior at Brown, I started working on a paper with him that we actually got published in *Nature* that summer. So before I started in graduate school—

ASPATURIAN: You had your name on a scientific paper.

JONES: I was first author on a *Nature* paper ["Frequency of Foreshocks," *Nature* volume 262, 1976]. A very simple paper. Peter always called it a piss on the problem paper. A short paper to propose an idea, and then you go into a second paper where you really develop it.

Aspaturian: How did you feel about that? Were you extremely proud?

Jones: Oh, yeah! Here I was, I got a paper in *Nature* before I even started graduate school. Today that's actually more common, right? You get the undergraduates involved in research much more.

It was pretty rare then. So I was excited about that. And that Peter had used my research. For grad school, I had applied only to MIT and Caltech. MIT accepted me early on, and Peter called me to tell me and asked me to come up to Boston and talk with them. I went up there, and they had me meet with Frank Press, who at the time was the chair of this Committee on Scholarly Communication with the PRC, and he promised me that if I came to MIT, he would do whatever he could to send me to China.

Aspaturian: That's quite an inducement.

Jones: I withdrew my application from Caltech—I didn't wait to see if they were accepting me or not—and accepted going to MIT. Oh, and Peter offered me the opportunity to go spend the summer in Afghanistan where he was doing field work with French partners. There was another grad student, a guy named Steve Roecker, who was doing his thesis there. Two French students were going to be there too, but deploying portable seismometers takes manpower, and so Peter offered this to me if I came to MIT—so, yeah, he knew the right bribes. I love traveling and wanted to see all of this. That fall, I also got sick. I picked up some sort of parasite in Taiwan that was causing problems, and I ended up dropping out of my two physics classes in the fall. Because I was in the hospital.

Aspaturian: This was a digestive issue, I assume?

Jones: Yeah, and abdominal pain. That was also the point at which I asked them at MIT, "Do you care what my degree's in?" They're saying, "No. We don't care what the degree is in. You've got the physics courses you need—advanced electromagnetics or quantum mechanics is not really relevant to geophysics, and we'd rather have you taking structural geology." So after I dropped those two classes, in the spring I filled it up with geology classes instead.

Senior research project documents earthquake references in Classical Mandarin records

Then I had to have a major. So I had to add a Methods of Sinology class to complete my Chinese major.

Aspaturian: You had to take a poli sci class, basically.

Jones: No. Methods of Sinology is really a literature research thing.

Aspaturian: I would have thought it was political science.

Jones: No, no, it was how to do research in classical Chinese. Because classical Chinese is really a different language.

Aspaturian: So I understand.

Jones: And I did my project on earthquake references in the classical Mandarin records. Because Brown had this amazing collection of old Chinese books, and I basically documented the earthquakes in the Mandarin records.

Aspaturian: How did you find that?

Jones: That was really interesting, I really liked that. It did make me wonder though—"Oh, maybe I want to do Chinese after all; no, too far down this other road now." But I really enjoyed it. Science— I can see now with time that we're not actually that objective, but it seemed like there was a more objective standard of something being good or right than there is in literature.

Aspaturian: That is true.

Jones: It is more so. I think we scientists underestimate how much, really, we are being subjective, and we certainly make subjective evaluations of people. But I liked the fact that with science there was a more quantifiable right and wrong—I think that's what really pulled me over.

“Things were so much harder at MIT”

So I went to MIT. Most geophysics graduate students are physics undergrads who then realize—and it happens to a lot of us—that what actually appeals are the applied aspects of it. If, like me, you opt for geophysics, you then have to make up the geology classes that you didn’t take. So in my first term at MIT I had a petrology class.

Aspaturian: Well in addition to having an uncharacteristic bachelor’s degree background, you were a woman. How did all this play in?

Jones: There was one other woman accepted into the department that year. She ended up in marine sciences at Woods Hole [Woods Hole Oceanographic Institution], which has a joint program with MIT, and so she was actually out there. There were a couple of predecessors: Shamita Das was a third-year student, and she became a professor at Cambridge University, and there was a woman who had gotten her PhD in ’76. There was another woman who failed out, and I got her desk. It’s interesting, Caltech’s seismo lab [Seismological Laboratory] tends to—or at least used to—fail a significant chunk of their PhD students. If you got left with a master’s from Seismo, it’s because you didn’t pass your generals [i.e., qualifying exams for the PhD]. And the attitude was like, “Well, we accepted you, but are you really up to snuff?” Whereas MIT was more like “We accepted you; you must be good,” and it was pretty hard to fail out. But this woman had. Like I say I got her desk, and my officemate told me how she asked him what the derivative of E to the X was. Maybe you're—

Aspaturian: No.

Jones: The definition of E—it's that the derivative of E to the X is E to the X.

Aspaturian: I see. Yes.

Jones: So she clearly was not at all made for this stuff. So I was the only one in my class, and then Shamita was a few years ahead of me.

Aspaturian: Was geophysics embedded within the geological sciences department?

Jones: Yes. It was earth and planetary science when I was there. It's now earth, atmospheric and planetary science. So I was in course 12—MIT numbered all of their courses. The MIT department is in an eighteen-story building. It wasn't very big around—sort of like Caltech Hall but eighteen stories. And so fifth floor was the seismologists, ninth floor was planetary people, and the seventh floor had the rock mechanics lab. But Peter, a seismologist, insisted on having his office on the seventh floor because he wanted to try to be more interdisciplinary and not structured. So I was on the seventh floor, and one of my officemates was a Peter grad student, and another one was a rock mechanics guy. Things were so much harder at MIT.

Aspaturian: Than at Brown.

Jones: Yes. Brown was an Ivy League school, and, you know, I'd taken all the Chinese, and all these other classes. But at MIT it was as if the philosophy was "Let's put five times as much material in the class, so you'll learn twice as much." And these are all kids who've been the top of their undergraduate class. And now all this stuff is being thrown at them, and you can't get it all. I think it's true here at Caltech, to a certain extent too.

Aspaturian: This was a type of pressure unlike any you'd had encountered before.

Jones: It was much harder than anything I'd had before. It was very shocking. I got through, but I think I had two Bs.

Chinese-language & music activities at MIT

Aspaturian: Did you continue to work on your Chinese language and music while you were at MIT?

Jones: Music mostly got dropped for a while. There was a Renaissance music group; I went and met with them, and it was mostly people who were also with the Society for Creative Anachronism. They were there not because of what the music was, but mostly because they thought it was weird. That wasn't what I wanted to do. The viola de gamba I had played at Brown had belonged to the university, and I had learned how to play tenor—gambas come in different sizes. When I bought my own to take to Taiwan my junior year, I bought treble because it was the cheapest.

Aspaturian: Is that smaller or larger?

Jones: It's smaller. And it was really an awful instrument; it sounded like crap. So I had an instrument, but I didn't play much at MIT, at least for the first few years. The Chinese—I didn't want to lose it, and actually I audited a Chinese class. It was taught at MIT, but I think technically it was a Wellesley class; MIT had an exchange program with them. The other thing is that all but one other student in the class were Chinese. And I probably knew more characters than they did. [Laughter] We were reading novels and, you know—200 pages a week. It was great, and I stuck through with that. And then in the summer of my second year when I applied to go to mainland China and realized I could be going there, I audited something that fall at Harvard to try and get my Chinese back up, because at that point it had been two years since I'd been in Taiwan. And then after I came back from China, I actually discovered a group of Wellesley women playing the viola da gamba. I think it was about a year—maybe my fourth year, maybe my last year—that I played with this Wellesley group. It was not only a chance to do music, it was all women, which at that point—

Aspaturian: Was kind of a relief?

Jones: Yes. It was my escape. Mostly MIT took everything I had.

1976 Haicheng earthquake; field work in Afghanistan; giardia

Aspaturian: That's what it sounds like. So let's talk about your experience, applying to and going to China.

Jones: Okay, so a little Chinese history is important. At the time I'm applying to graduate school, the Cultural Revolution was still going on, and the Haicheng earthquake had happened, preceded by a lot of foreshocks. The Chinese claim was that they had successfully predicted it. Our proposal was to go study Haicheng, which they wanted to show off because of the predictions.

Aspaturian: Where was the earthquake located geographically?

Jones: It's up in Manchuria. Liaoning province, so pretty near Mongolia, northeast of Beijing. But first in the summer before I started at MIT, I went to Afghanistan. I think we went over in early July or mid-July, and we came back on the day that classes started.

Aspaturian: What year was this?

Jones: Summer of 1976. I fly off to Afghanistan in early July, and then the Tangshan earthquake happens on July 28th, I'm in

Afghanistan getting marginal news, and actually the Tullises sent me a Taiwanese newspaper report. I don't know how they'd seen it, but newspapers in Taiwan were saying this must mean that Mao's going to die because traditional Chinese culture had it that earthquakes were the sign of too much yin in the system—the earth overpowering the sky—and too much yin, too little yang would mean that emperor's become weak—he's going to die, or a minister's usurped his power, or having women in government could be the cause of earthquakes. Whereas an autocratic, overpowering emperor would cause hurricanes. That's the sky getting angry. And so there was this article about it that got sent to me while I'm in Afghanistan.

Aspaturian: In Kabul?

Jones: Well we were based out of Kabul. There was a French geology mission there, and so there was a French professor at Grenoble who had worked with Peter, and he came with two students, and Peter also came with two students and then there was the head of the geology mission who ran this hostel to organize things. There was a pretty elaborate French geology program going on. I'm getting off the plane after a long direct flight, and when the head of the mission sees me, he turns to Peter and says, "What the hell did you bring her for? How are we going to keep her safe? She's going to get stolen." Women were not people. They were property. And it was a pretty weird experience a lot of the time. I had the choice of wearing a *chadri*

—they call them burqas now—or wearing regular men's clothes —I had very short hair that summer—and passing as a boy.

Aspaturian: What did you pick?

Jones: Boy. And it was interesting. It was so far out of so many people's experience, they just assumed I was a boy in that situation.

So we spent a couple of months there. The Haicheng earthquake happened, and I saw some reports from the sources we had then, and then we fly back, and I had gotten sick; I had gotten actually giardia from the last day in Afghanistan. We'd spent the night in London and on the plane coming into Boston, I was getting sicker and sicker.

Aspaturian: I imagine that was miserable.

Jones: It was really miserable, and the Monday that we arrived was MIT registration day. This was long before computers, and I had to go to the armory for physical registration before I could see a campus doctor, because I wasn't a student yet. I passed out trying to register and had a temperature of 104 my first week back at MIT.

Aspaturian: Did they put you in the infirmary? I would imagine—

Jones: Oh yes. I spent my first week in the infirmary, missing my first week of classes. And then I had to find a place to live and all that. But I did, rooming with some seniors who had a place. Anyway. I took all my classes and, of course, with the week missing at the beginning, worked really hard to try and catch up.

MIT graduate research on foreshocks & rock mechanics

The second year, I had fewer classes, and I started to work on the research, and I ended up doing a paper on foreshocks and also a paper on rock mechanics. Because you're supposed to do two really different ones for your generals.

Aspaturian: Where were the studies centered?

Jones: The rock mechanics laboratory was on the seventh floor, where I had my office.

Aspaturian: I see.

Jones: It was literally taking fault gouge—the finely ground up rock that you find in the center of active faults—putting it under extremely high pressure and then measuring how it deformed. And then the foreshock paper involved using data from the worldwide seismic network that had been created as part of the 1963 nuclear test-ban treaty to monitor compliance. So for the first time, there was a global catalog of seismic events. And then, of course, the International Seismological Center had records dating back to 1900, though obviously the completion threshold was much lower. I basically went through all of that data.

Aspaturian: Was this the first time a study like this had been done?

Jones: Yeah, it's sort of funny but the ISC data wasn't computerized. The only stuff that was computerized was from the WWSSN [Worldwide Standardized Seismographic Network].

Aspaturian: So did they have it in large notebooks or—?

Jones: They were published books. And the main East Coast repository was down at Columbia, so I went down there. I spent a couple of weeks basically reading every page of the global catalog, looking for foreshocks. I was looking at how often do they happen; how many earthquakes are preceded by smaller ones; what are their characteristics; are they different for different regimes.

Travels to mainland China with first US–PRC scientific exchange program

And so those two papers were what I presented in my generals exams at the end of my second year, and after that you sort of blow school off for a month or two—everybody does. And so, by now, Frank Press has left MIT to become President Carter's science advisor. He had gone to China in the summer of '78; this is now a year and half after Mao dies, and the Chinese government wanted to have an exchange program. There are 60 Chinese scientists ready to go. And the Americans wanted to seize the opportunity—and we don't usually do things that quickly, right—so there was this really fast process to find scholars to go to China. They would be sending students, who were mostly graduate students in Chinese studies, to Beijing University; actually one of them was going to Shanghai. And then there were six scientists who would be chosen to go over and work in research institutes, and I applied for that.

I was working with Peter on how to do the application, and I joked with him, "You remember how Press said he'd do whatever he could to send me to China?" Peter said, "Why don't you call him up? He's the president's science advisor." And Press ended up being one of my references. So —I've just written this paper on foreshocks, I'm fluent in Chinese, my research proposal is on the Haicheng foreshocks, *and* I have the president's science advisor in my corner. [Laughter] So I was chosen to be in that group. I was the first and only graduate student in that group of six scientists. Everybody else in the research group were professors.

Aspaturian: What sciences were represented in the group? I don't imagine it was all geophysicists.

Jones: No, I was the only one. I don't really remember because they came later, and I only met a couple of them. Geophysics was something the Chinese really wanted because it seemed like they were successfully predicting earthquakes; and we were trying to learn how they did it. I went over with the graduate students who were going to the universities, and then the professor researchers came over later. That was in February of 1979.

Aspaturian: Where were you based?

Jones: Beijing. There was a state seismological bureau there. It's now called the Chinese Earthquake Authority, but that's just a different translation; the Chinese name's the same. The bureau had several different institutes, and I spent the first couple of months at the Institute of Geophysics and the second two months at the Institute of Geology. I lived at the Yǒuyì bīnguǎn, the Friendship Hotel, which was an old Russian building where they had what they called the "foreign experts." At that time, there was very little outside exchange with China, but the "foreign experts" were foreign language teachers and translators, and they all lived as a group at this hotel. Being with them was one of the most interesting parts of being in China because on that first trip I didn't get to interact a lot with the Chinese.

Aspaturian: How long were you there the first time?

Jones: Almost five months. It was from February to August. When I left Beijing, I got permission to visit various places in southern China, and before that, my aunt, who had grown up in Nanjing [formerly Nanking] and was a missionary in Hong Kong at the time, got permission to come visit me. She goes into the Beijing embassy in Hong Kong and says, "I want to go visit my niece in Beijing," and they're looking at this white lady and saying, "What?!" But she got permission, and she came up. We met, and we were able to meet with one of my grandfather's former students, which was pretty amazing.

And when she left, she went back down to Hong Kong through Nanjing, and she wrote me back these really detailed instructions on how to find the family places. [*See also Session Three*] So I then got permission to leave China through Hong Kong, and I first went down to Shanghai and over to Nanjing and up the Yangtze River.

Aspaturian: When you say you got permission, from whom—

Jones: The government, the Chinese government. You couldn't just travel.

Aspaturian: I have a number of questions, but I notice you looking at your watch. Do you have a time constraint?

Jones: No, no, I have another hour. It's that I just got an email that there's an earthquake, and I had to look up where it was. [LJ was wearing an Apple Watch] I think it was San Fernando.

Impressions & experiences during first stay in China, Feb.–Aug., 1979

Aspaturian: That's some distance away. What were your first impressions of being on the mainland? I know it was a long time ago, but—

Jones: Oh, no, the memories are really clear, starting with the contrast with Taiwan. Taiwan was a society with a very obvious class structure. You had very rich people, you had very poor people. And in Afghanistan, you'd see children with arms twisted around because they'd broken them and had never gotten the injuries set properly. No medical care. In Beijing, you didn't see that. Everybody had decent clothes. Everybody had decent medical care. And nobody was rich. There were no advertisements. There were these occasional billboards around Beijing that were in red with Mao's aphorisms on them.

Aspaturian: And his picture, I imagine.

Jones: Sometimes, you saw that too, but these were big billboards that were just red backgrounds and white characters with phrases—"To Serve the People," that sort of thing. And I remember driving in from the airport and seeing these beautiful trees, but it was also all gray. There was no color, there was no greenery; now it was late February, and it's cold. God it was cold! And I still remember, how when my aunt came, her biggest shock was that there were trees. When she was there, there were no trees. People were so desperate for fuel—

Aspaturian: They cut everything down.

Jones: They would have stolen the wood in the night. In Beijing people were well enough off to not need to take out the trees. I have a mixed feelings about it—it was a combination of the incredibly egalitarian and then sometimes just ruthless autocracy. Eventually, I got taken around to a couple of earthquake sites, including Songpan in Sichuan province.

Aspaturian: Was that this first time also?

Jones: In that first time. I remember being up in the mountains in Sichuan, and every morning at six a.m., they would play *The Internationale* [traditional anthem of the world socialist movement].

Aspaturian: Of course.

Jones: Everybody's supposed to get up. The state controls when you get up in the morning.

Chinese earthquake science during & immediately after the Cultural Revolution

So first I went to the Institute of Geophysics, where we did some work on the Haicheng foreshocks— Our proposal was to go study Haicheng, which they wanted to show off because of the predictions. and then I went to the Institute of Geology, which is when I visited Sichuan. And they took me to Xi'an, you know the buried—

Aspaturian: The soldiers, the terra cotta soldiers?

Jones: And they were still in the ground, and no Westerners had seen them at that point.

Aspaturian: It must have been thrilling.

Jones: It was thrilling.

Aspaturian: How long had this Institute of Geophysics been in existence?

Jones: At that point, well, all right, before the Communists took over, there was an Academia Sinica [today in Taiwan, the Chinese Academy; and in the PRC the Chinese Academy of Sciences].

Aspaturian: Yes, of course.

Jones: And it had an Institute of Geophysics and an Institute of Geology. The Earthquake Authority was formed in 1966. There was a big earthquake, do I remember the name? 7.2, March of '66, southeast of Beijing.

Aspaturian: I can look it up. [Xingtai earthquake, March 22, 1966. –*Ed.*]

Jones: I have it somewhere; it's in my book. [*Session Three*] And it was just as the Cultural Revolution was getting going, and it killed thousands of people. Zhou Enlai went down to the site and learned that it had been part of a sequence, so the 7.2 was not the first earthquake, and the thinking was, "If only we could only have recognized that it was coming." So he called on the scientists to figure out how to predict earthquakes, and the government established this earthquake prediction program in '66.

Aspaturian: So was this program basically under the protection of Zhou Enlai? Because so many scientists and academicians were stripped of their privileges and sent to the countryside during the Cultural Revolution. [Zhou Enlai was the PRC's first premier, serving from 1949 until his death in 1976.]

Jones: Right. My impressions from people who talked to me is that it was created to protect scientists. Zhou Enlai saw what was coming, and he wanted to protect some of the intellectuals. They still had to send a few to education camps. One of the guys I worked with said he had volunteered to go to the education camp—to work in the fields for two years to demonstrate his reliability. It was a way of advancement in the long run.

Aspaturian: He told you this during your first visit? He spoke about this to you freely?

Jones: Only when we were on a train where nobody could hear us.

Aspaturian: I see.

Jones: There was this train ride that we took, and during it. I heard a lot more stuff. He had actually trained as a structural geologist and had had nothing to do with earthquakes. And he said, "I just found myself fascinated by earthquakes, and I moved over, because—you know." Well, yes. He was a lovely guy and over time—the second time I was back—things were a little bit freer.

Aspaturian: We'll get to that. [*See also Session Three*]

Jones: I don't always remember in which year he told me what, as we developed a relationship over time, but he became a good friend with Peter Molnar and led a lot of work over there. The other thing is, I was so much younger than anybody else. Because the last Chinese researchers would have just gotten out of school in 1966. Anything later—

Aspaturian: There was no organized education for several years.

Jones: Right. So the youngest Chinese scientist there was around 36, and I think I turned 24 right as I got over there. So there was a paternalism in a good sense. Everybody was sort of like I was this kid, and they were excited to have me there, and they're taking care of me. So it was good relationships with people.

Aspaturian: What strikes me is that you were also there just as Chinese science was beginning, very painfully, to reconstitute itself.

Jones: Yes!

Aspaturian: Did this strike you at that time?

Jones: Well, during my last trip in 1983, we uncovered a case of scientific fraud.

Aspaturian: Among the Chinese scientists?

Jones: Yes. Although it was a Chinese scientist who helped uncover it. It was a horrible loss of face for everybody, and that's when I decided to work in California instead of China; I needed to have a reputation that was separate from what I could see coming. So it was a mixed bag. You had people—these were older people—who had been trained in the West before things had shut down. And in the earthquake program you had some really good scientists who had been able to stay in their field and keep spinning. There were structural problems is the way I guess I'd put it.

Aspaturian: I certainly would imagine, yes.

Jones: On my first visit, I brought over an HP calculator. It was a programmable calculator, one of the very first ones, and I wrote a program on it in BASIC to locate earthquakes—to do the BASIC matrix inversion algorithm. The Chinese had never had any computerized earthquake location devices; it was all done graphically. When I took the data back to the States. we got the quakes approximately located and analyzed, and when I went back to Beijing the next summer, we were able to clean it up. But they didn't have computers.

Aspaturian: No, they didn't have much of anything at that point, I'd imagine, except for raw people power.

Jones: They had lots of people. And each province had its own seismology bureau. So we went up to Liaoning province, which is where the Haicheng earthquake had happened and read the physical paper seismograms up there for a couple of weeks, and then I ran that data through this hand calculator to come up with approximate locations of foreshocks. We learned a lot of about the process of analyzing those earthquakes.

Aspaturian: Your first time there, how did your Chinese colleagues react to you as a young woman and a Westerner, do you recall?

Jones: It was like parental. It was very affectionate. They were excited to have me there.

Reflections on native vs. foreign Mandarin speakers; awareness of "handlers"

Aspaturian: And you spoke the language and you understood it.

Jones: I could talk with them all. My second summer in Beijing, there were starting to be some more people from overseas—both Americans and Europeans.

Aspaturian: Is this 1980?

Jones: Yes. We had an American Chinese professor from Berkeley who came over. And when he spoke Mandarin, he sounded like he was Chinese. Almost all Americans speaking Chinese have accents. I mean, I was fluent, and I was completely comfortable in all of my phrasing, and people who I spoke to over the phone thought I was Chinese—which I think says more about how they just couldn't imagine that a foreigner was really speaking. But I definitely had an accent, and this Berkeley professor didn't. If you heard him speaking just around the corner, you would be sure he was Chinese. I still haven't figured out how he did it. Jimmy Wrenn was the only other person who that was true of.

Aspaturian: Some people have that ear.

Jones: These were the only two foreign scholars about whom this could be said. Others were completely fluent, could read anything, say anything, but you still could always tell they were foreigners. I was more in that category; I could say whatever I needed to say, and I was even translating other people's lectures, but you could tell I was American. When this professor came from Berkeley, somebody I was working with was telling someone else about how incredible he was, that he sounded

Chinese; and this other person said, "Lucy is practically fluent," and— "No, no, no, *he sounds Chinese*." [Laughter] But I was comfortable saying whatever I had to. But then there was also this aspect that a Chinese man couldn't be alone in a room with me. It was just a year since the Chinese could never be alone with a foreigner. Now it was, You couldn't be alone with a foreigner of another gender.

Aspaturian: Progress of a sort.

Jones: So there was a woman seismologist who had been assigned to work with me as a colleague. It took me a little while and some talking with the other foreigners to realize that she was my handler, too. The scientists had to take on that political role too, and some of them definitely were handlers. The people I was working with were wonderful, and I thought of them as colleagues, and they were really nice and happy to have me there, but there was always this sort of awareness of the handlers. And since I was fluent in Mandarin, I sort of caused a problem, because they couldn't have my handler assume the role of a translator. And my mail was all read.

Aspaturian: Of course.

Life with Beijing's foreign-language teachers at the Friendship Hotel

Jones: My first year, my first week or two was really weird. I was staying in the front part of the Friendship Hotel. There were

Japanese businessmen staying there as well, and when I'd walk by, there were all these men looking me up and down. I had written a letter home that said something like, "Arghh, I feel like I'm in this gilded cage, what am I going to do?" And then I discovered there was a whole back part of the hotel where all the foreign language teachers were living. They were there from all around the world; many of them had been there for years, and they were really friendly. I started hanging out with them, and everything was great. And then somebody from the institute of geophysics to talk to me: "How are things going?" I said, "It's great, I'm really enjoying it." "But aren't you having problems with—" And I'm thinking, "You read my mail, haven't you?" [Laughter]

It was so obvious from the questions they were asking me that they had to have read this letter where I had been sort of depressed at the beginning and feeling out of place and isolated. But it ended up being this really wonderful time with these teachers. I couldn't really be friends with my Chinese colleagues that first year—they had to be careful with me.

Aspaturian: Of course.

Jones: As I said, toward the end of my first time, I went down to Sichuan by train with this one geologist, and there were things we were able to talk about on the train. That was really the place where I felt like I was becoming friends with him. Before that, not really. But it was a really fascinating international group at the hotel. Because for each language, the government had come up with some way of recruiting teachers. At that point, all of the English-language teachers were from the New Zealand–China Friendship Society. They all had Kiwi accents. And actually by my next summer, in 1980, China decided they didn't like the Kiwi accent, so they didn't renew the contracts for any of the New Zealanders, and instead they brought in all these Americans. The Germans were from the West German Communist party.

Aspaturian: Italians, too, I would imagine.

Jones: Oh, no, no, the Italians were crazy. They were from a radical left-wing group that was like left of the Red Brigade. They were knee-cappers. Everybody was really, really scared of the Italians. The Spanish teachers were from Shining Path in Peru.

Aspaturian: Oh, my gosh!

Jones: Yeah! They were such sweet guys, too. But they were all there to not be extradited back to Peru. And actually the Arabs were from the PLO, but they were a part of the PLO that had fought [Yasser] Arafat, and they were basically hiding out in Beijing because it was the one place Arafat couldn't get them all.

Aspaturian: All these splinter groups.

Jones: The Yugoslavs were actually from the government; they were like Zagreb journalists. And so it was this incredible mix of people. Everyone stayed away from the Spanish and the Italians. [Laughter] The rest of us mostly hung out together. It was a really interesting, one of the more interesting parts of my life, being with all these people.

Investigates Chinese claims regarding earthquake precursors & uncovers fraud

Aspaturian: Between '79 and '83, how many times did you visit China?

Jones: Four. So 1979 was like February to August; and 1980 was June, July, August. By then, there were more overseas geologists. Peter came over for part of that summer, and a French geologist, Paul Tapponnier, whom Peter had done a lot of work with, was over there, too. And then I went back in December '81 to January '82. By then I had graduated: I'd gotten my PhD and gotten married, and a couple months after our wedding I flew off to China for six weeks. Of course, Egill [Hauksson, research professor of geophysics, emeritus] spent a month in Alaska right after our wedding. Two geologists, right? And that trip to China was with another student of Peter's who was going to be picking up some of the work funded under Peter's grant. We went back and actually looked at the aftershocks of the Haicheng earthquake and did a follow-up study. And then my last trip was February to April '83, with my husband. Egill had done his thesis work on radon anomalies. There was a time when we thought that changes in radon emissions might be earthquake precursors.

ASPATURIAN: I seem to remember that, yes.

JONES: There was a guy here on campus, Tom [Thomas A.] Tombrello [Goddard Professor of Physics; d. 2014], who was looking at this.

ASPATURIAN: Oh, yes, I did his oral history. Sure.

JONES: Okay, he was working on that, and I'd met him by then. My husband's Icelandic, and he had come over here to be a seismologist, but there was a geochemist, Wally [Wallace] Broecker, who was really interested in this radon possibility, and he convinced Egill to do this project in Iceland, measuring radon around a big eruption that had been going on for several years at the Krafla Volcano. And Egill did a great job demonstrating that there was absolutely no precursory information whatsoever in the radon levels.

But then this Songpan earthquake in Sichuan, where I had gone during my first year in China, was claimed to have been predicted because of a combination of animal, water, and radon anomalies. I had just done a paper on the animal anomalies that had been reported before the Haicheng earthquake, where we actually concluded most of that signal was politically motivated. You could see the anomaly reports spike on Saturday afternoons after the commune meetings—that type of thing. But it also looked like there might be some correlation between some of the reports of unusual animal activity and ground water changes. Our paper was somewhat inconclusive but said there was a possibility there.

So Egill and I wrote a proposal to go look at Songpan together, with him looking at the radon and me looking at the animal anomalies—because I had the Mandarin to interpret all of those reports—and then we would also do locations. I had brought along this 16K HP computer to do calculations, and this is where we uncovered the fraud I was talking about earlier. We ended up being able to demonstrate that the animal anomaly data had been faked by one guy at the provincial seismology bureau. He had a high school diploma, he wasn't a highly trained guy, no one was watching him too closely. He made up part of it and only collected data from where he wanted the pattern to show up.

Aspaturian: He wanted so badly for it to be true, I imagine.

Jones: Yes. So that was my last trip. We wrote a paper with our Chinese colleagues with the locations of the event. Actually the day we arrived, they told us, "Oh, yeah, there really aren't any radon anomalies." "Oh? Okay. Okay!" There's that one gone. Then we analyzed the animal anomalies reports and found that was a mess too.

More on navigating science, culture, & social interactions in China

Aspaturian: During your four to five years of visits there, you must have seen changes in the nature of the interaction with your colleagues and how the scientific enterprise evolved—

Jones: It was a huge change.

Aspaturian: Would you talk about that.

Jones: Okay, right. When I first got there, there were something like 35 Americans living in Beijing. Anything that happened at the embassy, we were all invited to. I got to meet [famed ballet dancer, Mikhail] Baryshnikov.

Aspaturian: How exciting.

Jones: Yes! But that year there was still this pretty strong wall between the Americans and the Chinese. I went and bought a bicycle my first time there, and they were so upset about it. I don't know how much of it was because they thought that I would get injured, and they would be held responsible. I actually think that was a chunk of it.

Aspaturian: The Chinese were upset?

Jones: The Chinese were upset. But they also then didn't have control of where I went.

Aspaturian: Of course, of course.

Jones: I used to bike around Beijing. The only automobiles at the time were owned by government agencies. Basically everyone was on a bike.

So that first time, I was hanging out with these crazy Arabs and Yugoslavs and the Czechs and the French—the French were pretty cool; they came from the government. I got a lot of support for my science at the institutes, but mostly there was a wall. The scientist who was assigned to be my handlers was the youngest scientist at the Institute of Geophysics. After the Tangshan quake, the city government of Tianjin asked for a seismologist to give them warnings about aftershocks— and every seismologist knows you can't really predict aftershocks. Anybody who could got out of it. So as the most junior person, she was the one who got sent.

Aspaturian: She was the designated—

Jones: Victim. Yeah.

Aspaturian: Good word.

Jones: And she told me stories about that. She would say something like, "I think there won't be any more magnitude 6 aftershocks," and she'd be told "'I think' is not good enough." [*See also Session Three*] So we got to know each other well enough to be able to share things like that. Then I went to the geology institute where there were no women. And this is what I think is an interesting thing; the Chinese don't have a cultural bias against women doing math. They do have a cultural thing against women camping: *Geology was so rough, you had to go sleep out in the wild, and no woman would ever want to do that!* So there were no women geologists.

When I was in geophysics, I shared an office with a female colleague. If she left to use the bathroom, and some man came toward the office and saw that she wasn't there, he would leave. Later, when she was there, he'd come back and want to talk to me.

ASPATURIAN: There had to be another woman present before—

JONES: They could not come into the room otherwise. But they didn't explain this to me.

ASPATURIAN: This was your first time there?

JONES: This was the first time.

JONES: So when I went to the geology institute, they assigned two men to work with me. If one of them went to the bathroom, the other one would get up and leave. It was on the train to Sichuan that they later told me that there was this requirement. "Ohhhh! Now I understand what's going on here."

ASPATURIAN: What did you wear? Do you recall? Were you in the Mao outfit?

Jones: At the time, everybody wore the Mao jackets. I could only shop at the store for foreigners and so actually the jacket I got was this incredibly beautiful, brocaded thing. But then there was the plain blue cotton standard outer jacket to wear over it to keep it clean, and so I could fit in a bit more. I don't think I ever got the pants.

I remember, there are pictures of me in that blue over-jacket. I've just recently been looking for them.

Aspaturian: Well, we will be illustrating this, so something to keep in mind.

Jones: There's a picture of me at the Great Wall. In the first month that I was there, someone took me up there on a trip, and I have a picture from there with my hair up in a Mao cap. I used the cap to get my hair out of the way, too. So mostly I just dressed like that to not stand out even more, and to put the blonde hair away. By that time I had shorter hair. When I was in Taiwan in college, my hair was down to my knees and that caused—

Aspaturian: A stir.

Jones: A stir.

Aspaturian: I believe at that time, most women in China had to keep their hair quite short.

Jones: Usually they did, yes.

Aspaturian: That would have been another aspect.

Jones: But when I cut my hair short, it curls.

Aspaturian: Which most Chinese hair does not.

China's changing cultural & political landscape, 1979–1984

So, over the course of these five years, what did you witness in terms of a change in dynamics?

Jones: At the beginning, they were so restrictive. By the second time, overseas Chinese with relatives could come and visit. There were starting to be more people from outside. Actually at the end of my first time, in '79, you started seeing colored scarves. And that was like the first sign of color showing up. The second time I came back, there was a wider range of clothes, but you could always tell the mainland Chinese because of the plastic shoes.

Aspaturian: This was in 1980.

Jones: In 1980. And then'81, '82, there were a lot more foreigners. When we went up into Liaoning, Italian engineers were working there in some plant. They were supposedly helping the Chinese develop a bicycle plant, but it was military work, and they were covering that up. It's funny— I think I'd just gotten married; I just wanted to get home. That was one trip where I was just doing the work as fast as I could rather than interacting too much. And then when Egill and I went back in '83, the fact that they let us go and stay in Sichuan was new. And my husband was with me.

Aspaturian: Also a difference.

Jones: But we found that things were not as open, staying in a province, as they'd been in Beijing. My guess is that this still hasn't changed that much— well, actually, I wonder what the internet has done with that aspect. So, while we were in Sichuan in March of '83, living on the ninth floor in a 1950s Russian-built concrete building, there was an earthquake, about a 4.5, not too far away. And it was enough to make us go, "Oh, holy crap." And then, "How do we find out where and what it was?" We weren't allowed to go to the seismology bureau. The bureau rented a conference room in the hotel and that was our office.

Aspaturian: Why? Who knows.

Jones: Who knows. So the morning after this earthquake, the seismologists come over to our hotel, and I'm like, "What's the story with this earthquake?" They gave us the information about it. "How do you get the information out?" "Well, we report it to the Party." "What about the people?" "Why should we give it out to everybody? They don't need to know." The attitude was, This information should only be given out if there's a reason.

Aspaturian: Centralized control.

Jones: That degree of control. But the idea that you only get information that the government decides you need to have—I think that part is probably still true.

Aspaturian: I'm sure there's been some erosion around the edges.

Jones: There was also so much more commercialization happening around that time. The foreigners coming in, and all of that. It was sort of sad. For all that it was weird the first time I was there, there was an integrity to it. Nobody had money. I did encounter one exception. One of the Arab language teachers was actually an American named Graham who had been a Quaker missionary in Palestine—Israel. I'm not quite sure, but it's possible he was also Canadian—the Chinese government liked them better than the Americans as teachers. Graham knew a guy named Ma Haide—his American name was George Hatem. He was a medical doctor—a tropical medicine epidemiologist—who had gone to Shanghai in the 1930s. With the beginning of the Communist revolution, he and a Canadian doctor joined the Red Army as their doctors. He ended up staying and becoming powerful. He eliminated syphilis in China because they didn't ask who you'd been with— they just tested everybody because you could do that in China.

By the time we were there, he was in some sort of assisted living residence, and somehow Graham got to meet him, and then I got to go over and meet him. He was a really inspiring figure. There's sort of the idealism of communism—the early idealism. Looking at what's happening in Russia now, you see all of the distortion of it, but there was an idealistic part of it. He was there for that reason, and I found I could really admire it. When my dad was a teenager in China in the 1930s, he identified as a socialist because he saw how corrupt the government was and the idealism of the other. I think it was sort of that outlook that I'd been brought up with, and that first time in China, I could really see it.

Aspaturian: You could relate to that.

Jones: I could relate to it. The reason I brought up George Hatem is that he was the one person I met that had some extra privileges. But he was a member of the Chinese Central Committee— the only foreigner to ever become a member. So he was in the top twenty of all of China. His life wasn't luxurious. He was in a nice little suite: It was at a higher level than average, but not much different than where I was staying in the hotel. And that's what they gave to the Central Committee members, and it was like—*this is right*. The egalitarian ideal was really there, and then it was gone. The Communists also screwed a lot up; you had the Great Leap Forward and all the ways in which they really blew it. But there was, as I said, an integrity to it that I admired. And that was mostly gone; it was definitely going away on my last trip there.

Cultural Revolution's damaging impact on Chinese science

Aspaturian: How about the ability to do science? Did that benefit flow in the opposite direction?

Jones: Well, yes. The Cultural Revolution did a lot of damage in a lot of ways, and one of them was the designation of the "Stinky Ninth." There were eight enemies of the state outlined by Mao early on, and during the Cultural Revolution, intellectuals were designated as the ninth enemy of the state. This was a pun based on the word for "arrogant," which was a homonym for a word that meant "stinky" or "smelly." There was traditionally a real arrogance in Chinese science about the theoretical being so much better than the experimental, and that attitude came back in spades after all this came off.

The problem at that time— and I think they addressed this finally because there's so much more interaction with the West —is that they really didn't have peer review. The Chinese scientific journals were all published by the work units. That meant that if you were high enough up in whatever institute you were affiliated with, nobody was ever going to give you a bad review. I mentioned how on our last visit in 1983 we were starting to realize we were dealing with the earthquake prediction fraud in Sichuan.

There had been a Caltech graduate and USC professor, Leon Teng, who had been invited to Sichuan before we were there. The Chinese had given him data about this earthquake, and Leon published as an article in the *Bulletin of the Seismological Society* [*of America*—BSSA] with the acknowledgement, "We can't check any of this; all we can do is provide you the information that we were given." It really wasn't a peer-reviewed article in that sense, right? It basically said, The Chinese say that they predicted this earthquake; here's what they showed us." And it included a graph about the animal and water anomalies.

So what Egill and I did was to go back to the original reports, and I created an index card for each report. I translated every one of them into English, and then we could start organizing them and try to see what the patterns were. We did the first basic thing of just repeating what had been in Leon's paper and did a histogram. There was no correlation! It didn't look anything at all like it. And we looked at this, and we were like, We've got to figure out what's going on. We talked with our colleague from the Sichuan provincial seismology bureau and his response was kind of like, "So what? We already know what we did."

We're trying to explain it to him this way: "Let's imagine we write a paper about this and send it to BSSA. They're probably going to ask Leon to review the paper. And he is going to look at this, and he's going to see how different it is from the first paper, and he's going to want to know why. You gave him this other stuff before; now he's going to see this and he's going to want to understand why it's different." His attitude was, "Why would Teng think we're lying?" And we realized, "You don't get it, do you?" Real peer review was not part of the scientific culture at that point. So when it turned out that the data had been completely made up, you could recognize that it wasn't that everybody was corrupt, but rather that they didn't have a mechanism to have caught the guy who did this. I think that is one thing that's changed.

Aspaturian: I'm sure it has, yes.

Jones: It's had to by its exposure here. But that was the point at which I said, "Other people are going to be figuring this out, and I don't want to have my reputation solely tied to stuff that's going to go through a bad period as people figure out that it isn't reliable. That was when we decided to apply for the jobs in California.

Completes PhD thesis on mechanics of faulting, incorporating Chinese field data

Aspaturian: What did you write your PhD thesis on?

Jones: The mechanics of faulting.

Aspaturian: Was that based partially on your Chinese field work?

Jones: Yes. It was interesting. It was a time when they were just starting to move to where you could do your thesis as a series of papers instead of one big study. So I had four chapters. One was the global foreshock analysis that I had done as part of my generals and had gotten written up and published. The second was rock mechanics paper from the generals also. I actually had two people signing my PhD because my work was split between Peter and Bill [William R.] Brace, who was my advisor for the rock mechanics part. The third paper was the Haicheng analysis. It was one of the first to do a stress analysis of the earthquakes, looking at triggering in terms of how movement on one fault changes stress on the others. The fourth one was another rock mechanics paper, looking at permeability in fault gouges. That's why it was called the mechanics of faulting, looking at both seismological and laboratory data about what's physically happening on faults. With two of them I literally stapled in the reprints of the published papers.

Aspaturian: Were the other two published as articles as well?

Jones: The last one never did get published.

Aspaturian: I'm sure the Chinese data did.

Jones: The Chinese data all did. It was published in both English and Chinese, actually. And all the later Chinese research all got published.

Aspaturian: I'd like to come back to a little of this next time and then go on. But it has been an hour and a half, and you need to be somewhere at five.

Jones: That's true.

Aspaturian: So we should probably stop.

Session 3, March 29, 2022

Experience in China crystallizes thinking on relationship between earthquake prediction & public policy

Aspaturian: I wanted to ask if you still use your Mandarin.

Jones: No. I dreamed in Chinese for a while when I was living there. It was the language I was living in, but my last trip to China was in 1983. Then I had kids, so I didn't have time to travel. It was not something I was able to keep up really well. I can still do a decent job ordering in the restaurant. That's one of the things I keep around.

I went back to Taipei in 2009. It was the tenth anniversary of the Jiji earthquake, and there was a symposium. We only spent a week or so there, and I sort of refreshed my Chinese. It's in there; I can use it; but it needs help to come back at this point.

Aspaturian: Last session, you told an interesting story that was partially drowned out by the wind on the audio file. It was about how your "handler" [*Session Two*] got stuck with dealing with the authorities on aftershock questions after the Tangshan quake. I was going to ask you to repeat it and also to give me her name.

Jones: Her name was Wang Biquan. She was the most junior person at the Earthquake Authority when the earthquake happened, so she was the only one who couldn't get out of being sent to Tianjin, where the city government was asking for aftershock assessments. There was a point in the process where things had died down, and she said, "I don't think we're going to have any more magnitude 6s," and the response was, "'I think' is not good enough; you have to tell us *yes or no*."

And so she had to choose, and she chose "no more magnitude 6 aftershocks." She told me that she stayed afraid of being proved wrong for a long time. Because there's always some probability, right? At the time I was talking with her, it was three years after the earthquake and probably a year-and-a-half after she had said that. There were no more magnitude 6 aftershocks, so she wasn't wrong.

The question they put to her, though, was actually part of why I started getting into earthquake statistics. Because, if you could legitimately say, for instance, "There's a less than 10 percent chance of a magnitude 6 aftershock," that potentially would be more acceptable—at least in the United States— than "I think."

So that story made an impression on me, as well as recognizing that while the Chinese were able to act on the Haicheng foreshocks, basically they were *guessing* that they were foreshocks. And realizing that the same level of seismological uncertainty led to different levels of action in different parts of the country— that you could evacuate an agricultural area in Haicheng in February. It's an awfully cold time of year to be outside, but you wouldn't really be disrupting the economy. Whereas imagine if we made that prediction in LA. The costs of wrong predictions could be worse than anything we would be saving.

Aspaturian: Of course, and here you didn't have the type of central control they did, which made an order like that virtually mandatory.

Jones: Right, and in fact it's pretty clear that there was an evacuation in Haicheng conducted about six weeks earlier when another swarm happened, but there was no major quake afterward. After a few days, it was like, "Oh, never mind, go back inside." And then six weeks later, they evacuate again—and they get away with doing it twice! That would never happen here. I recognized that if I could turn these types of predictions into statistics, that then becomes a matter of handing the scientific information to the people who have the responsibility—

Aspaturian: The policy makers.

Jones: The policy makers, who would then be able to use it and incorporate the other factors that would need to go into such a decision.

Enjoys more relaxed interactions with Chinese colleagues & explores family roots

Aspaturian: We'll go into a little more depth on that in a few minutes. When you first met your Chinese colleagues, China had been virtually isolated from the rest of the world for a quarter century. Were they curious about the West? Did they ask questions; did they become more open in their questioning as your visits went on?

Jones: As the visits went on, China was in a process of opening up, so people were much more open in 1983 than they had been in 1979. That wasn't due so much to more familiarity with me, though that also happened. I got to know people on repeated trips, and I felt like I became friends with some of them, especially in the geology group. You know, the first time it was all kind of stilted—the two men who couldn't be alone with me, all of this. [*Session Two*] I continued to work with them, and by the 1982 trip, I was going out and hanging out with one of the geologists and his daughter.

Aspaturian: That is a big change.

Jones: Yes, so all of that happened. Their curiosity about the West— I think they felt they had to be a little careful about asking me too much about it. There was curiosity about me personally, partly about my being so young, to their way of thinking, their view of the world. There was also interest and curiosity over the fact that my family had spent years in China, that I had history there, that in fact I had a *gūgu* still living in Hong Kong. In Chinese, you don't just say "aunt." *Gūgu* is the word for your father's older sister, and there are separate words for your father's younger sister, your mother's older sister, and so on. Your father's older brother's wife has a different name than your father's younger brother's wife. All of those are separate names. So they would ask more about my aunt and that connection with China. And on that first trip, my aunt came to visit me. [*See also Session Two*]

She also sent me detailed information about how to get to the family properties. And so my first time there, I was able to tack on this trip at the end where I went with a friend—actually a guy from Yugoslavia who was at the Friendship Hotel, where we were all living. He was one of these experts who was brought in to teach the Serbo-Croatian language, and we traveled together to Shanghai and Nanjing. I was able to find the house where the family had lived in Nanjing and my grandfather's office.

And then we went to their former summer house, up in the mountains. Nanjing can get up to 110 degrees in the summer, and people who could just used to leave for spots that were cooler. There was a resort essentially—I'm not sure of the legal status of who owned what—where all the missionaries would go. Kuling is what they always called it. It's now called Lushan, up the river. It's a beautiful famous place, and so we went up there and then found the house. The Chinese were like, "So are you going to ask for it to be returned to you?" Because all the properties owned by foreign nationals had been confiscated.

Aspaturian: Of course.

Jones: And they were starting to talk about giving them back. And the response was like "Nooo, I don't think so." [Laughter]. So there was that kind of thing and a lot of curiosity and questions about what it had been like when my grandparents were there. There was also curiosity about MIT and what an American grad school is like.

Aspaturian: Had they heard of MIT?

Jones: Oh, yes.

Aspaturian: MIT, Harvard; I imagine a few names percolated through.

Jones: Caltech, MIT, Harvard, Yale. Yale especially had a big Chinese program. So they knew all the big places. They knew Berkeley because one of the other exchange students there came from Berkeley, and I remember that they were impressed by that. We're also talking now about 40-plus years ago, and this is what I can remember about how they responded.

Aspaturian: I understand.

Jones: With the colleagues who got to know me as a person there was much more curiosity, many more questions than with people I would know in other situations. It was always interesting. China probably doesn't have class distinctions like they used to have anymore, but there's definitely an education difference.

Aspaturian: Yes, I think that's historic in China.

Jones: Right, and it's still there no matter how much of an effort the Communist government made to eradicate it. Dealing with the drivers from the research institute was a different situation than dealing with the scientists. There was just a lot of curiosity in general— my blond hair. People would want to touch it. Especially the first trip when China really was isolated.

Visits sites of "predicted" Chinese quakes & is successfully treated with Chinese medicine

So on that first trip, I went around with a Chinese geologist, and we went down to Sichuan, and that was the train ride I talked about earlier [*Session Two*], where he opened up a lot more. We visited with the provincial seismology bureau in Chengdu, and then we drove up into the mountains where there'd been an earthquake in 1976, about three weeks after Tangshan. It was "predicted," if you will, at a time when about half of China was living outdoors because the country was so freaked out about the Tangshan earthquake and what else might happen. We now look back on it as what we call a triggered earthquake. It wasn't that far away from Tangshan, maybe 1000 kilometers.

ASPATURIAN: What is the name of it?

JONES: This was Songpan. It is located up in the mountains that are starting to rise into Tibet.

ASPATURIAN: Was this the one associated with the fraud later?

JONES: Yes.

ASPATURIAN: Okay. I've been looking into it a little bit since we talked.

JONES: But Songpan was one of their "predicted" earthquakes, so they wanted me to come and see the place. Songpan is up in the mountains at some elevation, a town of about 5,000 people, and they said I was the first non-Chinese to be there since 1949. So we drive in, and we drive into the Party headquarters, which is in a kind of courtyard compound and the compound doors are closed behind us. They take me in to meet the Party leaders, and then you realize that most of the people of the town, 1000–2000 people, are standing outside the compound.

ASPATURIAN: To get a look at you.

Jones: To get a look at me. Not having actually seen me but just having heard that I was going to be there. And so, yeah, it was a little intimidating. Then— I got sick. I had gotten giardia in Afghanistan, and back at MIT I had started having some intestinal issues that nobody in Boston, could figure out: "Well, this is really fascinating," and they did all these tests on me. I'd get these repeated bouts of it, and it came up again in Songpan. I'm staying in this hostel, where again, locked doors, but you'd have to walk across the courtyard to get to the bathroom, and these crowds of people are standing outside waiting for a sight of me as I have intestinal troubles.

Aspaturian: Of course, the Chinese bathrooms are very different, as I recall.

Jones: There's that, too. That part I lived with. The fascinating part was I finally had to admit to my hosts that I was sick, and on the way back to Chengdu, we went through a town with a famous Chinese medicine college. All of my Chinese colleagues were saying, "For respiratory diseases, I go to a Western doctor but for intestinal things I go to a Chinese doctor." And when we go there, this ancient professor talks with me and asks about my symptoms, and then he asked—and of course this is all in Chinese—if I had had giardia. I had never heard the word in Chinese—and giardia was something that had never come up in my Western medical interviews—and we had to get out a dictionary so I could understand what he meant, and then — "Oh, yeah, I had that three years ago in Afghanistan."

He told me that my problems were a chronic condition that you sometimes see in people who've had a severe case of giardia, and he prescribed medicine for me, an herbal mixed tea. I can still remember them bringing out the prescription because it was a bag like this big, and I'm like, "Oh, my God, am I supposed to eat *that?*""Oh, no, no, we make a tea out of it." And this Chinese geologist, who was really very paternal with me, wanted to make sure I got my tea, and it tasted like hell. Oh, God, it was awful! And it cleared the problem up.

Aspaturian: That's an amazing story. Did you ever find out what was in the tea?

Jones: I had the prescription, and in fact I actually tried to get it filled here about five years later when I started having a much milder problem.

Aspaturian: A recurrence?

Jones: A recurrence. I did try to find an herbalist who could fill it, and I couldn't. I do remember that licorice was in it; most of the other things I didn't recognize.

A meditation on Chinese given names, including her own

And I'm thinking of this geologist, Deng is his surname, and given name QiDong, which means "rising in the east," because his parents were good Communists.

Aspaturian: Yes, I've heard of this tendency during the Cultural Revolution to give mainland children these patriotically themed names.

Jones: Historically in China you didn't have a name as we think of them. There are a few stereotypic ones, but it's almost as if it's trite to have a name that is recognized as a name, if you will. There's also a tradition that the first character in the name would always be the same across a single generation. So my grandparents tried to do this. They wanted to have all the children have names starting with De, which means virtuous. My uncle was named De-Yi: The meaning is sort of "virtuous virtue," and then De-An—"virtuous peace"—which was my dad's name. But before they'd chosen a Chinese name for Uncle Phil, Chinese friends had given a gift for the baby using the name that's used for Phillip in the Bible. So they felt like they had to stick with that, and he didn't get one of these "virtuous" names.

And my grandfather named me Guang Yueh, which is "Brilliant Moon" because Lucy means moonlight and "brilliant" means either shining or intellectual brilliance.

Aspaturian: That's a lovely name.

Jones: It's a lovely name.

Aspaturian: Was this given to you as an infant?

Jones: No, it was given to me when I went to Taiwan at fifteen.

Aspaturian: I see.

Jones: To put on my documents in Chinese. And I really like the name. The "Guang" is considered a very masculine character, though, because its meaning is "intellectual brilliance." I've had Chinese criticize it, saying, "Oh, that's not a good name; that's too masculine." "It was given to me by my grandfather; and you wouldn't criticize somebody else's name." I'd hear that sort of thing.

It's an intellectual process by which you choose those names. When I was first published in Chinese, I got them to put down my name as Zhang Guang Yueh, which is the surname my grandfather took when he was in southeast China, where it's pronounced "Jiong." So it was much closer to Jones. But the article says Zhang and in parenthesis, the American Chwun-si because Chwun-si is what they use for Jones. [Laughter]

Aspaturian: They were very scrupulous about how they rendered it.

In Beijing in August 1980 with MIT advisor Peter Molnar and (from left) Chinese colleagues Jiang Pu and Deng Qidong, just after coauthoring an article for a Chinese journal. *Photo courtesy of Lucy Jones*

Chinese seismology evolves away from Cultural Revolution's citizen-science model

Reading up on seismology during Mao era, I found an article that talked about how Zhou Enlai had a vision that basically amounted to crowd-sourcing seismology in China. Did you encounter this to the extent that you were aware of it?

Jones: All right. That's one way to put it.

Aspaturian: I'm paraphrasing a little.

Jones: It is a way of looking at it, and that's part of it. I personally believe that his fundamental goal was to protect scientists from persecution.

Aspaturian: As you said. [*Session Two*]

Jones: And that was one way to say, "Alright, they're doing something for the good of the people. Predicting earthquakes is an obvious benefit for the people, so we'll allow this to happen; we'll support it." But at the same time, given what was going on in the Cultural Revolution, you had to demonstrate that you weren't one of the Stinky Nine—that you weren't being that arrogant intellectual. So doing the crowd sourcing—we also call it citizen science—gathering data from the peasants, was explicitly put out as learning from the peasants, which was a social good under Chinese communist ideology. And so it was a very important piece of demonstrating that you were not one of the Stinky Nine.

Aspaturian: He was a very subtle thinker, Zhou Enlai. Very clever approach, actually.

Jones: It is. Yes. And it is interesting that among the people I worked with, this was something that shifted. As time went on and things relaxed, there was more disdain for the citizen science.

Aspaturian: I was going to ask you about that.

Further investigations of anomalous animal behavior as potentially predictive of earthquakes

Jones: There was a certain sense that it was politically required rather than scientifically valid. That said, I wrote papers on that data, and that is what I was looking at. And that was where the fraud that I described last time [*Session Two*] occurred—in how that data was put together and collected, with chunks that didn't fit thrown away and other parts made up. There was something there, but it's very noisy data, because, as I think I told you last time, you'd see these big peaks every Saturday afternoon, right after the morning Party meetings where people were urged to go out and find and turn in more reports.

Aspaturian: The data was politically driven on those occasions.

Jones: Right. And I think a lot of these reports were made up because you could, right? There was essentially no control on this. But I thought, "There's something in there. There's still a signal, but it's very noisy and hard to really interpret. There's enough to suggest maybe it's worth doing a real study where you actually have controls."

After the reports from China came out, there was a great study done in the late '70s on animals in central California, where the researchers tried to put controls on the data gathering and reporting. They went to farms along the creeping section of the fault—

Aspaturian: This is the San Andreas we're talking about.

Jones: The San Andreas, along the part where there tend to be moderate earthquakes on a regular basis. They enrolled a bunch of farmers, and said, "Tell us what your animals are doing, and we aren't going to count your report if it comes in after an earthquake. We want to get the baseline." So they set up this system of trying get regular reports every week. Because otherwise you have an earthquake, and afterward you'd hear "Oh, what about how weird the horse was acting yesterday?"

Aspaturian: The hindsight interpretation is not so good.

Jones: To try and get around that is huge. So they tried to get these farmers to turn in reports every week or every month. If you hadn't turned in a report until after the earthquake happened, it wouldn't count.

Aspaturian: Right.

Jones: They ran the study for a few years, but when you run this kind of thing under grants, you want results, and in this case the results were that there weren't many earthquakes, and the ones that did happen didn't provide any clear signal. Eventually they had to stop; there clearly wasn't anything definitive coming out of that. And there wasn't a physical model.

This was especially true with the Haicheng data more than Songpan. There were something like 500 foreshocks that happened over a four-day period, and in that period there were a lot of water anomalies recorded: artesian wells overflowing, bubbles in the wells, muddying of the wells, as well as a lot of snake reports. In Manchuria in February, the average temperature is like minus 10 or 20 C. There was one report that included a picture of a snake that had come out of hibernation and frozen to death. The front was frozen, and the back was still moving was what they said—you can't tell from looking at the picture.

Something driving a snake out of hibernation seems significant. It could be that there was some migration of groundwater in response to the foreshocks, the faulting that was going on, and maybe that was flooding the snakes' dens.

Aspaturian: I would think it's very hard to tease out proximate cause and effect in a situation like this.

Jones: If I were to go and study this, I wouldn't go and study snakes; I'd try to look at the groundwater.

Tangshan earthquake disaster catalyzes interest in foreshock analysis & human-focused seismology

Aspaturian: I read up a little on the Tangshan earthquake after we talked, and I gather it was just a devastating, devastating event. You went up to study it?

Jones: I never got to go to Tangshan. Our proposal was to go study Haicheng, which they wanted to show off because of the predictions. I don't think they allowed foreigners into Tangshan until like the mid-'80s.

Aspaturian: Probably because the damage was so widespread?

Jones: Yes. The reports I've heard suggest that two buildings stayed up out of a city of a million and a half people. The early estimates, and even when I was there, were for 600,000–700,000 dead, about half the city.

Aspaturian: That's what I saw.

Jones: However, the Chinese now say 200,000.

Aspaturian: I saw that, too.

Jones: I wrote about this in my book. I have a chapter on Haicheng and Tangshan and my experiences of doing this work. You might want to read that if you want more details.

Aspaturian: I probably do; what is the title?

Jones: *The Big Ones: How Natural Disasters Have Shaped Us (and What We Can Do about Them)*. [see *Session Ten* *for a fuller discussion*] I mean, given was what was being said when I was there, I do think the death toll was something like at least half a million. We have a family story, and this is in the book too: At the time my aunt was living in Hong Kong, and she had some very close friends there—husband and wife—who were from Tangshan, and their extended family was all in Tangshan. Almost the entire family died in the quake, but his mother survived, so they were then called up to Tangshan to pick her up because China doesn't really have Social Security for the elderly—your family's supposed to be taking care of you. There was an uncle in the wife's family who was traveling, so he lived. All the family was in a 10-story apartment building that collapsed. The mother who lived was in the infirmary which was on a first floor, and she jumped out a window. There were two nieces on the seventh floor when it collapsed, but they had been taught in school how to make airspace and so as the building was settling, they tried really hard to keep air around their heads, and they were eventually dug out alive. All the rest of the family was gone. So, yes, it was a devastating event. I wasn't allowed to go there.

Aspaturian: I'm sure you heard about it.

Jones: Deng Qidong told me how at that time, before Tangshan, there would be a meeting every six months to discuss "prospects" for earthquakes. You're ordered to predict earthquakes, but you don't really know what to look for, so how do you do it? Well, you get everybody together, look at what you have, and you say, What can we report to the authorities about this? There was a phenomenon going on— a series of magnitude and 6s 7s over that time that seemed to be part of a migration toward the northeast. There were the Xingtai earthquakes in

’66 near Beijing, then there was a ’67 earthquake somewhere along there, in Hejian, and then there were magnitude 7s in Bohai Bay, Tianjin, and so this seemed like—

Aspaturian: They thought they could discern a pattern?

Jones: Yes. It was sort of like imagining you had an Imperial Valley earthquake, followed by something in San Ysidro, and then something in San Bernardino, and— what do you think is going to go next? If were to have something that happen now over three years, after not having had many earthquakes, what would we think? We’d be worried and we’d be looking in that direction.

Aspaturian: Right.

Jones: So Haicheng was a direction to be looked at. Tangshan was sort of back by Bohai. So you had this general sense of “Oh, we’d better look around here,” and then you get the Haicheng earthquake and the meetings afterward in early ’76, where the question was, *Is it over?* At some point these migrations stop—how do you tell if they’ve stopped? So there was an area of heightened concern that included Tangshan but also included the homes of a few hundred million people in Tianjin and Liaoning provinces. And then they got a monitor’s report —there were monitors in each county. Each county would have people who probably didn’t have a high school degree, but who would give reports to the provincial seismology bureau, which would send information to Beijing.

And so through this chain came a report that some wells had gone artesian in the Tangshan area. And I asked Deng Qidong "How often do you see that sort of thing without an earthquake?" He said, "Plenty of times, but, still, it's something you take notice of when it happens." So they had been having some sort of meeting in Beijing, and there were two scientists from the provincial bureau heading home who decided to stop in Tangshan to check out the reports. They got in late at night and stayed in a hostel; the earthquake happened early in the morning, and they were killed. So that was the story I heard from colleagues; I've never seen it anywhere written or official. There's the real reality that they could talk about with me, but what's hard with the government, which wanted absolute certainty, is that there are times when you know there's an increased risk—

Aspaturian: But you can't quantify it.

Jones: You can't quantify it, and probably the absolute risk is still pretty low.

Again, hearing about all this and seeing the consequence of it, and being part of these discussions, is part of what got me into statistics: We need to be able to quantify this, and we need to be able to determine how often these things happen when we don't have a report. That was one of the problems in China: If nothing happens, no one saved the data. So our picture was very incomplete in terms of understanding how often these phenomena would be seen without them.

Aspaturian: Yes, at that time.

Jones: At that time.

Aspaturian: Well, they were starting from scratch in a way, I suppose.

Jones: And also you couldn't say, "You guys have to go to graduate school to do this right." This was the Cultural Revolution when there wasn't any education. You had to be quiet about talking about any sort of systematic data collection because you're all supposed to be learning from the people.

Aspaturian: Right, right.

Jones: And people really want to find patterns about earthquakes, whether they're true or not. We don't have internal fact-checkers.

Aspaturian: This brings me to what you were saying after we turned off the recorder in the last session. You said that your experience in China was what made you realize that earthquake prediction was not just a scientific problem, but a human and a social one. So let's talk about that.

Jones: It was all of these things we've been talking about: You've got information that tells you there's some increased earthquake risk, but it's probably not a high risk; and then to actually do something about it, you have to take actions, and those actions have consequences and costs. I saw that there were two big differences between China and the US. One of course is that China was primarily an agrarian economy at the time. Evacuations requiring people to be outside did not have a big impact on the economy, whereas in LA, for instance, it would be a huge impact. The other difference was that politically, Chinese officials could tell people to go outside and if nothing happened, they could still get away with doing it the next time. We didn't have that luxury.

But also, of course the chances of dying in an earthquake in China were much larger. In Tangshan, 700,000 dead, and we think our worst California earthquake resulted in maybe a couple of thousand dead.

So there's all of these other factors, which became really obvious to me when I looked both at the kinds of data the Chinese were seeing and interpreting and at what the consequences were for their society of acting on those data. And of course given the political situation in China at that time, the consequences of *not* trying to predict an earthquake were really significant.

Aspaturian: You visited China four times in a period of about four years. When did your thinking start to coalesce around what you've just described?

Jones: It was a process through all of this.

Aspaturian: I imagine so.

Jones: It's reflected in some of my thesis work. I have four papers in my thesis: two on mechanics, two on foreshocks. One of the foreshock papers was this global study that I did before I went to China. The second was a detailed analysis of the Haicheng earthquakes, and it was really about their physics through observations of how the faults were moving. I was able to get good enough locations to be able to delineate the first time the faults that were moving in those earthquakes. And what was interesting about this was that the foreshocks actually delayed the main shock. It was like the main shock was starting to get going, and then the stress change from the foreshock actually tended to clamp the fault that was about to go. So it was more that the main shock was delayed, rather than promoted.

Aspaturian: You were able to determine all that?

Jones: Yeah. It was pretty cool.

Aspaturian: I bet.

Jones: This was basically opening up a different way of looking at foreshocks. None of what I did was really technically difficult. It was all about asking the right questions.

Aspaturian: And also for seismology, a whole new environment, given that China had been hermetically sealed off.

Jones: There was that and being able to get access to earthquakes that nobody had previously looked at.

Aspaturian: All that data.

Jones: All that data. So that part was really big. So the '79 and '80 trips are while I'm at MIT, and then the next two trips are while I was a postdoc.

Aspaturian: This was at Columbia.

Postdoc at Columbia & meeting Egill

Jones: I went to Columbia. So after my first trip to China, I was "the woman who'd been to China," and I was invited to a conference—a Ewing Symposium—on earthquake prediction. Morris Ewing was a quite famous guy in plate tectonics, and the Lamont-Doherty Geological Observatory is the earth science group at Columbia. They're outside the city, up on the Hudson River, and they had their own ships and their own dock on the Hudson River, and they're located on a former estate up there [Torrey Cliff], which was a lovely place to live. They had these symposiums every couple of years—there was some fund for doing it. The one in 1980 was on earthquake prediction, and all of the people who were speaking were professors except me. I was the only graduate student speaker because— I'd been in China.

Aspaturian: Of course.

Jones: There was also a delegation that ended up coming from China for that.

The grad students at Lamont were brought in to do the audio–visual production. Back in the days before computers, it made it easier to have help for this, you know. So they were there to do some of the service work and have a chance to listen to the talks. And my [future] husband was one of those students. So we met at that symposium—

Aspaturian: That's how you and Egill met.

Jones: We had actually met at an AGU [American Geophysical Union] meeting the December before, and it was just one of those situations where someone who knew both of us was getting together a group of people to go to dinner. So we were in this group of like twelve people and happened to sit next to each other and flirted a bit. So when I saw the participant list for the symposium, it was like, "Oh, that cute Icelandic guy whose name I can't say is going to be there." [Laughter] But then we started seriously noticing each other at that meeting, and he came up to visit me in Boston the next weekend, and we went to another AGU meeting together, and then I had to go off to China. He was going to California to do fieldwork for the summer, and we got to know each other by letters.

Aspaturian: Was that the work on radon that you mentioned? [*Session Two*]

Jones: Radon, yes, and he also did strain meters; it was all volcano monitoring of various sorts. There was an active eruption going on for years at the Katla volcano up in northern

Iceland, so he had put out a lot of instruments there and was going back to work on them.

So anyway, my work in China sort of made me more visible, and then I started dating Egill and going back and forth between New York City and Boston. He wanted to change fields, so he wanted to stay at Lamont because that way he could switch to doing seismic networks instead of continuing with radon. And so I applied for a postdoc at Lamont and got it, and the third and fourth trips to China were while I was a postdoc.

Resumes research in China & scrutiny of animal behavior before earthquakes

Aspaturian: I noticed that maybe ten of your early papers dealt with the research you'd done in China.

Jones: Yes! First the work on the Haicheng foreshocks; then we did some Haicheng aftershocks. Then there was the Songpan earthquake, and we did a seismological analysis of it, even though we had to walk away from the animal anomalies. I had a paper on the Haicheng animal anomalies that was presented at the Ewing symposium. Actually, I guess it wasn't at the symposium, but it was in the volume that came out of the symposium.

Aspaturian: Was this before you realized the data was fraudulent?

Jones: That's not the same; this is the Haicheng animal anomalies as opposed to the—

Aspaturian: Oh, that was Songpan, that's right. I've got it straight now. Okay.

Jones: Haicheng is where we see the spike in anomaly reports after each Party meeting, but we also see—

Aspaturian: The frozen snake.

Jones: The frozen snakes.

Aspaturian: That must have been an interesting paper to write, teasing out these different variables.

Jones: I started with translating all the reports, which are all handwritten from the communes and compiling them and putting each of them on a three-by-five index card. I did that one with one of my professors, Peter Molnar. He came over that summer. I can just remember that with Peter, there were all these reports of oddly behaving chickens and dogs; chickens in trees. And we were asking, Is it really that unusual for a chicken to fly up into a tree? [Laughter]

Aspaturian: These are the deep questions you got into.

Jones: Chickens and dogs turned into kitchens and gods [laughter] as we got flippant. A lot of it was just straightforward — making histograms and spatial maps and seeing what correlations there were, and that the correlations were ambiguous but not zero. There was sort of the seismological aspect— I tried to look at the faults and understand the structures, and then there was the other part about how to look at these prediction issues. Because by now the Chinese were backing away from saying "we're predicting all these earthquakes."

Aspaturian: The political pressure to do that was dissipating.

Jones: Yes, that was easing, and there more exposure to the West and Western science, you know, so it became a more nuanced story.

Statistical work on foreshocks & early publications; US seismologists react to Chinese earthquake "predictions"

But as they were backing off, I was thinking, "You've really got something here: there's a real probability gain, clearly. You're a lot more likely to have a big earthquake when you've just had a magnitude 5 than at ordinary times."

Aspaturian: So this had not been really looked at in the West before?

Jones: Not as predictively.

Aspaturian: How interesting. We're so accustomed to it now, to hear you say this is—

Jones: Actually at the time, there was really no one in the U.S. doing statistical work. Obviously, there was quite a group working on it in Japan.

Aspaturian: That makes sense.

Jones: You've got the Gutenberg–Richter relationship, which is a statistical formulation, and Omori's law, which is out of Japan, on how the rate of aftershocks dies off with time. And if you look at the early history of Caltech seismology, you've got some of that with Harry Wood, who argued for putting out a seismic network in southern California because he said we need to know where the small earthquakes are to know where the big ones are.

Aspaturian: Yes, that was his driving motivation.

Jones: But then you also have [Charles] Richter [professor of seismology, emeritus; d. 1985], who was famously quoted as saying earthquake prediction is the province of charlatans and fools. And then with these claims coming out of China, there were also reports out of the Soviet Union that looked like predictions, and we really jumped on that. The Alaska earthquake in '64 did a lot of damage and got the USGS pulled into doing earthquake work, and that was when it set up its first office in Northern California. And when the San Fernando earthquake happened [1971], that's actually when Congress told USGS to come down to Southern California as well—we can get into that story—and then there started to be a push to try and get funding for the prediction work. "The Chinese are predicting earthquakes! How can we get left behind?!"

So NEHRP, the National Earthquake Hazard Reduction Program was first passed in 1978, and the driving forces behind it included quite a few people from Caltech. Frank Press, who had been the seismo lab director here before he went to MIT and then became the president's science advisor, was a big push behind it. And there really was a lot of justification: The thinking was, *It looks like the Chinese are predicting earthquakes; We've got to figure out what's going on.* And that environment sort of swept away the skepticism. Haicheng happened just as I was starting graduate school, and this is when Peter Molnar, who became my thesis advisor said, "There was just this earthquake in China, so why don't we get you studying foreshocks? Then if China ever opens up, we can get you over there." So in 1976, we published our paper in *Nature* on foreshocks and how often do they happen. [*Session Two*] It was really just a counting exercise, and it got into *Nature* because people hadn't done it before.

Aspaturian: No one had done it?

Jones: Not systematically. And then I did the bigger, systematic global study on how often foreshocks happen and what characteristics control that and how does this relate to tectonic regime and so on. I think that's the first paper really trying to compare the rate of foreshocks around the world and looking at different types of regimes. But there was a guy in New Zealand, David Vere–Jones, who developed important statistical formulations for these, and then this group working in Japan, but otherwise, in the West there wasn't much.

So I started trying to do this, and the main paper that I wrote on California when I was at Lamont was just really systematically going through the history of California foreshocks and where they occurred, and how did they compare to one another.

Aspaturian: What year was this?

Jones: That was published in '84.

"We were looking at the wrong problem & asking the wrong question"

Aspaturian: Okay, so that was after you had left the East Coast and moved to Pasadena.

Jones: Yes, but I worked on it while I was still at Lamont. And then we came out here, and I had this realization: *We're looking at the wrong problem and asking the wrong question. We shouldn't be asking how often foreshocks are preceding big earthquakes, we need to look at how often the big earthquakes follow the small ones.* It's interesting how hard people find it to understand the distinction. There are always way more small earthquakes than big ones.

Aspaturian: Yes, and they're not all foreshocks either.

Jones: Most of them aren't, right. So if you have some number of small and big earthquake pairs that are foreshocks and main shocks—you've got ten of them in a group and you have twenty big earthquakes so half of them are preceded by something smaller. But if you have twenty big ones, you've probably got 2,000 small ones. And then you've got ten events, and it's only 5 percent that are followed by something bigger. So the same basically, depending on what bigger and smaller means—

Aspaturian: That was going to be my question: How do you distinguish quantitatively?

Jones: It's all by relative to the magnitude.

Aspaturian: I see.

Jones: That's one of the things I did early on, showing that as far as we can tell, the characteristics are not dependent on the magnitude: It all scales with the magnitude of whichever event you're looking at: Six percent are followed by something bigger than themselves. Or 50 percent are preceded by something smaller than themselves. Those numbers are actually controlled by our completeness thresholds. So if you can't see the really small earthquakes—say, a magnitude 5 preceded by a 1, and you don't record the 1s, they're not included in the count.

And so the fact that we're only getting a complete count at magnitude 3 is part of what controlled our saying that approximately half of them were preceded by something smaller.

People always get confused by this, you just have to take a moment and say, what direction are we looking at? If you want prediction, you've got to be looking forward. And yet most of the time because we define the earthquake sequence by the main shock, we're looking backwards. And we have to—

Aspaturian: Turn our thinking around.

Jones: The first paper I wrote based on research here, was "Foreshocks and Earthquake Hazard Assessment in Southern California." It originally was going to be named "Foreshocks and Earthquake Prediction in Southern California," but by this time earthquake prediction was starting to look so bad that I changed the title. And it just basically said that if you've got an earthquake, you have a 6 percent chance that it's followed by something bigger. And nobody had done that before, either. Since then, earthquake statistics has become both a much bigger and more complex field. It has its own conferences now and much more complicated statistics; you don't get away with dividing one number into the other like I was doing early on. It involves trying to do the math properly so that you really incorporate all of the different factors.

Marriage & experience as sole female seismology postdoc at Columbia

Aspaturian: How did you happen to join the USGS? This would have happened after you finished Columbia; or is there anything else from Columbia that you'd like to talk about?

Jones: So we were there as postdocs.

Aspaturian: Did you get married when you were there?

Jones: Yes. I came back from China: Egill's in New York; I'm in Boston. We got together every weekend. He either drove his car up, or I flew down. I owed $500 by the time I graduated, spending all that money on Eastern Airlines. It was really straightforward: "I can finish my PhD; I'm not going to move down before I'm finished. I'm not going to walk away from it, so when I finish my degree, I can live In New York, and we can get married." I probably got my degree done six months earlier than I would have because I had all the incentive in the world. [Laughter] I would just work like crazy all during the week.

Wedding day, September 1981, with groom Egill Hauksson and train bearer Chris Bilham. Having recently seen Lady Diana's televised wedding, "when he saw the train on my dress, he latched on and stayed there the whole reception."
Photo courtesy of Lucy Jones

We ended up getting our PhDs five days apart because we were both facing the same incentives. Then I moved down to New York and got this postdoc at Columbia. We actually hadn't originally planned this, but it turned out that my aunt who lived in Hong Kong, with whom I'm close, was coming back to the States for a sabbatical, so we scheduled the wedding to happen while she was here, which was in September, four months after we got our PhDs. So I spent two years at Lamont as a postdoc. It was a time when the Columbia seismology group had never graduated a woman in seismology.

Aspaturian: Were you the first female postdoc there?

Jones: I don't think so. I think I was the only one when I was there. There was one female grad student who left the department and went over to marine geophysics. So there was a time when it was not really conducive to me to be there; I didn't like it.

It was really important for Egill to be there because it allowed him to go work on the Shumigan Seismic Network in the Shumigan islands of Alaska—so really to change his research direction. I got these cool papers done. I had thought I was going to do more rock mechanics—there was a rock mechanics lab there—but it was another place where it wasn't the most congenial environment.

Aspaturian: Did you find Columbia in this regard less congenial than MIT?

Jones: Yeah, I did.

Aspaturian: What do you think accounted for the difference?

Jones: Individuals. Probably we don't want to be naming names in this sort of situation, but I didn't enjoy it, and also Egill and I were looking at, what's our future, right? We could have stayed at Lamont—as long as you raise your own money, you get to stay there. But you had to raise all your own money, which is very stressful. I don't like having to write that many proposals.

Critique of validity of Chinese earthquake predictions stirs up controversy

Aspaturian: Do you think some of your senior colleagues felt intimidated by the fact that you had done this work in China?

Jones: All right. There was another issue that came up toward the end of our stay. Before we went to China and uncovered this fraud, I had been asked to give a speech at AAAS, which for a postdoc is a pretty big deal.

Aspaturian: This would have been 1982, maybe?

Jones: Eighty-three.

Aspaturian: Early '83?

Jones: Early '83. So I was asked in '82, and then we go off to China, uncover the fraud, and I'm giving the talk like a week after I get back.

Aspaturian: You were asked to talk about earthquake prediction in China, I presume.

Jones: Yes. It was by far the highest profile situation I'd been in, and, looking back, I was a little naïve. I really didn't think about the implications of being repeatedly asked, *How are they doing it?*

I didn't say the Chinese made it up, and I didn't talk about fraud, but I did essentially say—at least, I was quoted as saying—that it was based on guesswork. What I was trying to say was that they have the same incomplete information that we do, and they are willing to act on it when we aren't. It's not that they can predict it; it's that they're willing to take the risk, they're willing to take a chance that a prediction might be correct.

Aspaturian: Which is slightly different, yes.

Jones: So then there was a headline in the *International Herald Tribune* saying, "Chinese Earthquake Prediction Based on Guesswork," also New York Times, which almost certainly got people in China who knew me into trouble. And then there was all this activity going on with NEHRP, which had been funded five years earlier. Most of us in seismology were getting grants out of NEHRP, and a lot of the justification for NEHRP was that—

Aspaturian: The Chinese were doing it.

Jones: So one of the professors at Lamont took me apart for having said this.

Aspaturian: Publicly or privately?

Jones: At the lab. Not like in the newspaper or something but in front of other people at the lab, and it was pretty hard. He really attacked me. He was really upset, saying that I was destroying

NEHRP; I was going to undermine it: "You can't possibly say that; you've got to be saying earthquake prediction is our goal, or we aren't going to get funded." I'm sort of saying, "You want me *to lie to get funding*? That doesn't seem really scientific," and he got even more upset at me for saying that. And then there was another professor who clearly had issues with the first one and who seemed really into backing me, partly because I was arguing with this other guy, so it was a very unpleasant experience at a young enough age that I wasn't used to those sorts of politics.

Aspaturian: I was going to say, was this your first direct exposure to academic politics and in-fighting on this level?

Jones: By far, and the real shock was "you would think of misrepresenting information to get funding." That was the way I interpreted it, and I found it pretty upsetting. So it helped inspire us—

Aspaturian: To go elsewhere.

Attracted to SoCal/Pasadena USGS by research & public service opportunities

Jones: To get out. Though by the time that happened, we'd already been applying to some places in California. Also, I grew up in California, my family's here, and we had discussed that I loved the idea of living near my family. I was also quite willing to think about going to live in Iceland, and my husband's attitude was, "I think I like the idea of living with your family more than I like living with mine." [Laughter] And, you know, just comparing the opportunities: Iceland at that point was 200,000, 250,000 people in the whole country. I mean, they're at around 350,000 now, but it's a tiny place.

Much more than that was that Egill knew lots of people who married Icelanders and tried to live there, and within five years, they're either divorced or they've left. It's too insular a society and the foreign spouse never fits in. He felt quite strongly about that: "You're worth too much; I'm not willing to risk it by having you in Iceland." So then we look at where we can go in the U.S., and California is the place where you can get two jobs.

Aspaturian: Was that because of the active opportunities in seismology?

Jones: Yeah, yeah. USGS was still hiring then, and I wanted to go there because of the earth science in the public service aspect.

Aspaturian: So you were not interested in an academic job particularly?

Jones: Sort of. I thought about it and decided against it for two reasons. One was the idea that with USGS I would be doing research for the public good. I think that for all the problems in China, they were doing it for the public good. And the idea of using science to make society better really inspired me. And it seemed like that was possible in the USGS and much less so in an academic situation. In an academic setting, your job is to teach the students. Maybe you do more good in the long run because your students go out and do all those other things, but you're more divorced from it. So that was one factor. I also discovered while being at Lamont that I hated writing proposals. I was successful at it; I was actually very proud of the fact that I had to give back $200,000 in grant money when I took the USGS job. I had successfully gotten all of these grants that I could have stayed there and used, but I really liked the idea that I didn't have to write grants all the time.

Aspaturian: Did your experience with this AAAS talk and the repercussions affect your thinking on the whole grant proposal environment, too?

Jones: Yeah. The whole thing made it—

Aspaturian: Unsavory.

Jones: Yeah. That's right. Egill had gotten a bunch of grants too, but he could take them with him to USC.

Aspaturian: He got an academic job at USC [University of Southern California]?

Jones: Yes, he got a research professor job at USC. At the time, the seismic networks in California were not in any way as unified as they are now. There was the Caltech network, but there was also a USC network, which had been primarily built with petroleum industry money around LA. Because people forget that the Los Angeles basin is an active oil field.

Aspaturian: You still see the drilling occasionally.

Jones: It's one of the biggest oil fields in the world, at least in the U.S. It was a big deal. And earthquake monitoring was mostly done by USC. So USC had this dense network of stations in the Los Angeles basin, and the Caltech stations were mostly around the basin. Caltech tended to stay out of noisy urban environments. And then there was an agreement between Caltech and USC to send some stations over so that they could exchange some data from different stations to be able to share information.

Aspaturian: So there was a collaboration of sorts going on at the time.

Jones: There was a collaboration of sorts, yes. And Egill came and was working with this data and really worked it up. His research ended up focusing on the seismic tectonic structures of Los Angeles because he had all of this data. He wrote seminal papers on what's really going on under the basin, seeing that clearly. And so we came in late '83, started getting papers out—'84, '85, '86—and then in '87 we had the Whittier Narrows earthquake.

Aspaturian: I remember it well.

Jones: And in '88 we had the Pasadena earthquake.

Aspaturian: I remember that well, too.

Jones: And in 1990 we had the Upland earthquake. And 1989 we had the Malibu earthquake. In 1991 we had the Sierra Madre earthquake

Aspaturian: I remember that multi-year swarm very well.

Jones: Then of course Landers out in the desert. And so we had this period with a lot of earthquakes in LA, and Egill's work was very much in the forefront. And partly because it was him and me, the Whittier Narrows paper was USGS and USC working together.

First impressions of Caltech & initial interactions with campus seismologists

Aspaturian: What were your first impressions of Caltech when you came here? How closely did you interact with the seismologists?

Jones: I've always interacted very closely with the seismo lab. Much less so with the rest of the Institute. Just before I came—in '82, I think, or maybe it was early '83, the USGS started renting a house on Wilson from Caltech. Until then, our people had all been in the seismo lab. So there were two scientists, Tom [Thomas] Heaton [professor of engineering seismology, emeritus], and Caryl Johnson. He was Carl Johnson then, and she became Caryl Johnson. She had been a graduate student at Caltech and was hired by the USGS to run the seismic network here, and then Tom, who had also been a Caltech graduate student, got hired. I was the first scientist coming into the office who hadn't been a Caltech graduate student.

Aspaturian: But you were fluent in Mandarin. That must have made up for it. Caltech's thinking probably ran along those lines.

Jones: [Laughter] I was in on a postdoc—the National Research Council [NRC] had postdocs for the USGS. So my supervisor was in Menlo Park, but I was stationed out here. That was an interesting time when there wasn't a lot of local earthquake research going on. So Hiroo Kanamori [Smits Professor of Geophysics, Emeritus], being Hiroo and brilliant—you know, he does everything—was interested in local earthquakes, but he was also famous for a lot of his global stuff. Don [Donald] Anderson [McMillan Professor of Geophysics, Emeritus; d. 2014] was a classic deep earth seismologist: If it's shallower than 400 kilometers, who cares?

Aspaturian: Not interested.

Jones: Yeah. And then Don [Donald] Helmberger [professor of geophysics, emeritus], like Hiroo, studied earthquakes everywhere, including local ones. What I did is seismo-tectonics—really looking at local earthquakes with the dense network, which is now being operated out of here. At the time, nobody was using the network that the USGS was creating down here.

Aspaturian: Really?

Jones: Yeah. We were going computerized, and Caryl was much better at writing data recording systems than data distribution systems, so it was really hard to get the data out. And actually the USGS told me that part of my job was to get data out of CUSP—the Caltech USGS seismic processing program that Caryl had written. We used to joke about it being—do you remember ROM, read-only memory on early computers?

Aspaturian: Oh, yeah.

Jones: We joked that she made WOM—write-only memory. So my job was to get the data out. There was tension because she was going to create this wonderful new system, but she kept on never getting it done. I finally said, "I'm going to go and work in China again." I do remember that when I said this, the supervisor in Menlo Park said, "No you're not. We gave you a job; to get into that data. If you want to stay, that's what you gotta do." I mean it was actually a showdown like that. He very explicitly said it.

Aspaturian: This was 1984-ish?

Jones: Yeah. I mean my foreshocks and earthquakes hazard assessment paper was with the catalog, so the catalog was available but looking at seismograms was almost impossible. The seismograms had started to be recorded digitally, but no systems existed at Caltech to look at them. Now, Egill had systems to look at them; they were actually written out of Lamont, I think. He and I worked together to get the CUSP data into this Lamont system.

Aspaturian: I see; that's how you did it.

Jones: Oh, it made Caryl furious with me because, "I was going to get this to you!" "You've been saying 'two' weeks for the last six months." So I come up with this workaround. She always got it done a few weeks after I'd gotten it some other way. There were several different systems that happened then. So I was definitely the prod. The guy who told me it was my job to get the data out knew what he was doing. So that was—

Aspaturian: An interesting baptism by fire.

Jones: It was a difficult time. Caryl ended up leaving in '86, and Tom Heaton became scientist in charge in her place, just as we were starting to have all those earthquakes. By then we did have systems for looking at them. I really focused on looking at the local earthquake data here, as did Egill. By helping me figure out how to get into the data, he could also get at it.

Aspaturian: Did you have any pangs giving up the work focused on China to shift directions in this manner?

Jones: Not at the time. After being there in '83, I didn't want to go back to China. Running into that fraud and just realizing the challenges and the complications at that point in Chinese society I didn't want to deal with. I moved off; I wanted to get this work done. It became more relevant to be here. At some point I did start thinking about going back. USGS kept on talking about sending me back because there continued to be exchange programs, and once we broke through the CUSP data logjam, they were more interested in sending me over. I seriously thought about it in early '89 when there was the Beijing Spring, and it looked like there was going to be—

Aspaturian: But led instead—

Jones: Which led to Tiananmen. Before that, I was starting to think that maybe I could go back and maybe things were really changing, and I can still remember watching on a small TV what was happening in Tiananmen and crying. "Okay, I'm not going back. I can't go back. I can't be part of this now." And then by the time I might have shifted again. I had kids and was just too busy to try and take off that way.

Recollections of late MIT mentor & 2014 Crafoord laureate, P. Molnar

Aspaturian: I have one other question for you. This actually goes back to your MIT advisor Peter Molnar, whom I looked into; and I see that he won the Crafoord Prize in 2014.

Jones: Oh, right.

Aspaturian: For his work in tectonics, which I thought was interesting because you said he was originally hired because MIT was interested in his wife's work in tectonics.

Jones: Girlfriend.

Aspaturian: His girlfriend. I just wanted to ask what you remembered about him as a colleague.

Jones: Oh, Peter was wonderful. He's still wonderful. He does have cancer now.

Aspaturian: That's too bad.

Jones: He wrote to a bunch of his students recently, wanting to tell us himself. He's a fantastic guy. Brilliant and obsessive.

Aspaturian: That is often necessary for extraordinary success in these fields.

Jones: He came up for tenure while I was at MIT. In his seven or six-plus years on the faculty—or maybe this included graduate work—he had 93 publications. And he refused to be considered for tenure. He didn't believe in the system, and it was quite a conundrum for MIT because they couldn't keep him if he didn't have tenure. They were quite at loggerheads. The agreement they reached is that he accepted tenure and gave a letter to the department chair, resigning effective three years from the date of the letter and left it undated. So that the chair could at any point write in the date and give him three years, which is what you would have if you didn't have tenure. And he insisted on doing that before he would allow himself to be considered for tenure.

Aspaturian: Knowing him as you did, did this come as a surprise?

Jones: Not in the least. He had principles, and he followed through on them. He also told me a story; shall I share it? Sure; he's told it. His father had been the vice president in charge of research at Bell Labs, and when he told his father that he'd been offered a professorship at MIT, his father's response was, "Well, they must have lowered their standards." Which might explain why he was as obsessive as he was. And he really cared about his students. He was very deeply emotionally invested in us. To the point where he couldn't keep on doing it. A few years after me, he had a group of students that all had rather significant emotional needs—they weren't the most easygoing people—and he ended up saying, "I can't do it."

Aspaturian: It was too much.

Jones: He resigned as a professor. He then became a researcher at Woods Hole for about a decade before he was hired at the University of Colorado. As a professor he really cared about what we did. But he was also this obsessive guy. So there was a point where I just felt like he was criticizing everything I was doing to the point where it didn't seem like our relationship was working.

Aspaturian: While you were still a graduate student?

Jones: While I was a student of his. I had already started doing lab work with Bill [William F.] Brace, who's another professor.

Aspaturian: Your co-advisor.

Jones: My co-advisor. He became my co-advisor partly at this point, and Bill seemed to be quite happy with what I was doing in the lab, and Peter seemed to be not happy.

Aspaturian: What was his objection?

Jones: I can't even quite remember at this point; there were a lot of little criticisms. It was sort of his environment, and I finally went to him and said, "It's really clear to me that you aren't happy with my work, and Bill Brace is, so why don't I just switch?" He looked at me just like really shocked—"What do you mean? You're the best student I've got!" At which I started crying and thinking, "Oh, God, the last thing I want to be doing is crying at this point."

I think it was a soul-searching moment for him, too. We ended up having a long conversation about how he could offer advice in a way that's supportive. Because he'd only known criticism. But he took it to heart and tried really hard. And he's brilliant and he was a wonderful guy to work with, and he had incredible ideas. Although I didn't stick much with tectonics; I ended up really going into earthquake statistics.

Aspaturian: Yes.

Jones: And his attitude was "That's great"; it wasn't what he was doing, but he was my advisor through this transition; really supportive of it; lots of good criticism, so it worked out. He's one of those people who has joy in knowledge.

And once he got past all these psychological issues and the pressure and could become a research professor at Woods Hole —by which time his father had died—he actually inherited a chunk of money and so he could just do what he wanted. and then he became who he wanted to be. Because gathering and contributing to new knowledge really did make him happy. Even as he's battling cancer, he's asking, What new thing and interesting question can I find?

Aspaturian: Well, I hope he succeeds in his battle from what you're saying.

Jones: It's pancreatic cancer. [Peter Molnar died in June 2022.]

Aspaturian: Oh, that is too bad. With regard to his father's comment, I will just mention, Murray Hill has faded from its former glory, and MIT is still going strong, so let that be the coda to that.

Jones: Right. And the fact that he could tell me about that comment of his father's. I think he told me about that in the long discussion we had after I said I should stop working with him. I think it was part of that conversation.

Aspaturian: On that note.

Jones: We've barely gotten to Caltech, have we?

Aspaturian: That's quite all right.

Session 4, April 8, 2022

"The '70s was a big time of change for all of seismology"

Aspaturian: I wanted to start by asking you what the state of the USGS and seismology studies in general was in Southern California when you and Egill arrived here?

Jones: Ah, okay, so, the '70s was a big time of change for all of seismology. Plate tectonics developed really in the '60s. My thesis advisor, who was an assistant professor when I was at MIT—

Aspaturian: This is Peter Molnar. [*See also Session Three*]

Jones: Yes, Peter. He had really done his thesis on Benioff zones—being able to finally locate distant earthquakes because of the world-wide seismic network—which was in place because of the nuclear disarmament treaty. So there was this opening up of information, and there was this global work being done. In the 1970s, it looked like the Chinese and the Russians were predicting earthquakes, and in response the National Earthquake Hazard Reduction program was passed in 1978. [*See also Session Three*]

It was supposed to bring a big increase in money. In actual fact, it was basically taking all the existing programs and wrapping them together and *authorizing* much more funding but only appropriating what they'd already had. But it was still a time when things were growing because of the earthquake prediction claims coming out of China and the Soviet Union. By the time we get into the early 1980s, there's recognition that the optimism about earthquake prediction wasn't justified. We were starting to get the data out, and when we weren't seeing anything particular, there were questions raised about the Chinese and Russian work.

Aspaturian: As we talked about last time.

Jones: And so the '70s had been a time of change, and it was also a time of a lot of hiring at the USGS. I came in on a postdoc from the National Research Council just as things were starting to shut down, and I was actually hired on a permanent search in 1985 at the same time that Tom Brocher was hired at Menlo. Andy Michael and Rick Harris were brought on in '92 also at Menlo Park, and those were the only hires after us, compared to the several people—many people—a year that were being hired in the late '70s and early '80s. So there'd been this big growth, and there was a whole bunch of young people: All these Caltech people from the '70s were now up in Menlo Park with the USGS in the '80s—and we were still explicitly trying to predict earthquakes.

Origins & growth of SoCal USGS office in the 1970s

Aspaturian: How did the size of the Southern California office compare to the one in Northern California?

Jones: Well, there you're onto a critical issue. Menlo Park dominated. Menlo Park was the center of the USGS earthquake team—at the time they were in seven branches. In like '68, the USGS earthquake group had gone to Berkeley and said, "We've been told we have to expand our coverage in Northern California; let's work together." And Berkeley basically said, "Get lost." So the office got set up next to Stanford, and that network developed independently of the Berkeley network; and they quickly had a few hundred people. When I started with the USGS, I think there were 300 people in the Menlo Park earthquake branches, and there was a total of about ten down here. I was the fifth scientist, and then there were some technicians.

And then there was the group at Caltech. After the experience at Berkeley, when the '71 [Sylmar] earthquake occurred, the USGS, in something like '74, came to Caltech and said, "We've been ordered by Congress to expand coverage in Southern California; can we work together?" Caltech's thinking was a little more strategic than Berkeley's; they saw the example of what had happened up in Northern California said, "Sure! Come on in." [Laughter] And there was one scientist hired, and he got an office—a guy named Gary Fuis. He had gotten his PhD from Caltech and was immediately hired by the USGS, so he just stayed where he was, and he was now supervising the technicians that were being hired in Southern California to install the network. The only point of the USGS office in Southern California at that time was to supervise the technical staff that was needed down here. Gary was the only scientist on it for a few years, and then he was moved up to Northern California. He was replaced by Caryl Johnson when she received her PhD from Caltech. I think that was in '77 or '78. And the people in Menlo Park knew that they were in charge of things and got to determine how they were done.

But then Tom Heaton graduated from Caltech, I think in '78, spent a year or two in a private company and then got hired by the USGS. And then there were a couple of other scientists who came here from elsewhere. There was one guy who had had challenging enough personal interactions in Menlo Park that he came down here to never have to talk to anybody again. Which he succeeded at: He got good papers written, but he didn't really interact. And then I got hired. So by the time I came in, we're now at the fifth scientist, who were all sort of one-offs. It was sort of a gradual process of evolving from just needing a couple of scientists to supervise the technical side to starting to be a bit more of an independent office.

Aspaturian: A real research entity.

Jones: It was only in '82 that we got the separate house. Up until that time, the USGS scientists had offices on the second floor of the Seismo Lab, and the technical staff was in the basement. But things were growing, and we needed to have more space, and when Caltech bought these two houses across the street, USGS started renting one of them as part of a cooperative agreement. [The USGS office is located at 525 S. Wilson Ave., directly across the street from the Caltech campus. –*Ed.*]

Issues between Caltech & USGS complicate seismic research in 1970s

Aspaturian: What did the technical staff do, briefly, as opposed to the scientists? Were they responsible for installation and monitoring of the seismic equipment?

Jones: Yeah, installation, and they were called electronic technicians. Actually, all of the original group were vets who had learned electronics in Vietnam. So they were running the radios that transferred the data in from the field and installing seismometers and setting up the transmitters and the power systems. The Caltech network was like 30 stations when '71 [the Sylmar earthquake] hit, and by the time I got here there were 220 stations. The extra 190 or 180, or whatever it was, were put in by these USGS technicians. And so Caltech still had a technical staff that ran its stations, and then we had a technical staff that ran our USGS stations.

There was a period in there when things weren't going very well. Gary was here running the USGS network, and a guy named Jim Whitcomb was running the network for Caltech—this was after [Charles] Richter had stepped back. And I heard stories that Gary and Jim would communicate via terse notes and memos because they weren't talking to each other—that kind of thing.

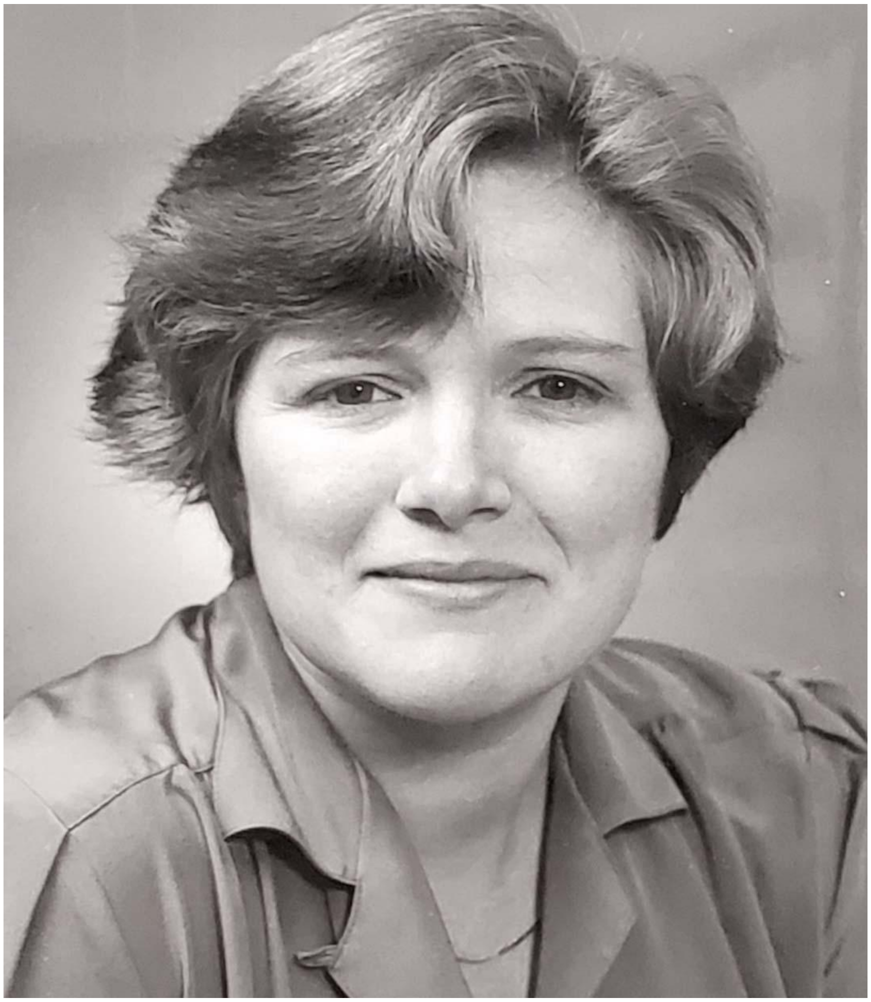
There was even a point in the mid-'70s where the datasets from the USGS and Caltech network were being maintained separately. So actually if you go back and you want to look at records of some old earthquakes, looking at earthquakes in the '70s is not easy to do because the data is still not completely organized. We made sure we could do the catalog, but the actual seismograms and phase readings are practically impossible to find. To get seismograms you end up going back to the paper records; the digital records are essentially impossible to get at this point. So a lot of those early stations installed by the USGS were recorded digitally on develocorder film, and it was essentially like microfilm. I think people mostly just give up looking up earthquakes in that time period.

Named Caltech visiting associate in 1984; recalls interactions with C. Richter

Aspaturian: I noticed that you became a visiting research associate at Caltech about a year after you came. Was that standard for USGS scientific personnel?

Jones: It's hard to say. That guy who never talked to anybody: Even though he was a well-respected scientist, he didn't become a research associate at Caltech because he never talked to anybody. There's a daily "coffee," where people gather in the Benioff Room in the Seismo Lab; and at least in that first decade, I regularly went to coffee and talked with people and got advice from them on things I was working on, and so on.

Tom [Heaton] definitely was a Caltech research associate, and so was Steve [Stephen] Hartzell, who did a lot of work worked with Tom and Hiroo [Kanamori]. So it was standard as long as you were interacting with people. There have been people who haven't gotten it if they aren't really connected. Somebody has to put me forward every two years or something to renew it.



Jones in 1987, a few years after she joined Pasadena USGS and Caltech. *Caltech photo by Robert Paz*

Aspaturian: You mentioned Richter; did you have any interaction with him ?

Jones: He had completely retired by the time I got here. I had met him a few times before that. When I was a graduate student at MIT, I came to visit at the lab a few times since my family home was nearby. The first time I went to China, they invited me to come give a seminar here on my way back, and I do remember that Richter was here for that. I think that actually I had visited a year before I went to China, and Richter was there too. But at that stage, he'd come for some of the seminars type things. So I never worked with him.

Aspaturian: And he wasn't really active then, it sounds like.

Jones: No, no. And by the time I came here on staff in '83, he wasn't showing up for seminars. I remember there was an event for him that Karen McNally, who was a postdoc in the lab, arranged— she had worked with him quite a bit. She left soon after I got here, but first there was this event, and he seemed infirm at that point. He came by and enjoyed it, but clearly he wasn't an active—

Aspaturian: He was past his better days.

Jones: And physically he didn't get around very well. I'm not quite sure when he died, but I think it was a few years after that.

Investigating Southern California foreshocks & developing predictive statistical models

Aspaturian: I see that in 1985, you authored "Foreshocks and Time-Dependent Earthquake Hazard Assessment in Southern

California"; this must have been your first paper along those lines in Southern California.

Jones: Sort of, yes. There was one that I had worked on at Lamont that was based on California data—it looked at all of California and was about foreshocks in the San Andreas system. That '85 paper is the first one I completed while I was here.

Aspaturian: Had much work been done on this prior to your becoming interested in it?

Jones: Not really; nobody had looked through the data. You sort of knew the reports—yeah, oh, yeah, Kern County had a foreshock—but nobody had really taken any systematic look at this.

Aspaturian: I have the summary for it here, but for the record, would you like to summarize what it said, and then I'll ask another question?

Jones: Okay, it's one of the simplest papers, maybe the simplest paper, I've ever written. Basically, we were trying to find discriminants for foreshocks—that is, a quake that is followed by a larger one within a short period of time in the same location. Is there something we can see in an earthquake that can tell us it would be a foreshock? So let's find some characteristic. High-stress drop, for example: Is it more likely to be a foreshock than if it didn't have a high- stress drop? You might see a foreshock, and it has a high-stress drop; it could be a coincidence. Can we say that if you find that characteristic, the quake is more likely to be a foreshock? Well, then you need to know how often anything's a foreshock before you can recognize the discriminant. I actually saw this paper as setting the baseline—a simple paper that was going to start it off—and then I would be able to find those characteristics and find higher rates. Of course, I never found something that gave us a higher rate. But the idea of just saying how often something is a foreshock turned out to be a really useful piece of information.

Aspaturian: I think you closed in on about 6 percent of the time. [*See also Session Three*]

Jones: Six percent of quakes are followed by something larger. I started my analysis at magnitude 3 because that allowed me to use earlier data without worrying about completeness. We were complete at magnitude 3—meaning, we have recorded every quake that is magnitude 3 or larger—back to 1932. And because we were looking at foreshocks, by definition, any main shock would be larger and therefore not missing from the catalog. By going down to magnitude 3, I had a much longer time window because of that big change in the network between the early '70s, and the late '70s. By then your completeness threshold goes down to one-and-a-half [mag 1.5] with all the new stations that got put in. It's this big dividing line in the data, and I wanted to be able to use the earlier stuff. But I found that 6 percent was the number, and the magnitude of the foreshock didn't seem to change that number. So everything had about a

6 percent chance of being a foreshock, no matter what the magnitude of the first earthquake was.

Then, later, when you actually look at aftershock statistics, you have some rate at which some–size aftershock follows the main shock, and the bigger the aftershock, the less often it happens. So a magnitude 6 always has magnitude 3 aftershocks. Most of them have magnitude 4 aftershocks. Some have a magnitude 5 aftershock. Basically 6 percent have a magnitude 6 or greater aftershock. Essentially that 6 percent number ends up fitting on that decay curve of where you see aftershock magnitudes.

Aspaturian: What was the reaction to this paper? Nothing like this had been done before.

Jones: I think when I first put it out, the reaction was like, "Oh, yeah, that's cool." That idea that looking forward is different than looking back, how many quakes are preceded by a foreshock is a very different question than how many are followed by a main shock. The usefulness grew with time because we started using it when earthquakes happened. The paper came out in '85; in '87, the Whittier Narrows earthquake happened, and we said publicly on the morning of that earthquake that there was a 6 percent chance of this triggering something bigger, and actually— well, you want to get into the Whittier Narrows story now?

Mapping stress data along the southern San Andreas

Aspaturian: I have a lot of notes written down here, so if you have anything you want to talk about before that— I see that you did some work on the San Andreas as well.

Jones: Right. The next big paper I did had to do with this whole thing of somebody having to get a look at the seismograms. Nobody had been to look at the digital seismograms that started being recorded in 1977 because we had this write-only memory software, but I finally did. And for my paper, I went back and looked at a couple hundred earthquakes that had happened very near the San Andreas. So it was like, What's the characteristic of the seismicity around the San Andreas? That also hadn't been done in such detail because we needed this recent data to be able to determine focal mechanisms to tell you the orientation of the stress. So what I was doing was getting a map showing the orientation of the stress along the San Andreas fault as you go down the fault.

Aspaturian: Was this largely the southern San Andreas?

Jones: Yes, I just did it with Caltech data. But at that point, automatic data was unreliable enough that I went back and read every seismogram to determine those fault mechanisms.

Aspaturian: Every single one.

Jones: Yeah, and getting 200 earthquakes—I'm not sure it was even 200—was a big deal. It took a lot of work. Now, you can do inversions with 100,000 focal mechanisms because the automatic data is quite accurate now and we have great station density, and you can deal with it in a whole different way. But this was, again, sort of the first study that had gone and really looked at this stress data along the San Andreas fault. I spent a lot of time reading these seismograms.

It was actually when I was pregnant with my first child, and I wrote the rough draft of it after my due date because he ended up being two weeks late: "I may well disappear at any moment, what else am I going to do?" and I sat there and I wrote, and I got a first draft done before I went into labor. What I was doing was trying to grapple with the data from this network that had been created as the USGS-Caltech partnership really built up in the 1970s, but it wasn't until I was in there in '84, '85, that we were really actually getting access to the seismograms.

The first paper I did was all based on a catalog 'cause I just couldn't get to the seismograms. And then I was thinking, Okay, now we know the characteristics of quakes along the San Andreas fault; let's see which ones are different when it's a foreshock. That was sort of the idea, except after that we never found anything that was different.

1987 Whittier Narrows quake: immediate responses, Seismo Lab reactions, dealings with media

ASPATURIAN: So in 1987, Whittier Narrows comes along. I remember it very vividly; I'm sure you do, too.

JONES: So at this point, Egill is still down at USC, running the network for the LA Basin. His stations actually had the best data because they're the closest to the earthquake, but nobody goes down to USC to find out what earthquakes are happening. So in the immediate aftermath, it was all up here. It happened at 7:42 in the morning—

ASPATURIAN: I remember.

JONES: On October 1st, at which point I was just about to drive into the JPL daycare center to drop my son off, and my car started shaking, and I thought the car was actually breaking down. Because we'd noticed a little oil leak in the axel— and now I can't control the car, and "Oh my God, the axel's gone." And then the radio station I had on went out, losing transmission for like fifteen seconds, and I was thinking, "Egill and I just brought this car and *everything's breaking down*." And then the radio comes back on saying "Earthquake!" and I park the car at the daycare center, and I feel an aftershock and it's like, "Oh. My. God."

So I grabbed my son and ran in there and practically threw him at his teachers because "I've got to get to work!" And then I thought, "Wait a minute," and I went by the director's office and said, "You know, there's a 6 percent chance this is a foreshock to something bigger, and half of that likelihood is in the next few hours—want to spend the day out in the school yard?" And they're like, "Good idea." So the kids spent the morning in the yard away from the buildings. I always thought that was one of the better things I did. Only a 6 percent chance, but you know, it's what mattered.

I ran into work, and at this point we had all these stations that were on computers, and we also had all the old Caltech Wood-Anderson seismographs still on—well, some of the Wood- Andersons. Of course we were out in multiple locations with local photographic recording—we were spending a lot of money on photographic paper at that point—but then we also had a bunch of stations that were just put on drums so that we could see quickly what was going on, even if we couldn't get into the computers.

Aspaturian: I actually found some remarks you made on Twitter on the anniversary of Whittier Narrows.

Jones: Oh, yeah.

Aspaturian: You said the quake damaged a lab below Seismo, causing a gas leak, and you also talk about how the quake was so close to Caltech that the room holding the paper recorders moved before they could record.

Jones: So we had data from these various stations that are coming in on phone lines, basically, and spit out and put into the computer. And then we put some of them on the drums if we couldn't get the data out of the computer, because computers were really slow back then. I had a disc drive for one of those computers recorded as worth $22,000 on my property list, and it was 450K of memory.

Aspaturian: Very impressive.

Jones: It was purchased right about the time I arrived. So the needle is controlled by motion at different sites and brought in on these radio signals, but the paper it's written on is in the lab, and as the lab moved, all of the paper moved even though it wasn't necessarily moving at the other sites because the Seismo Lab was closer to that earthquake than all but one of our stations, I think.

Aspaturian: What was the reaction in the lab?

Jones: At that time, at 7:42, who's in the lab?

Aspaturian: Well, I mean, when you got there—

Jones: Yeah. Freaking out, trying to figure out how to do all this stuff, right? And Clarence was there—

Aspaturian: This is Clarence Allen [professor of geology and geophysics, emeritus; d. 2021].

Jones: Yeah. Kate Hutton was there. And Tom Heaton. Don [Donald] Anderson [McMillan Professor of Geophysics; d. 2014] was director of the seismo lab at the time.

And we had a measuring room, which had these big tables—I think they were quite old, probably brought in by [Beno] Gutenberg—where you could lay out a long seismogram, and the tables were high so that you didn't kill your back trying to look at these things because you needed to use a magnifying glass to get in close and see in detail exactly to do the timing—get your ruler under that magnifying glass and measure it really accurately. And we were still doing that off the Wood-Andersons, so we still had those out there.

We did it off these paper records of the other stations because we couldn't get the computer data quickly enough. So the first piece of information we gave out was off of half a dozen records that we pulled off the drums and read with a ruler. Kate and I were doing that reading, and we were talking with Clarence, and everything was clipped; everything had reached the edge, so you don't see quite how big the earthquake is. And then the movement of the drums themselves made it difficult to read the amplitudes to figure out how big it was. So, you know it's funny because I remember very clearly from the year earlier with the North Palm Springs quake, literally voting on the magnitude. Clarence is saying, "This looks like about a 6; what do you think, guys, shall we go with a 6?" "Yeah, okay, it's 6." Both that one, North Palm Springs and Whittier Narrows came out at 5.9.

So I think it was just the three of us, with maybe Tom in the lab, and then they came up and said, "There's a leak.

Maybe Don came in too at this point—I can't remember.

Aspaturian: It's a long time ago.

Jones: Yeah, it's a long time ago. But Tom came in and said, "There's a leak." I actually recall it as fluorine gas, but then Egill remembers it as having been damage to one of Barclay Kamb's [Rawn Jr. Professor of Geology and Geophysics, Emeritus; d. 2014] freezers.

Aspaturian: Could be. That would hold his glacier specimens, I imagine.

Jones: And it was heavy, so it was down in the basement. They said, "It's too dangerous, we've got to evacuate!" and we're like, "Yeah, right. You're going to tell the press we aren't allowed to look at this stuff?" We came to an agreement that Kate and I would stay and finish measuring the records and getting an estimate of location and time, and everybody else would go outside and then we'd come out. They'd pull us out if they got a measurement of fluorine showing up on the first floor. Because we'd have to go in through the first floor to get out, so—whatever.

I guess we could have left through North Arms [Charles Arms Laboratory of the Geological Sciences], but that's probably where the leak actually was. We could have gone through the other entrance. I think that was the plan: Go down through the east end of South Mudd [Seeley W. Mudd Laboratory of the Geological and Planetary Sciences, in which the Seismo Lab is located]; you can get out from the second floor outside. So we had an evacuation route planned. But the leak never did get up above the first floor, and we stayed, and we finished that, and then we did some interviews outside—

Aspaturian: I was going to ask: Had the media converged by then? I mean we, over in public relations [Caltech Office of Public Relations], were all dispatched to Seismo, and we rushed over like a herd of wildebeest, as I recall.

Jones: Somebody came up with the idea, because we couldn't go back in the lab, of having the press conference in the Millikan Boardroom. So somewhere later that day—I think like at 10 or 11 in the morning—there was a news conference with Don Anderson, Paul Jennings [professor of civil engineering and applied mechanics, emeritus; Caltech provost, 1989–95; 2004–07], Clarence Allen, me, and Kate Hutton, sitting at this table at the front of the Millikan boardroom, with all of these media there reporting on it.

Whittier Narrows earthquake press conference, October 1, 1987. Seated to Jones' right is Caltech geophysicist and Seismological Lab director Don Anderson. *Caltech photo by Robert Paz*

ASPATURIAN: Was this the first time you'd done anything like this?

JONES: Yeah. The only other earthquake had been the previous year when I had been eight months pregnant, but Tom Heaton really handled that interview for North Palm Springs. So calm. When he was asked if the quake was predicted, he just replied, "Not yet." [Laughter]

Aspaturian: That's very clever. A little bit cynical.

Jones: I was like, "Oh, you're so good, Tom; I'll never handle the media that well." I didn't do any of the North Palm Springs one, but then only five days later was the Oceanside earthquake. Which was like a 5.4. It was felt all over. So people are freaking out. And we did interviews on that. And two weeks after that was the Chalfant Valley earthquake, which was a 6.5 out of Bishop. That technically was probably in UNR's—University of Nevada Reno—network—but Menlo and Pasadena also recorded it. By that time, we'd been through these two other earthquakes, and now I'm eight and a half months pregnant, and Tom had to go to a meeting at USGS headquarters in Virginia.

But things had calmed down; it seems like it's going to be okay; Tom goes off. Kate had been putting off all kinds of personal stuff, while she was dealing with all of this, and she finally had to get her dogs to the vet, so she was with that when Chalfant Valley happened. And I was sitting at home with Egill. At that point we lived on San Pasqual, over by Sierra Madre, and we were sitting at the breakfast table, and there is this really long, slow motion, and we look at each other, "Is that an earthquake?! If that's an earthquake, it's a long ways away."

And if that's a long way away, and we're feeling it, it's gonna be big. Oh God. Oh, there was stuff in the news. You can see this in the *LA Times*—a day or two later, there was an editorial cartoon, by [Paul] Conrad.

Aspaturian: Conrad was a very good cartoonist.

Jones: Yeah, and he had a picture of the state falling into pieces, and something like, "Seismologist say none of these are related!" You know, they were just laughing at us that we would keep on insisting these quakes weren't related. I remember doing the statistics on how often by random chance we would have three magnitude 5-plus earthquakes within two weeks, and it actually came out about once every 40 years. And this was the first time we had had it since recording began 50 years earlier.

I'm willing to say this is probably coincidence because the quakes were pretty far away from each other. Not that anybody would believe us. But I'm ending up having to do all of those interviews with nobody else there, at a point where my ankles were swollen; I was at the beached-whale stage of pregnancy. I couldn't go stand up at Caltech. I'd just sit in my office, and we ended up having this line of TV cameras down the hallway waiting to do an interview with me because I couldn't stand up. So that was my first experience with doing interviews. Whittier Narrows was a year later.

Aspaturian: And you're no longer pregnant.

Jones: I'm no longer pregnant. I threw the toddler at his daycare teachers and ran to work.

Foreshock probability misunderstanding results in criticism of Jones & USGS

So anyway, I think that's the main story of the Whittier Narrows tale— except, we did say, 6 percent chance that it could be a foreshock. I told it to the daycare, and we said it to the media. And—there were very few aftershocks.

Aspaturian: Yes, you wrote a paper with Egill on this, and this is one of the points you made. ["The 1987 Whittier Narrows earthquake sequence in Los Angeles, Southern California: Seismological and tectonic analysis," *Journal of Geophysical Research*, July 1989]

Jones: Yeah, it was a small aftershock sequence, but this was a magnitude 6, and it was going to have aftershocks. We kept on saying, You need to expect aftershocks. So, "Magnitude 3.7— are we done with the aftershocks now?" "No, you're not done with the aftershocks now." We kept on trying to say there'd be more aftershocks. The quake was on Thursday, and on Saturday, Governor [George] Deukmejian [California governor, 1983–1991] came down and toured Whittier to see the damage with the head of the governor's office of emergency services [OES]. Dick Andrews the OES deputy director was the guy who showed up, and he's taking the governor around these sites, and there's all these photo ops going on in Whittier.

The next day, Sunday, around 4 in the morning, there's a magnitude 5.4 aftershock that causes more damage. The chimney of the USGS office in Pasadena had to be taken down after that, actually. But it also brought down a brick wall in Whittier in front of which the governor had been photographed the day before. OES was criticized for having allowed the governor to be at risk. And their response was "The scientists assured us there was less than a 6 percent chance of this happening, so we were right." *But of course, that's not what we said. We'd said there was a 6 percent chance of something even bigger*. This didn't count, despite the fact that there was damage. In fact the chance of a 5.4 aftershock is probably something like 30 percent.

So I'm getting criticized for this, and I realize we can't prove him wrong.

ASPATURIAN: You're getting criticized because they misunderstood what you were saying.

JONES: Because they misunderstood what we said, and we didn't have anything about the aftershock to give out, and so they had misrepresented— which is when I then decided to work on trying to say what aftershock probabilities were.

Transitions to aftershocks research & publishes "Earthquake Hazard after a Mainshock in California"

And for other reasons there was a guy up in Menlo Park who got interested in the same problem, and we ended up doing the work together. So there's a 1989 paper called [Paul] Reasenberg and Jones, which is still the basis of all aftershock probabilities issued by the USGS.

Aspaturian: I have it here: "Earthquake Hazard after a Mainshock in California." It appeared in *Science* [April 1989].

Jones: Right. We got it in *Science*. The fact that they're still using it now, 33 years later tells me somebody—

Aspaturian: Found it valuable.

Jones: Well that, but that we also still need to get our act together.

Aspaturian: The two of you also did an article a bit earlier on foreshocks from what I see here: "The Realtime Use of Foreshocks for Earthquake Prediction in Southern California."

Jones: It's a proceeding of a workshop.

Aspaturian: Ah! I could not find a copy of it because it was a proceeding.

Jones: Yeah. This was us exploring how these two things—prediction and foreshocks—fit together. It was because of Dick Andrews using us to cover himself. I won't say it was to cover *his* mistake because letting Deukmejian pose for photos in Whittier was being attacked by unreasonable people who were trying to politically undermine him. But then he used what I had said in a way that wasn't true, and so we had to really start working on that.

ASPATURIAN: What view did this give you of the media? You had become a spokesperson and you were also—

JONES: I was never a spokesperson.

ASPATURIAN: Well, you were.

JONES: By default? The thing is we all did those interviews. I was no more of a spokesperson than Tom Heaton. Jim [James] Mori did more interviews than I did. People remembered me.

ASPATURIAN: Because you were female, I suppose.

JONES: I think a big chuck of it was being female and therefore a bit more comforting in a time of stress. I do think—I like to say—you feel better when mommy tells you it's okay. So I got remembered and the guys forgotten. But I was never— Kate [Hutton] had it as her job to do media.

ASPATURIAN: That's right, I remember. So Whittier Narrows did kind of move your work in a new direction?

Recollections of 1988 Raymond Fault earthquake

JONES: Well, Whittier Narrows moved me into doing the aftershocks and really trying to look at that.

ASPATURIAN: Did you find that a more rewarding field than the foreshocks?

JONES: I mean it's really the same thing, right?

ASPATURIAN: Well—

JONES: No, statistically it is. And that I think is one of the important things I was coming to realize. There was a title I used for a talk—I never published it, "Are foreshocks main shocks that happen to have big aftershocks?" I presented its abstract at some conference somewhere. So that part I took on. But also there were all these earthquakes happening in LA.

ASPATURIAN: Well, there was the one in Pasadena on the Raymond fault, which I remember.

JONES: So December 3rd, 1988, we had a 5 on the Raymond fault. That's the strongest shaking I've ever personally felt. We were living in West Pasadena at that point, basically right on top of the fault. The earthquake was 17 kilometers down, but the motion coming into our house was essentially vertical and threw me out of bed. Ran into work and responded to that one. That was the first one where our son, who by this point is two, had seen me on TV.

Sven was at home with his father, who was still at USC at that point. Egill had turned on the TV to see what was happening, and there was some live interview. Sven's like, "Mommy's in the

TV, Mommy's in the TV!" And I get home: "Mommy, you were in the TV!"

ASPATURIAN: Just like some cartoon hero, I suppose.

JONES: At that point, he'd never watched any. We hadn't had him watch any TV. We'd have news on. Maybe a few months after that, we finally got a VCR player and a recording of *Lady and the Tramp*, which he watched, as they do at that age, many times. He was, "Mommy, I want my dog news."

ASPATURIAN: What?

JONES: The dog news, since when we turned on the TV, we said we were watching the news. It was his dog news.

ASPATURIAN: That's cute.

JONES: So he didn't know. At that point, it was still, I think, pretty shocking to him. The funny part of that was just two days after the earthquake, we had the annual AGU meeting. The American Geophysical Union meets every December, and it was always up in San Francisco up until a few years ago, so back then, we would always go up to San Francisco for this. It was actually where Egill and I met for the first time, and we got engaged there the next year. [*Session Three*] So we're up there, and Sven was with us, and Egill had his big paper on the earthquakes of the Los Angeles basin—a big summary. He was interviewed and on TV—which Sven saw while sitting in Egill's lap.

He had trouble figuring out whether Papa was with him in the hotel room or in the TV.

Discovery of buried faults beneath LA proves controversial

So both of us were sort of doing tectonics using the earthquake locations, which, if you get a lot of them really accurately, you can start illuminating the active geologic structures. It's using earthquakes to add the third dimension to geology and figure out what's moving now.

Aspaturian: Had this been done before?

Jones: Oh, sure, seismotectonics was a field, but—

Aspaturian: But not so much in Southern California.

Jones: Not with any detail because we didn't have good earthquake data. The 30 stations that Caltech had were not enough to do an accurate analysis of these small local earthquakes. And then, you know, we got the stations in, but we didn't get the data out for almost a decade. So this was sort of the first wave of papers.

Aspaturian: To build up a 3-D picture, as you put it.

Jones: And we had done the Whittier Narrows paper together the year before, so we had that. And that opened up the whole idea of the buried faults under LA. We now talk about them all the time, but Whittier Narrows was the first one that we saw.

Aspaturian: Yes, this was unprecedented—I remember.

Jones: People weren't sure whether to believe us. It was an article of faith that any earthquake big enough to be really damaging had to show up on the surface of geology, and the idea that you could have a really big earthquake that was really significant and not have it in the geology was new.

The very, very beginning of that idea was a 7.0 earthquake in Algeria in '79, I think. There was a guy from Cambridge named Geoff [Geoffrey] King, who enjoyed being flamboyant, who went and looked at this earthquake and concluded there was no surface structure. He just got told he missed it. He said, No; it was really like that, and what happened was the fold grew. But we didn't have great GPS then, so how do you prove that the fold grew, especially if there was nobody there to see it beforehand? He was trying to argue that it was on a buried thrust and basically nobody bought it from him. It was like this controversial idea, and he was so flamboyant, he was pushing it even though —Ha!—right? That was sort of the feeling about it and then Coalinga happened in '83. That was a 6.5.

Aspaturian: A similar buried fold?

Jones: In the middle of California, and it was a buried thrust. We were sort of just getting the papers out on that and having to go, "Look at this: It's real; you really could be missing the potential for an earthquake this size." And then it happens again. Whittier Narrows was sort of the first of the buried thrust faults to really be significant. I mean Coalinga caused quite a bit of damage, to be honest, but this was now in LA.

Aspaturian: And this was a third such event.

Jones: Right. At this point.

Aspaturian: Was—is—seismology inherently a rather conservative science? Do new ideas have difficulty gaining traction, or was it— ?

Jones: I don't think that it's any more conservative than any science. The whole scientific process is fundamentally that somebody makes a discovery, and they publish it. And the job of everybody else is to figure out what they did wrong. It's not being too conservative. That's what science is. Because the easiest person to fool is yourself.

Aspaturian: This is true. Cold fusion.

Jones: Right! So you have peer review so that others can tear you apart and figure out whether you're actually right. I think it is a fundamental issue. That is the research process. It has to happen that way. At the same time, I think the evolution of the scientific method in Western, male-dominated society helped encourage a nasty aspect of it. You don't have to be a nasty reviewer, but you haven't until recently been punished for being a nasty reviewer. And it's often anonymous. You can be as nasty as you want. All those fights that go on. There's a whole downside to it that isn't necessary. But that is the fundamental principle—that you have to examine somebody else's stuff and your assumption is they're wrong, and you have to prove that you're right.

So when Geoff first did it, it was a little bit sort of a hand-waving thing. The thinking was, that fault could easily have been offshore—it was close enough to the coastline. Until Coalinga, the locations just weren't good enough. Coalinga was also in an active oil field, so that led to discussions about whether or not that might have caused something that wouldn't otherwise happen. And then Whittier Narrows happened— here in the city, and we had really good locations, and we could constrain it, and we could show that the shallowest it came to the surface was twelve kilometers. So doing that was part of the progression that's a natural part of all science. "Here's a crazy idea; oh, wait a minute; here's another aspect that really does support this. And then—here's the solid proof."

Aspaturian: Right, here's something that seems more incontrovertible.

Jones: You can't get rid of it. It still left open the question of how big such a quake could be. And then, of course, Northridge happened.

Aspaturian: Which we'll get to probably next time.

Jones: 6.7, another buried thrust. LA was central to this evolving understanding of earthquakes, of seeing that these buried faults really are a significant risk, and you can't trust everything to the geologists.

Initial analysis of Bay Area Loma Prieta quake triggers USGS turf dispute

Aspaturian: Loma Prieta occurred a couple of years after Whittier; were you involved in that at all?

Jones: Not directly. So here's another aspect of the seismic networks in California. I said that up in Northern California you had Berkeley and Menlo Park, and they were kept separate, and then they gradually merged more. In Southern California, you had Caltech, and then the USGS in Pasadena came in. The northern and southern networks are separate. They each had their own area. We used to talk about the Gutenberg–Byerly line because Beno Gutenberg [professor of geophysics; d. 1960] ran the lab down here, and Peter Byerly ran the lab in Berkeley. And sometimes we call it the Gutenberg–Byerly discontinuity. [Laughter]

Aspaturian: That's funny.

Jones: Because also, maybe you're using different techniques; you don't have exactly the same software, and so on. And so taking things across the boundary, you had to accept that maybe you didn't have all the data or that it hadn't been located the same way. Or say you saw some fault that had an offset coming down from north to south. You'd be back to this "What can you prove wrong? That's probably the network screwing up or not communicating effectively versus there actually being an offset in the fault." And a big part of it just was that there wasn't a unified California network; there were two networks that did separate things. Like there's a network in Utah; there's a network in Nevada; and a core value in seismology is that you don't talk about somebody else's earthquakes. The regional networks are the ones who understand their own earthquakes, and so you should let them talk to the press. That was Rule No. 1. Rule Number 2: You don't share your kooks. So when you get the crazy people calling the network, you don't shunt them off to the other guys unless you're really wanting to piss somebody off. We respected those boundaries.

Now, when Loma Prieta happened, I think it took out power in Menlo Park. It was 6.9 and really damaged things. So at the beginning, none of the press could get hold of the Northern California network people. So we did give out some information from down here, but we said, "This is really approximate because all our stations are south of the event; they're not at all close by. So we're not going to see the data that effectively." So we said some stuff about a magnitude of about 7 and a location in the Santa Cruz Mountains. And we're leaving it to them to do the rest. But *we're in the lab* and we're trying to look at what we got, and we tried to do a focal mechanism, right?

Aspaturian: A focal—

Jones: The focal mechanism uses the directions of motion recorded at all the stations to calculate the orientation of the fault producing the earthquake. So, we're asking, Is this quake on the San Andreas? We used all of our data, but with all of our data only to the south of the event, the result was not very reliable. We got a focal mechanism that showed a big component of thrust—a reverse motion, and not just the sideways motion you'd expect on a San Andreas. And internally we were saying, "Do you think this data is real: Was this not the San Andreas, or is the San Andreas moving in thrust fault?" "Yeah, right." But it was all internal, trying to figure things out, and we only had this one-sided data. Were we seeing it correctly? We assumed we probably weren't seeing it correctly—except, we had a lot of data, and it seemed pretty good.

LA Times report on Loma Prieta lands Lucy in middle of USGS dispute

I don't know how this happened, but somehow the focal mechanism that I had created got into the hands of an *LA Times* reporter, a guy named Ken [Kenneth] Reich.

Aspaturian: That name is not familiar to me.

Jones: Oh, oh. He was the *Times*' primary earthquake reporter for a long time. He was originally a political reporter. And that's relevant because he saw things in political terms. In politics, the story's in the conflict. In science, the story's in the consensus. Conflict's what we do every day, yes, but when we actually agree, then we've got something. I've never found out how he got hold of the data, but he knew that I had created it. And he knew enough to say that if it was a thrust fault, then it's not the San Andreas.

So he went to Menlo Park and in a press conference after the earthquake, he asked the head of the earthquake group, at that time a guy named Al [Allan] Lindh, "Lucy Jones says that the earthquake is not on the San Andreas. What do you say about that?" To which Al said, because he's Al, "If Lucy says that, she's full of shit." There was also sort of this violation of the agreement that you don't talk about each other's earthquakes. And I had never done that: As I said, I never knew how my focal mechanism got out—it wasn't meant to. So Al and I talked, and we realized what was going on, and that, you know, we hadn't actually violated the "only talk about your own earthquakes" agreement.

But after Ken reported that in the *LA Times*, I called his editor, and basically said, "I did not say that to him. I never spoke to him." And that's where I found out that he had somehow gotten hold of this focal mechanism. And I said, "I didn't give it to him. It shouldn't have been given to him. We don't report on their earthquakes."

Aspaturian: You never found out how that information got to him?

Jones: No. But down here, we were handing it around to each other. And this was before we had the media center, so the media would come straight into the measuring room to do interviews. So he could have picked it up off somebody's desk. He could have overheard something, or maybe somebody had put a note on it, you know.

Aspaturian: Easy enough.

Jones: Right. So I was furious at the *Times*. I told them: "I am not talking about their earthquakes. Do not report me as commenting on their earthquakes. I did not make a public statement about this quake; I'm not going to be out there saying things about their earthquakes. That would just get me in trouble." It was also at that point that Menlo Park still really ran everything for the Pasadena USGS office. I told Ken Reich and his editor, *You can't quote me. Do not do it*. They tried to keep the controversy going, and I was—let's just say, not happy. They finally agreed to not quote me again.

Aspaturian: Do you think if you'd been a male scientist, they would have done this?

Jones: I don't know.

Aspaturian: An obvious question, I think.

Jones: I don't know how you could tell. Well, then, the funny thing was, of course, it turns out *it was a thrust fault*.

Aspaturian: You were right.

Jones: The focal mechanism was correct. And basically it means that the San Andreas has a thrust faulting component, not that it's not the San Andreas. But then a day or two later when the *LA Times* is reporting that in fact it was a thrust fault, and I wouldn't let them attribute anything to me, they were then getting attacked by readers for not crediting me: "Lucy was right in the first place in bringing all of this stuff up," and I was there saying, "No, do not bring me back into this."

Omori's law regarding time decay in aftershock sequences explained

That's my nonscientific action on that. Well, except, that was our first big earthquake after Paul [Reasenberg] and I had published our aftershock paper. So I was working with Paul to monitor the aftershocks.

Aspaturian: The paper and the quake must have happened fairly close to one another. Since the paper came out in—

Jones: March of '89, and the earthquake was in October of that year.

Aspaturian: Within six months.

Jones: But you know, the paper always comes out six months after you wrote it. So we had finished the work at least a year before, I think. And here's our first big earthquake since we finished the work, and we were able to show within a day that the aftershock sequence was particularly small. So, the big thing about this was that there's always this time decay, and sometimes it's a little faster, sometimes a little slower; and there's always a magnitude distribution—sometimes higher, sometimes lower. Whittier Narrows had a particularly what's called low b-value, which means a relatively larger number of big aftershocks and relatively smaller number of small aftershocks. The reverse from Loma Prieta, which had a high b-value combined with a relatively low overall productivity.

The time decay is sometimes called P-value or Omori's law. And then there's the overall productivity given those relatively small variations. The biggest variation is the total number of aftershocks. Northridge was about two or three times more than the average, and Loma Prieta was two or three times less than the average. So the first time we had this to use, we were actually able to say, "Chances are there won't be many aftershocks." And so I did the work for that professionally with Paul. But then on the media, it was only the lovely Ken Reich, trying to get me in trouble.

Aspaturian: How much of this work did you actually do in the field, or was it all—

Jones: Almost none. Back in Afghanistan [*Session Two*] it was all in the field, but by this point—

Aspaturian: And you were attracted to the field initially by the thought of clambering around on rocks as a profession.

Jones: Actually in '84 when I was still on the postdoc here, and Egill was working out of USC, he got a grant to go and look at Mammoth because he's a volcanic seismologist out of Iceland. So he went up there and put out a temporary deployment of seismographs and recorded earthquakes in Mammoth for two weeks. I took vacation and went up too because we were getting paid to stay in Mammoth. And I helped him put out the stations, but that was not my work.

Anomalous quake cluster highlights challenges in measuring & defining magnitudes

Aspaturian: When the 1990 quake occurred in Upland, did it begin to occur to you that you were in the middle of a serendipitous sequence of quakes? There'd been so little major activity in Southern California and then—

Jones: So that was one of the biggest ones, but there was also Pasadena—5.0 in December of '88, and in November there had been a 4.8 or so in Palos Verdes. In January of '89, there was a 5 in Malibu, and then in June of '89 there were two 4.5s in Montebello.

Aspaturian: So this was unusual, just this cluster.

Jones: Absolutely. Upland was even more so. I'm trying to remember. Egill and I and Hiroo [Kanamori] wrote a paper for an AGU conference proceedings about all of the earthquakes in LA and how we were seeing this cluster ["Anomalous earthquake activity in the San Gabriel Valley, southern California, 1987-1991," 1991].

Aspaturian: The 1988 and 1990 Upland earthquakes, was it that one? I have your publications list here ["The 1988 and 1990 Upland Earthquakes: Left-Lateral Faulting Adjacent to the Central Transverse Ranges," May 1991].

Jones: No. That paper was just about the Upland earthquakes. After the 1991 Sierra Madre earthquake, we did the AGU presentation about the cluster, but that was never turned into a fully published paper. It was one of those things that we tended to pass around at conferences and talk about without completely getting it out. Just plotting, doing this b-value. Okay. When you normally plot out number of earthquakes in intervals by magnitude, you see something approaching a straight line.

Aspaturian: These are unrelated earthquakes, I take it.

Jones: Well, no, any group of earthquakes. This is the interesting thing about it. Any group of earthquakes, if you plot logarithm of number versus magnitude, you get something pretty close to a straight line. And with a slope of b. And you'll see papers about whether b is always one. It's something close to one, which means that approximately for every magnitude 6, you'll have 10 5s 100 4s, 1,000 threes, 10,000 twos, 100,000 ones. Presumably at some point it no longer continues to fall off, but that's generally below what we can record. We now have good enough locations in enough different places to see that somewhere between zero and one, it starts to roll off. If you go up to a high enough magnitude, then it starts becoming a question of, Do you have enough data to really see it?

If you take the whole world, it goes up through the 9s. Presumably there's a physical limit to the size of earthquakes. Your size is determined by the area of the fault that moves, and, you know, if you split the earth in half, that's about a magnitude 12. So we do have a limit.

But if you look just along the San Andreas fault, you'll see a b-value among the small earthquakes getting down to a really low level, and then you have the 7s and 8s, and so it doesn't appear to be linear if you are confined to just a linear distribution. The b-value is a volumetric characteristic quantity: It's not necessarily representative of events on a plane.

And what that means physically is a hot debate still. So the statisticians can go and say, "Here are the statistical characteristics," and the geologists can say, "Here are the physical characteristics of the fault," and the question is, How do the two line up? We're still arguing over it and still arguing over assumptions. And then how linear is the b-value distribution really?

And there are plenty of sets of data that you can get where it doesn't look very linear, but you probably aren't using the same kind of magnitude through the whole thing. Because different ways of measuring magnitude have different limitations in terms of the characteristics of the data, and if you're calculating a magnitude the way Richter did it, you can't see above about 6 or 6.5 because it uses high-frequency energy, and as you start putting earthquakes on bigger faults, the quake is generating more long-period energy that doesn't get into the short periods.

So all of the magnitude scales saturate at some point, and on many of them you can't see the really small earthquakes because you just don't have enough stations to get the data on all of them. So most catalogs are compiled by putting different types of magnitude in together and trying to correlate them. That's been much of the discussion over the last 40 years: Which magnitude do you hold onto? And Caltech has changed its definition of magnitude. "Local magnitude" was the definition below magnitude 6 for a long time, and now we do a moment magnitude and it's supposed to be calibrated to be the same as local magnitude, but there is clearly a systematic offset in Southern California, and we just moved it to be consistent with the rest of the world.

Aspaturian: Sort of like a renormalization?

Jones: Yeah, and we still don't know why we have the offset between moment magnitude and the original local magnitude. You know when you have the automatic system putting out a magnitude and then there's a correction?

Aspaturian: Yes.

Jones: The first one is the traditional ML [local magnitude], and the second, better one is moment magnitude, and we were always revising downward. Right now, we just subtract .3 from what we give out. So there's a pretty big systematic difference. We're saying what was used for a long time is wrong.

I'm not quite sure where we are in cleaning up. This would be a real problem for statisticians trying to do this right across this time period. And nobody's really tried to grapple with that. If I ever went back to really focusing on these statistics, that would be what I'd look at because it frustrates me that I know we've changed the meaning, and we haven't really looked at what that means as you look at the probabilities. The Reasenberg and Jones analysis is done with the old magnitudes, not the ones we're using now.

But anyway, those problems mean that whenever you get a smallish data set, you tend to not see as straight a line, or you might have that problem making offsets. Loma Prieta rather famously had a pretty big kink that really was probably a break between a Richter magnitude [written as ML] versus what's called a Coda magnitude or a duration magnitude, and that boundary was showing up. In LA in that time period, we had a pretty straight line and then this huge bulge of extra 4.5s and 5s. Once Northridge happened [1994], it all went away. So was that bulge a precursor to Northridge, or is that a coincidence? We never did come up with a reasonable explanation for why we had that bulge.

So that was something we were talking about. When Upland happened in 1990, we could see not only those three 5s, but there were all these 4s—4.5, 4.8, 4.9—all around the LA Basin, and concentrated along the northern side of the LA Basin. When Egill and I had first moved to California, we did not buy earthquake insurance on the basis that we lived far enough away from the San Andreas fault that we didn't think that was going to be a big issue.

Aspaturian: But it turned out, of course—

Jones: And then here we're having all of these earthquakes around here, and we start looking at this, and then we got our house retrofitted, which is the better thing to do, but we also ended up buying earthquake insurance. I think at that point it was $300 a year.

Aspaturian: It's changed.

Jones: Yeah, I know. But just a few months later was the Sierra Madre earthquake, which broke all those chimneys —there was a lot of damage in Pasadena—and we never did convince our insurance agent that we didn't know that earthquake was coming. [Laughter] Even though we didn't have any damage from it.

Parkfield earthquake prediction experiment: history, implementation, methodologies

Aspaturian: I see that 1991—the same year as the Sierra Madre quake—you also wrote your first hazard assessment paper. Was that a coincidence, the timing? You were one of many authors on that.

Jones: But I actually wrote it. [Laughter] So what happened with that is that Kerry Sieh [professor of geology, 1989–2009] and I were named chairs of a committee. Do you remember Parkfield?

Aspaturian: Oh, yes.

Jones: This comes back to all the discussions in the 1970s and '80s of how do you predict earthquakes? You know, while the whole prediction thing is falling apart in China, we're holding monthly data meetings to try to predict earthquakes for California for years. Every month—

Aspaturian: "We" meaning the USGS?

Jones: Yes. Every month, one of us would fly from Pasadena up to Menlo Park, and we would go to a data review meeting where we were actively watching what was going on, and we'd make up all these plots, and this is where I started to see these patterns and this bulge in the magnitude 4s, and I'm trying to keep track of all this.

And they wanted to have a prediction of the next Parkfield earthquake. So they came up with this system.

Aspaturian: Do want to briefly give the history that led to that? It's interesting, as I remember it.

Jones: Okay. Parkfield is a place on the San Andreas fault just south of the creeping section, which has little earthquakes all the time. The Parkfield area had had a series of magnitude 6s on average once every 22 years. And one of them back in 1857 turned out to be a foreshock to the big 1857 Fort Tejon earthquake that ruptured down toward LA. Then there was one in 1888 and in 1901and 1922. So there seemed to be a pattern, but then there's 1934—it's only twelve years after the last one. But then the next one is 1966, so it started to fit the pattern again. If you'd just let '34 be '44, you'd have this perfect line. Talk about data fudge.

So that was used as the basis for a paper out of Menlo Park sort of making the argument that the next earthquake would happen in 1988. This got presented to the state. I wasn't really involved in all of the politics because this happened just as I was starting down here. But an agreement was made for the USGS to run an earthquake prediction experiment for a town of 34 people.

Aspaturian: Parkfield was heavily instrumented as I recall.

Jones: Lots of money went into it and lots of instruments, because we were going to catch an earthquake, right?

Aspaturian: Yes, yes.

Jones: Because that was other thing about these prediction discussions: Every time we didn't see something before an earthquake, we would ask ourselves "Well, we didn't have nearby stations—is that the reason? What if we were able to be right on top of the quake?"

So you've got this Catch-22 situation where you've got to be able to predict the earthquake to get the instruments you need so you can learn how to predict the earthquake. But Parkfield seemed to be the answer to our prayers, right? We could get in there and instrument the hell out of it, and the state backed us doing this, but they insisted on being public with it— essentially experimenting with 34 people how to do a public earthquake prediction. I had been doing this foreshock work, and we came up with a methodology to say What's the chance that a Parkfield earthquake is going to trigger something bigger, or what's the chance that a 4.5 at Parkfield is going to trigger a 6. And I did this with Al Lindh, the guy who said I was full of shit after Loma Prieta.

Aspaturian: Obviously, you'd reconciled by that point?

Jones: No, this was actually before that. [Laughter] So there was this open-file report; it wasn't in a published journal, but it was a heavily internally reviewed document saying, "We've come up with this methodology: Here's what we're going to say if these different earthquakes happen." And they came up with an A, B, C, D system. An A-level alert is, the highest probability; we really think it's going to happen now. I forget now what the definition was for those A alerts with Parkfield; I think actually it was a 37 percent chance of an earthquake—it was something that Al and I had come up with.

Extending prediction models to southern San Andreas fault & creating an alert system

But then, we started thinking, Why are we doing this for a town of 34 people when we have this San Andreas fault that hasn't had a big earthquake in 300 years, and it's here with 10 million people? What are we going to say if there's an earthquake near there? Or if something happens near the Salton Sea? What should we be saying?

I'm trying to remember what the trigger was that got the state to say, "We want an answer to that." It was something to do with the state geologist and the governor's office of emergency services.

So we had done these aftershock probabilities, and those had come out and the next step was how do we now deal with foreshocks for a really dangerous earthquake. And the decision was made that we should come up with a system like Parkfield for the southern San Andreas fault. Actually, I think this was a recommendation from the California Earthquake Prediction Evaluation Council [CEPEC], which was a group that had been formed in '75 and chaired by the state geologist who provided advice to the governor. And Kerry Sieh and I were named its co-chairs—Kerry from Caltech, me from the USGS. There were ten people on the committee. One of them was Duncan Agnew—

Aspaturian: Yes, he was your coauthor on prediction probabilities in '91 ["Prediction Probabilities from Foreshocks," *Journal of Geophysical Research,* July 1991]

Jones: That Agnew and Jones paper is the outcome of what happened through that working group.

Aspaturian: I see.

Jones: So he was on it from Scripps, and he had been a Caltech graduate. And there was a guy named Mark Ghilarducci, who worked for the governor's office of emergency services and is now its director. He was sort of Dick Andrews' eyes and ears to keep us in control. There were some other people from other places. I should look it up and see who all was on it.

Aspaturian: Take a look. ["Short-Term Earthquake Hazard Assessment for the San Andreas Fault in Southern California"]

Jones: Yeah, there it is: Jones, Kerry Sieh, Duncan Agnew, Clarence Allen, Roger Bilham—

Aspaturian: All your co-authors on this.

Jones: Mark Ghilarducci, Brad [Bradford] Hager, who was here at Caltech; Egill; Ken [Kenneth] Hudnut; Dave [David] Jackson at UCLA; Art [Arthur] Sylvester at UC Santa Barbara. So it's sort of taking all the local academic departments involved in earthquake science—they all had a representative in it. Bilham's an expert on creep meters, and Art did other types of triangulation measurements. By this point, Egill had been hired at Caltech, and he'd just taken the position of running the seismic network. This was like one of the first things he was involved in.

So for that report, we did an A, B, C, D–level model. We said that D means we think there's at least a .1 percent chance of a San Andreas earthquake in the next three days. C was a 1 to 5 percent chance; B was a 5 to 25 percent chance; and A was greater than a 25 percent chance. Then we had to figure out what would make us say what; and Duncan and I ended up working together to create a methodology. The math was Duncan's—I guess we came up with the model together, and he worked out the integrals. And the model was that theoretically you could divide all earthquakes near the San Andreas into foreshocks or not foreshocks. They may not have any physical differences and it may only be distinguished by what follows them, but you could classify them into those two groups.

And then we can use the network to figure out the rate of background seismicity along the fault: At what rate do you expect to see different earthquakes within these characteristics? We can then go look at big earthquakes and see how often they're preceded by earthquakes and what their characteristics are. That gives us a rate of foreshocks. So about half of them are preceded by a foreshock within three units of magnitude. So we actually just said our assumption was that every magnitude bin had a 15 percent chance of being followed by the big San Andreas earthquake. That would be the distribution.

So your chance of being a foreshock basically was independent of magnitude, but your chance of being a background earthquake was very dependent on magnitude, because the small ones are much more common. So that you then got your chance that you were in the foreshock category as a function of magnitude: The bigger the quake, the more likely it is to be the foreshock.

We then had to assume spatial distributions, and we'd never seen a foreshock more than ten kilometers away, so we just did a box of ten kilometers—it's also much more likely if you're really close by.

One of the things we're trying to do now is work out how do you do a more accurate spatial distribution that reflects the decay of risk with distance from the fault. So we had magnitude, space and time, and then worked out the math for this to come up with a probability that an earthquake of any size within ten kilometers of the San Andreas would be a foreshock of a San Andreas earthquake. We then created a table that gave the alert level for different size earthquakes on different parts of the fault. So, for example, if you had a magnitude 6 within ten kilometers of the San Andreas fault near Joshua Tree, you'll have a 15 percent chance of triggering the San Andreas.

1992 Joshua Tree earthquake: "I'm getting under my desk now"

And so this was done up as an open file report and submitted to NEPEC and approved by them. It was waiting for the signature of the director of the USGS when a magnitude 6 happened near Joshua Tree, eight kilometers from the San Andreas. This was in 1992. And so there we were with a—

Aspaturian: A model.

Jones: An agreed upon passed-through-our-review-process-but-not-yet-implemented system for giving the alerts. Well, there was an initial quake that was actually magnitude 4.6, which put it in the level C category, just barely. And so I went in to the Seismo Lab—I did all the things that we said in this report that we would do, notifying all of these various people, and then sat down in my office talking to Kerry Sieh, my cochair in putting this together; and we're talking it through, and by this time it's ten o'clock at night, and I'm getting ready to go back home when my office started shaking.

"Kerry, there's an earthquake."

He was over in mid-Wilshire: "I don't feel anything yet."

"Ah, yeah, it's getting stronger."

"Oh." He started to feel it.

I was like, "I'm getting under my desk now; talk to you later."

Well, it stopped at a 6, and it wasn't actually on the San Andreas. But now we had a 6 near the San Andreas. And the probability of that triggering something got into the level B at about 15 percent. It's the closest we've ever got to having a prediction. So we get back with Dick Andrews, who's like really sick of us by this point, but "Here it is. Here's what we were talking about. We have this procedure; we agreed upon it."

He notifies the governor, and he notified the city of Los Angeles, and their emergency manager called me up and just swore at me: "How can you possibly be doing this?! Why are you saying anything when it's only 15 percent! This is just ridiculous!" I told her, "This is as good as it's going to get. This is the best we know how to do. You wanted a short-term earthquake response plan; what did you think you were going to get?" But nothing more happened.

Aspaturian: And nothing happened at Parkfield either.

Parkfield finally has its moment: "Was this earthquake a roller or shaker?"

Jones: The Parkfield earthquake finally happened in 2004. So twenty years late. It actually happened during the National Earthquake Conference, which was being held that year in Memphis, I think. A lot of us from the USGS were out at this meeting. The conference includes state emergency managers and FEMA people; it's actually run by FEMA. So it's often about response and related issues. And we're sitting at the big awards luncheon in the banquet room on the second day, and I noticed that Mary Lou Zoback, who was the head of the Earthquake Science Center [USGS, Menlo Park] at that point, suddenly got up and left the room. She comes back in and whispers to the state geologist, and they get up and they walk out of the room. And then the state seismologist, a guy named Mike Reichle comes over and whispers to me, "The Parkfield earthquake just happened."

And I'm like, "Oh, yeah, right. You're joking." At this point, it had become a joke.

Aspaturian: This is *Waiting for Godot*.

Jones: Yeah, it was *Waiting for Godot*. I'm like, "You serious?" So I get up and I walk out. More people are like, "What's going on?" And the thing is, part of the Parkfield earthquake experiment was an evaluation of the probability that it would trigger another big earthquake coming into LA, which was estimated at about 10 percent.

Aspaturian: Which it did not.

Jones: Which it did not. But I had to go and try to explain all this to the brand-new director of the OES and the brand-new associate director, who had just come in from New York.

Aspaturian: Was Jerry Brown back in office at that point? [Reference is to California governor Edmund G. Pat "Jerry" Brown Jr., who served as governor of California, 1975–1983 and 2011–2019].

Jones: No, it was Gray Davis [governor of California, 1999–2003; succeeded by Arnold Schwarzenegger, 2003–2011]

Aspaturian: Gray Davis, before Arnold.

Jones: No wait a minute, maybe it was Arnold. 2004. At that point, I was on the state seismic safety commission, and I was appointed by Gray Davis but reappointed by—

Aspaturian: Arnold?

Jones: Oh yeah. The associate director was this New Yorker who was a donor to the governor. Now that I think of it, he stayed around, so it must have been Arnold. We were trying to explain all of this Parkfield stuff to him, and his only question was, 'I just want to know, was this earthquake a roller or a shaker?' This is on a phone call, and I've got like the state seismologist, the state geologist, and the head of the earthquake program all sitting here with me in this hotel room—

Aspaturian: It's an earthquake, not an E-ticket ride.

Jones: And as I try to answer that, they all looked so embarrassed and were just like mouthing "I'm sorry." Yeah.

Aspaturian: Let's stop there.

Session 5, April 24, 2022

"The baby, the mother & everything": Joshua Tree briefing turns into a media sensation

Media appearance with baby Niels the evening of the Joshua Tree quake. *New York Times & NBC News photos*

Aspaturian: When we left off last time, you'd just dived under your desk during the Joshua Tree quake, I think, and that is the one where you subsequently appeared holding your baby.

Jones: Yes. With a lot of stories that a lot of people have, many of them untrue. [Laughter]

Aspaturian: Was that that same evening or the next morning?

Jones: It was that evening. So the magnitude 6.1 was at 9:50 in the evening. Egill was home with the boys because I had come into the office with the 4.6. He felt it at home. Actually our older son, Sven, often had trouble going to sleep. So he was five and Niels was one. Egill was sitting in their room rubbing Sven's back to help him go to sleep and actually said, "Stop jumping around, you're never going to get to sleep this way," when he realized it was an earthquake. He grabbed both boys up and came into work.

This was 1992, and our computers were old. We used to have computer rooms, and they were dangerous places, with lots of cords hanging around connecting all of the computers. Sven did fine. We joked about how he figured out that by standing next to the candy machine and looking wistful, he got all the cameramen to— he had pocketfuls of candy by the time we got home.

Aspaturian: Clever boy.

Jones: Clever boy. Niels was nineteen months old. He had been sound asleep when the earthquake happened, and he didn't like being woken up, and he screamed if put him down. He was quiet and looked around at things if we carried him.

Of course, the earthquake was close enough to the San Andreas that it was setting off all the alarms about the fault, and I had just written this report with Kerry, which we talked about it last time. So we had all these agreements in place of how we would respond to this, and by that methodology, this had a 15 percent chance of being followed by a San Andreas earthquake within the next three days. And most likely immediately. So I was answering questions from the media when Egill, who headed the seismic network, had to deal with a computer crisis. And you don't take a baby into a computer room, so he literally handed me Niels in the middle of an interview. It was like, "Okay, you do what you gotta do, and you just keep on going."

Niels was quiet; he sort of leaned his head on my shoulder and would look around at things as long as I was holding him. So I ended up doing these interviews—doing this public discussion about the risk on the San Andreas fault—all of this just carrying him. I've heard lots of stories about it. There's a really common one that I shushed the reporters because the baby was sleeping. The whole point was— the baby wasn't sleeping! There was one moment when a pretty aggressive reporter stuck a microphone really close to me. I can't remember the question, but Niels looked at this thing as it comes up right beside him, and he took his little finger and pushed it away, saying "No-o-o." I don't know if that one ever made it onto film that somebody kept, but I think the reporter was with CNN. But it created this image, this whole thing of the baby and the mother and everything. But at the same time, scientifically—in terms of earthquake prediction and earthquake probability—it was sort of the most interesting place we'd ever been.

Aspaturian: In the wake of these statistical analyses you'd done.

Jones: Right, exactly. So it was an interesting night. We were there until about 2 a.m.

Aspaturian: Were you holding the baby the entire time once you—

Jones: Well, he was old enough that he was more sort of on my hip with his head up on my shoulder. I think I traded off with Egill at some point. I would have been exhausted trying to carry him the entire time myself because kids get big, you know?

Aspaturian: Yes, they do; I recall.

"Trying to explain hyperbolic decay in real time is not an easy communication activity"

Jones: So, the earthquake: The governor's office of emergency services—Dick Andrews had been deputy director and I think he was director at that point— was based up in Sacramento, but he also had a house out in San Bernardino County, where he'd spend a lot of time. So he was down in Southern California when the earthquake happened. He came over to the lab, and it was a negotiation, sort of, about what do we say publicly.

And part of creating the warning approach—this report that Kerry and I'd done with the committee and with the governor's office represented on it—was the agreement that it's the scientists' job to figure out what the risk is, and it's the governor's OES job to help inform the emergency managers on how to use this information. So we would give our information to OES, and they would distribute it.

And of course we were developing a relationship with them, and if they came over to the Seismo Lab we could just sit down and talk with them directly, and they then go and make the public statements. I can't remember at this point—this is interesting—whether we said the risk was level B, which means 5 to 25 percent, or did we flat out say 15 percent. We might have used the level B language, because of course by the time we were making a statement two hours later, a chunk of the risk is gone. It's at least down to 10 percent by then. So I think we used level B, which gives more wiggle room on what's actually true at any one moment. Trying to describe hyperbolic decay in real time is not one of your easier communication activities. The problem of course was we really hadn't socialized the levels yet.

Aspaturian: They weren't ready for prime time.

Jones: They were ready in the sense that we'd done all the work, but we hadn't yet shared with the public what a level means.

Aspaturian: That's what I mean by prime time.

Jones: Yeah. They were ready, but they hadn't made the transition—we hadn't done the education about what they would mean. But of course nothing more happened on the San Andreas fault.

"'Earthquake lady' image probably hurt me a bit scientifically"

Aspaturian: Your appearance with Niels—

Jones: Yes. As in Niels Bohr. [Laughter]

Aspaturian: Why not? It seems to have been kind of an iconic moment. It was all over the media. I remember coming into work the next day, and someone saying, *Did you see Lucy?* Everyone was talking about it. I was thinking about this: You were a working scientist; you'd done some highly respected, highly regarded work. You were beginning to become kind of a face for the public, and now you appear as a mother with a baby. With all these roles kind of converging, did you have any thoughts about it at the time?

Jones: Sure.

Aspaturian: How did this affect you as a public figure and as a scientist?

Jones: You've got to remember that seismologists all get caught into doing public stuff. We're a science that ends up having to do this.

Aspaturian: That has a public face.

Jones: That has a public face. We're also all supposed to ignore it. It is no indication of whether or not you're a good scientist, right? I do think that for me, because I became so popular because of that issue, it probably hurt me a bit scientifically.

Aspaturian: You mean in the aftermath.

Jones: In the aftermath because it's easy to then dismiss me as that figure.

Aspaturian: Ah-ha, interesting.

Jones: Think about the term "." The male seismologists were seismologists. The female seismologists were the "earthquake ladies." It was a way of saying we were something less than experts. At the time, I don't think I was quite as aware of the gender issues whereas now, sort of looking back with time, it's pretty clear to me that that— well, if we can jump ahead, many years later, I was in an advisory role to Metro because they were trying to figure out how to put in a line through Beverly Hills.

Aspaturian: This is the LA Metro.

Jones: The LA Metro was digging a subway line across the Santa Monica–Hollywood fault going through Beverly Hills and, to avoid the faults, they ended up putting the tunnel under Beverly Hills High School, and Beverly Hills was fighting it, dirty. So Mike [Michael] Antonovich, who was the supervisor for this area for the LA County Board of Supervisors, was at this Metro meeting where I testified about this, and he actually said, "Asking Lucy Jones about earthquake safety is like asking the weather girl about how to build your house to withstand an earthquake."

Aspaturian: Sounds like Mike Antonovich.

Jones: Right. There were a bunch of editorials attacking him for having said this.

Aspaturian: What a fool.

Jones: Well, yeah, it didn't succeed. He actually came out the worse for it. It was more sort of that experience, reflecting back and realizing that the "earthquake lady" thing is definitely a diminutive, right? The other thing is that after Joshua Tree, I got a huge number of letters from women saying, "What an inspiration you are!" It's funny—my son's thesis advisor down at UC Irvine is only a year older than I am and was having the same situation with young children at the time, and this appearance really made an impact on her. But at the same time, I also got a letter saying what a disgusting human being I was for using my infant son to get fifteen minutes of Andy Warholian fame. I was like, "Right, you have a babysitter on call at night, huh?" It wasn't like we did it by choice.

ASPATURIAN: There are always trolls like this out there, no matter what the precipitating incident is.

JONES: Right. Now, with Twitter they have a bigger venue than sending a letter. And then there were some articles in the newspaper, too. I remember the *LA Times* called me a seismo-mom. So there was an awareness that I did something important for women struggling with that balance issue at the same time that I was, but also, as I said, over the long run I think it probably hurt me a bit in terms of my scientific reputation. Because if you're doing that, you must not be a serious scientist.

"I think that what happened is I started filling a cultural role"

ASPATURIAN: Thinking about all this in preparation for this interview session, it struck me you were a very unusual figure in the history of science, even for the late 20th century, to be a female scientist, a public figure, publicly a mother. Had anything like this ever transpired before?

JONES: I don't know.

ASPATURIAN: I don't think so.

Jones: Not that I'm aware of. I think that's why it was such a moment. After that I started getting all these requests for me to give talks about it. I remember giving a talk for the National Charity League and saying, "I'm here because I couldn't manage my babysitters." [Laughter] "It's not like I have some special knowledge about how to do all of this together. It's 'cause I failed at it, right?" Though I think there's also that feeling that you've somehow failed if you had to bring your kid to work. That was definitely part of it.

Aspaturian: In that era, I think that was still the case. How about other female scientists. How did they react to this?

Jones: Number one, there weren't a lot.

Aspaturian: No, there weren't, but the numbers were starting to grow a bit, early 1990s.

Jones: I didn't have many directly with me. Kate and I were just friends. So here's a sign of how we weren't understood. We would get confused. And anybody who knew me and Kate—we really don't look anything alike. She's a body builder. I'm not.

Aspaturian: But you are two women who appear on television talking about earthquakes.

Jones: And she did an interview about a year before Joshua Tree, when Niels was an infant: "We understand you just had a baby," and she says, "Ah, no, I think you're confusing me with Lucy Jones." And the interviewer proceeded to argue with her!

"No, no, I'm sure it was you." And she's like, "I really do think I'd know."

So that sort of thing was going on. And, you know it's funny: The men were doing as many interviews as we were. Kate actually had it in her job description to do the interviews because most Caltech professors didn't want to. But for the USGS, all of us were expected to do it as part of our job. You're science and you're public service. At the time, it was really me, Jim Mori and Tom Heaton. And Jim probably did the most, actually. And he said that still, 90 percent of the time when he said he was a seismologist with the USGS, it was like, "Do you know Lucy Jones?" Even though he did a lot more interviews than I did. And so right there is this weird piece of it.

Now I think part of it is that this one event with the baby created me as a center and a focus. And maybe this is part of the same thing, because, afterward, as I thought back on it, it's like: Why are people hunting out seismologists after an earthquake anyway? Knowing which fault it's on doesn't help you rebuild your house. But what it does is that somebody understands this.

Aspaturian: Yes, it puts the event into some manageable perspective.

Jones: Right. It puts it back in the box. I give it a name, I give it a number, I give it a fault. It says somebody understands it, and that makes it less scary. Don't you feel better when mommy tells you it's okay than when daddy does? As a mother, holding the baby, giving you the information to feel less scared—I think that's where it all came together in a particularly connected way, and it wasn't just the science.

Aspaturian: You tapped into a kind of archetype with that appearance, it seems like.

Jones: Yes! Later, I remember, a friend who studied these sorts of things talked a lot about the Madonna archetype. That is—a sense of comfort. And I think that what happened is I started filling a cultural role.

And this comes back to my National Charity League talk. Part of it really was, "I'm only here because I didn't manage my babysitter. It doesn't mean I know how to do this." The other part, though, and I think it's still true, was about how we have accepted women working without accepting the consequences of women working. Pretending that the children don't exist, so you fit into the male model isn't working for society. And it's still a really big issue. The pandemic might have helped us make a bit more of a transition on this.

A scientific partnership: complementary & collaborative work with husband, E. Hauksson

When I think about how I managed it all, there were a couple of things. Well, there's one important thing, and that's my husband. That I had an Icelandic husband who didn't see it as "this is the woman's problem." They have in Iceland a really extraordinary level of gender equality. I mean it's not all solved, but they really have more gender equality, and they have a really exquisite social goal of gender equality, and they try to have policies designed around it. And it comes out of a culture that just never saw—didn't have the double standard. I think I understood it when we were engaged, and Egill's brother got married. They baptized their baby at the wedding, a very common thing in Iceland. Even back then, 40 years ago, something like 60 percent of first-born babies were born outside of wedlock. But it was also seeing my mother-in-law beaming as she held her grandson to be baptized and just realizing, There really isn't a double standard. Part of it's about sexual activity, but it's also just about where the problems are. So I had somebody who really completely did it with me.

Aspaturian: How common were husband and wife scientist couples at that time?

Jones: It was starting to happen. In fact, my thesis advisor, Peter Molnar, was the partner—they never legally married—of Tanya Atwater, who was sort of the first woman to be a big name in plate tectonics. And so when I started graduate school, that was what I was seeing.

Aspaturian: You had a role model.

Jones: I had a role model. Although it was still rare at the time, Mary Lou and Mark Zoback were graduate students at Stanford and got hired by the USGS just before me. So I wasn't the first, but you can see how easily I can name the others because they were still so few. And actually Mary Lou was really important because she worked half-time after the kids were born for fifteen years and still made it into the National Academy of Sciences. So there was a standard that it could be done.

Aspaturian: Were you or Mary Lou Zoback ever faced with the conception that despite all your research achievements, at some level it was really your husband's work?

Jones: No, that was not a problem.

Aspaturian: Well, that's good to know. That that did not become part of it.

Jones: That did not become part of it. At all. Of course with me and Egill, he is a quite reserved—

Aspaturian: It would also be Icelandic culture, I suppose.

Jones: Right, right. [Laughter] It's an introverted country. He has no desire to be out there doing the public stuff, and so I'm much more visible than he is. Early on, we wrote a lot of papers together. I had come in with earthquake statistics, foreshocks, and a certain amount of seismotectonics, using the earthquakes to do the third dimension of structural geology; and then Egill was doing some geochemistry earthquake prediction work with Wally Broecker on radon. That was part of his thesis. He was like, "I really don't want to keep on doing this, I've proven that there's nothing there."

Aspaturian: There's no connection.

Jones: "No connection. I want to get back to earthquakes." And then with his postdoc he got into running Columbia University's seismic network in Alaska, and seismotectonics became pretty much his main focus. He ended up doing seismotectonics plus tomography—how to use the travel-time data to invert for what the velocity structures are. So instead of just determining where the earthquakes are, he was actually looking at what can be learned about the rocks' characteristics from the speed at which the seismic waves travel through them. It's sort of like getting a geological CAT-scan. So we each had our own area. Mine was statistics, and his was the tomographic work, and then we had this overlap with seismotectonics. So we did some papers together and some not.

Aspaturian: Your fields complemented each other.

Jones: Yes. But this also a time when we were having a lot of earthquakes—the late '80s and early '90s—and writing papers on each earthquake. I remember we'd get a paper off to the journal, and then there'd be another big earthquake the next week. It was like, "Give us a break, really guys?!" [Laughter] So sometimes there was this real concern: What if you didn't get the paper out before the next earthquake happened? So that led to this whole suite of stuff we did together.

Of course with the Zobacks, it was like "Zoback and Zoback," who's the first author? But "Hauksson and Jones"—it was easier. I think that helped it not seem like we were the same person. I've been surprised by how many people I might have known for a long time, or that my husband knew for a long time, who didn't realize we were married.

Aspaturian: In a way that might have worked to your advantage?

Jones: I think it was to our advantage. But, no, that was not something I ever felt. Other issues—yes.

New seismotectonic data & insights gleaned from 1980s–90s SoCal quake cluster

Aspaturian: Speaking of articles, you got a series of very interesting papers out of the Joshua Tree–Landers–Big Bear cluster [April–June 1992]. Would you like to talk about that research in layperson's terms?

Jones: Well, okay. There were several papers that were sort of analyzing the seismotectonics. The series of quakes actually raised more questions than it answered. It made it clear that the San Andreas wasn't all of it—that there's this big series of events going up toward the east of the Sierra Nevada.

Aspaturian: None of those quakes occurred on the San Andreas fault, am I correct?

Jones: None of them are on the San Andreas. And actually the Ridgecrest earthquake that happened in July 2019 is sort of the same series. It's a trend going up to the east of the San Andreas. And this gets at a problem about talking about earthquakes with the public. You've got to emphasize the time scale. What we are looking at with the seismotectonic analysis is activity that's been going on over the last million years. And you've got to think about that very differently than you do about what's going on right now and the risk for you in your lifetime. They're just completely different questions.

So when we look at the seismotectonic picture and are seeing these earthquakes going up to the east of the Sierras, we realize it's feeding into the long-term picture of pulling a bit of the North American plate off of the San Andreas fault. So there's the opening of the basin range between the Sierra Nevada and Wasatch fault, that's stretching out— it's actually like stretch marks. If you look at the basin and range, it's a way to thin it out and make it wider.

Aspaturian: It's not just earthquakes, it's part of a much larger geological picture.

Jones: Well, the earthquakes are presumably the process by which this stretching happens, at the surface. Down deep, there's other plastic processes that go on. So one of the plate tectonics issues is sort of balancing out displacements. So you can see the movement of the Pacific Ocean plate compared to North America. Where is that happening? About two-thirds of it shows up on the San Andreas fault. So we can go in and use geological structures like a stream bed or a lava flow that are offset by the fault and see how much it's moving. And then we can use big-picture modeling of plate tectonic motions to see how the plates are moving with respect to each other.

And what we've found is that the plates are moving—there've been refinements over the years—at 55, 60 millimeters annually, something like that. On the San Andreas, it's 33 millimeters per year. So you've got a little less than half of the motion, maybe a third of that motion which you've got to put somewhere else. And one piece of that pretty clearly is that part of North America's getting dragged up—you're opening up the basin and range.

But now you've got this sort of geometry problem. If that's happening up in the basin and range, and it's not happening by the time you get down to Mexico, how do you transition it? And that's the Landers earthquake and the whole Joshua Tree–Landers–Ridgecrest sequence, showing us a way that some of that motion's getting transferred from the San Andreas, the plate boundary down in the Gulf of Mexico, up into this stretching into the basin and range. And Landers was the first one that really helped us see that.

Aspaturian: So it was a very useful probe for beginning to understand some of this.

Jones: Oh, yeah, it really did change the way we were looking at the plate boundary motion. Every big earthquake changes the way we look at the details, right? You look back to the 1906 San Francisco quake, where for the first time geologists recognized, “Oh, there’s a fault here!” And even the concept that the earthquakes are definitely related to the fault was something that developed out of that event. The Japanese didn’t completely accept that earthquakes were on faults until the 1950s because since their faults were all offshore, they didn’t see them.

Aspaturian: I see, I see. Much harder to visualize.

Jones: Right. But what you saw on the seismograms from an earthquake also raised questions. You can look at the first motions in an earthquake at many different locations, and it’s what is called the double-couple motion. When you look at how the ground moves, you see four quadrants. You’ll see the ground moving toward you in one direction; the next quadrant is away from you; the next quadrant is toward you; the next quadrant is away from you. The double–couple motion made it seem like, Why is that related to a fault? It was argued back then that if the earthquake represented motion on one fault, we should have single-couple motion because— it is only one fault and it moves in one direction.

Even after San Francisco in 1906, when the movement on the San Andreas fault was clearly connected to the earthquake, early seismologists weren’t sure—whether the fault caused the shaking, or the shaking caused the fault was a matter of debate.

Well, it turns out that movement on the fault always gives you a double-couple mechanism, but that wasn't completely resolved mathematically until, I think, the 1950s.

And of course, as with theoretical advances in any science, that knowledge then allowed us to better use the earthquakes to do our geology modeling, etc. But the Landers sequence was the beginning of really recognizing what is called the Eastern California shear zone. We'd seen the faults, but really, the earthquakes put it together. So 1906 gave us, oh, strike slip faults, and then we had 1933—

Aspaturian: Long Beach.

Jones: Yes, Long Beach, and then there was the realization that the San Andreas isn't one fault, it's a system of faults. We've got the Hayward and the northern Calaveras in Northern California, and San Jacinto, Elsinore, Newport–Inglewood—all parallel to the San Andreas—down here. And then we had the '71 Sylmar earthquake, and it was like—oh! those thrust faults. You gotta move the faults around that offset, and there you've got the transverse ranges, the San Gabriel Mountains.

Aspaturian: Nature is always a few steps ahead of you.

Jones: It's like, Shouldn't we have seen it? Well, all right, now that we have [laughter], we'll focus on it. Then Whittier Narrows happens, and you realize, "oh, blind thrust," and then Northridge, and "Oh, *blind thrusts that are really big*, and then with Landers, it's"Oh, there are branches to the San Andreas going east, not just going west."

So each one of those events shifts our point of view. And Landers definitely helped move us out to the Eastern California shear zone, really looking toward the mountains out there. But it was only a couple of years later that Northridge happened, and we all sort of jumped back into LA. Hopefully we're finding balance and gradually learning how all of it fits together.

Realization that 1992 Landers quake "could really set the San Andreas off"

I think there's another thing about Landers. There was that seismotectonic picture I've just described, but the other aspect is that it was near the San Andreas. Because, remember, epicenter is just where the earthquake begins: It doesn't happen over an epicenter; it happens over a fault surface. Every part of the fault surface moves, and it's just as important as all of these displacements. And when you looked at Landers, yes, it went up towards the north toward Owens Valley, but it also came south, reactivated the aftershocks down from Joshua Tree and extended down to the San Andreas. So we were seeing aftershocks to Landers that were within a kilometer of the San Andreas. Joshua Tree had been eight kilometers away, and we had debated whether that was far enough away to not be as big a risk. The ones that are really close matter even more. Well, there they were. They were right there, practically on the San

Andreas. And three hours after Landers, we had the Big Bear aftershock.

Aspaturian: I remember.

Jones: And that's on a different fault, one that's almost perpendicular to the Landers fault. So this is hard to do with just dictation. Shall we put a figure in with it? Because I think it's rather important. The San Andreas basically runs north-west, south-east, but with the transverse ranges it takes a bend, trending 60 degrees west of north.

Aspaturian: I'm going to note for the record that you're sketching something out here.

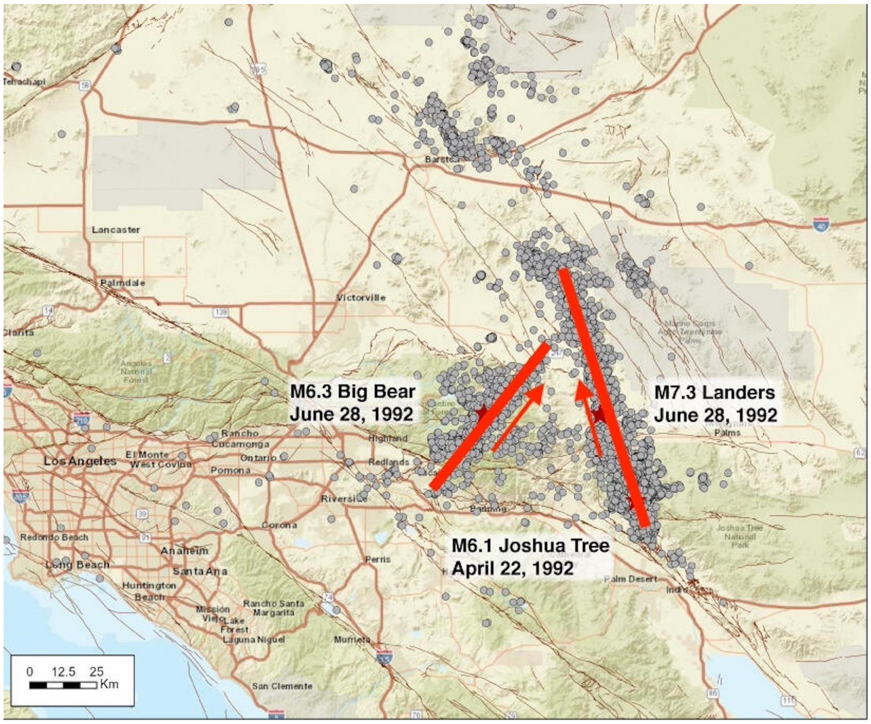


Photo courtesy of Lucy Jones

Jones: Okay. This is 60 degrees west of north—probably 65 actually. And this is only 40 degrees west of north. This is the San Andreas and then Joshua Tree–Landers is coming up like this. And then Big Bear came in like this, so it's perpendicular to the Landers zone. It also came down and touched the San Andreas. So you have this triangle of material between the Big Bear fault, the Landers fault, and the San Andreas, so this is a left-lateral motion for Big Bear and a right-lateral motion for Landers.

So what you have is this material in the triangle on the Landers and Big Bear faults being pulled away from the San Andreas, and this has very clearly reduced the normal stress in the area between them and on the San Andreas fault. Normal stress is what keeps the fault from slipping. That's where you build up the stress. Releasing that, we realized, "Oh my God, this could really set the San Andreas off.

Landers quake brings home challenges of communicating statistical risk to the public

So the Landers earthquake happened on Sunday morning, at 4:50, something like that, and Big Bear was at about 8:20. And again, the director of the governor's office of emergency services came over to the Seismo Lab, and we made a statement. Actually I'm pretty sure he made it, but I was standing beside him. Maybe Tom [Heaton] was too because he would have been the scientist in charge at the time. So, the statement was, "There's an increased risk of earthquakes on the San Andreas; stay off the freeways today."

We didn't give an actual number for the probability that we would have a San Andreas earthquake because since the epicenter wasn't within eight kilometers, we basically didn't have a method for quantifying it. But it was clear that this was a worse risk than it had been with Joshua Tree, and we said that publicly. So, the aftershocks continued, and the day after Big Bear, the California Earthquake Prediction Evaluation Council met in the evening with OES, which had an office in Pasadena at that point. Both Egill and I were there—I think my Aunt Dorothy came over to stay with the boys—and the meeting lasted 'til like 1 a.m., and we were saying "This is like the most dangerous situation we've ever had. Is the San Andreas going to go, and what do we do; what do we say?"

And we decided that because it had been almost two days since the earthquake at that point, and nothing more had happened, the risk was down. So we weren't going to say something then about that. Well, what about the fact we clearly had an elevated risk for the San Andreas because we had reduced the normal stress? What if we saw something else? What did we say? Did it still fit what with what we had said in the report about an earthquake near the fault?

Aspaturian: You wanted to alert the public, but you didn't want to cause a panic, either.

Jones: Right, and it's like "Let's be useful, too."

Aspaturian: Yes.

Jones: It's not like we would alert the public because we think the earthquake's coming. We alert them because there's an increased probability, even though the absolute probability is still low. So how do you say that? What do you say; when do you say it; what's the usefulness of saying it? To the degree that people think we're hiding something, we've really undermined ourselves.

So, all of this is playing out. But here we also had this methodology. We had this level A that we said we didn't know how to get to. The level A in the 1990 San Andreas fault was defined as a probability greater than 25% but we said we had no methodology to ever get that high a probability. At that meeting, we ended up agreeing that if we were to see a magnitude 6 within three kilometers of certain sections of the San Andreas fault, or a 6.5 in some other parts of the San Andreas that tended to have more background earthquakes like around Banning, we would call a Level A alert. And I was told at that meeting that they were going to have the governor record a message that could be released with this warning.

ASPATURIAN: This would have been Deukmejian at this point, I believe.

JONES: Yeah, it was Deukmejian. I never heard it. I assume it got done, and the USGS—which was me, Tom and Jim at this meeting—agreed that we would notify OES within twenty minutes if there was an earthquake that called for a Level A alert. Which means that we basically promised to immediately locate the earthquake *and* be sure it was really that close. And that the state would then call out the National Guard and have this recorded message from the governor. We called it the "go-to-war scenario." We came back, and Jim and Tom and I and talked to Doug Given, who was the lead programmer for the network at the USGS, about how to set up computer alerts to be sure we would know if we had hit the go-to-war scenario. And this system stayed in place until '97 or '98, when we said, "Wait a minute; don't you think we should officially call this off?"

Aspaturian: Five years later.

Jones: Yes.

Modeling earthquake probabilities after Landers & Big Bear quakes

The questions raised by all of this were really interesting. With this methodology we'd done, you could theoretically separate background earthquakes from foreshocks and at least determine the statistics of each separately and therefore be able to come up with a probability out of it. But now we were having aftershocks. So because the earthquake had happened, we had an increased risk of the San Andreas earthquake, which probably died off with time because that's what aftershock risks do. But we then had aftershocks, which were also dying off with time. So what was the probability that an earthquake was going to be a foreshock when you were having an aftershock sequence going on? And so I worked out those mathematics.

Aspaturian: You did the modeling for that.

Jones: I did the modeling, and that was one of those papers that got submitted just before Northridge happened. It was like, at least I got it in the journal before the next earthquake happened.

Aspaturian: Would you have been sole author on this?

Jones: Yes.

Aspaturian: "Foreshocks, Aftershocks and Earthquake Probabilities: Accounting for the Landers Earthquake." I read some of this, the title caught my eye. It looked like it was important.

Jones: That came out of this discussion after Landers–Big Bear, when I realized we needed to work out the math.

Aspaturian: That paper must have gotten quite a bit of attention.

Jones: Not really. I don't think the media figured it out—

Aspaturian: I mean within the scientific community.

Jones: But then we had Northridge. By the time it was actually published, we were dealing with Northridge. It was there and people noticed it, but—

Aspaturian: You were scooped by an earthquake.

Jones: Oh, yeah. Happens all the time for us.

Aspaturian: I'm sure you've gotten this question a lot, but with this earthquake cluster, did you have a sense of when it might terminate or what it might portend? Say, just in a qualitative sense even without recourse to your modeling. What were the discussions like?

Jones: Oh, we were scared. No question. That was the time when Kerry Sieh had recently published some of his dates off of the southernmost San Andreas.

Aspaturian: Oh, yes. His paleoseismology research.

Jones: They showed that on average, the quakes there have happened 150 years apart, and it's been 300 since the last one. So, now we're having this earthquake sequence that we know has reduced the stress on that part of the fault.

Aspaturian: Which is a dangerous sign.

Jones: Right So we were really concerned that the southern San Andreas earthquake was going to be happening. That it's now 30 years since then, and it still hasn't occurred surprises me.

Interpreting anomalies in earthquake data & analyzing the magnitude frequency curve

Aspaturian: We should talk about that in more detail later on. So Northridge comes along in early '94.

Jones: Well, there's the other thing. Starting in early 1985, there were several 3s and then the rate of 3s in the LA basin went up.

It's not something you really notice unless you're tracking it, which I was doing. Back then we used to have data-review meetings every month to look at what the earthquakes had been doing and whether we should be concerned about it. Then we had Whittier Narrows in '87; in November of '88 we had a 4.9 down near Palos Verdes. In December of '88, we had the magnitude 5.0 Pasadena earthquake. In January of '89, we had a 5.2 Malibu earthquake. Then, June of '89, there were the Montebello earthquakes, two 4.5s about 15 minutes apart. February of '90, we had the Upland earthquake. I forget what the magnitude finally came out to; it was about 5.5, 5.4. And then '91, we had the Sierra Madre earthquake, 5.8.

Usually, if you looked at a magnitude frequency curve of the earthquakes—plotting the logarithm of the number of earthquakes versus magnitude—you usually get a pretty straight line. But if you did the LA basin from '85 to '92, '93, you got a straight line on the smaller earthquakes, and then you had this big bulge of extra 4s and 5s. They did not fit the expected curve.

Aspaturian: That's what you were saying in our last session, yes. Were you looking at that, thinking, What does this mean?

Jones: Oh, yeah. We never published it per se, though we might have alluded to it vaguely in the Upland earthquake paper—I'd have to look back at what we put in with the papers on the individual earthquakes. But we definitely were looking at it and saying, "What does this mean?" The whole thing about there being a straight line has some theoretical basis, but also a lot of it is just empirical. So, what does it mean when it's not a straight line? The usual thought is that you're screwing up how you did your magnitudes, because that's the usual way you don't get a straight line. And yet we could be quite sure in this instance that we weren't. [*See also Session Four*] We talked a lot about what it meant.

ASPATURIAN: When you say "we"?

JONES: I can remember making graphs and sharing them at meetings. I'm trying to remember if we wrote about it in one of those papers or not. There might have been something in the Sierra Madre paper. And definitely my husband and I talked about it. And in fact after Upland, we made a decision personally in early '91 to buy earthquake insurance.

ASPATURIAN: You talked about this also. [*Session Four*]

JONES: So, yeah, it was a time that got me to make some personal changes. And then Northridge happened, and of course it's so big, and there was enough damage from it, that everybody was too busy dealing with it to spend too much time thinking about what's next. Whereas Landers, even though it was really big, and the scientists were really busy, didn't do enough damage because it was too far out in the desert.

Aspaturian: It was too isolated.

Jones: So people had the emotional bandwidth to be much more scared about it. After we'd had this meeting about how do we deal with the San Andreas, there got to be a rumor going around that Caltech knew a big earthquake was coming and wasn't telling anyone to avoid a panic. But trying to explain what we did know involved another timeframe: It's like once you're three days out from the earthquake, there's no particularly increased risk *right then*. I mean it's no longer the spike that you get on the first day, but there is a long-term tail, and so there's still an elevated risk but it's small. That isn't nearly as interesting as all the rumors that were going around.

Coming to grips with randomness & uncertainty in earthquake prediction

Of course this was back before Twitter and email, so you got letters instead of things like social media posts. I got one letter from a woman saying, "I know you can't tell me when the next earthquake is going to be, but will you tell me when your children go to visit out-of-town relatives?" And I've used that letter over the years because I think it's symbolic. It was like, If you're scared enough, you'd rather believe we're lying than that we really don't know. Because the not-knowing is what makes things so scary.

Aspaturian: Hard to live with uncertainty, and some people find it much harder than other people. *The secret knowledge is somewhere.*

Jones: Right, right. The idea that the secret knowledge is somewhere is a way to make things seem a little more in control.

Aspaturian: Yes. Well, we see this in the [Covid-19] pandemic: The secret knowledge of how it really started or of a cure is out there somewhere.

Jones: Actually, this may be the next book I'm going to write if I get my act together: This idea of how we deal with uncertainty and our rejection of randomness, and I see it in all sorts of things related to earthquakes, and it's all playing out again in the pandemic and in climate change. A lot of the same psychological impact plays out through disasters and randomness, and I'm trying to figure out how to organize this in my mind and turn it into a coherent idea.

Aspaturian: I think our modern world finds this harder to deal with as well because so much of what afflicted mankind in the past has been conquered by medical advances—antibiotics, immunizations, all of this. We don't live with that kind of uncertainty anymore.

Jones: Right. There was a Watson lecture last week on uncertainty. "The Rhetoric of Change in the Age of Pandemic"

About Fortuna and chance and sort of a literary take on this. And looking at how Western thought has been based on the Aristotelian idea that you can find a cause. And that's the fundamental basis of science, right? We're looking for the causes; we believe in that; and developing Poissonian distributions and analysis of randomness is a way to tease out the cause —

Aspaturian: Start quantifying, yes.

Jones: Quantifying the randomness. But it is very difficult to let go of the need to find the cause and acknowledge that it really is random. That's part of the evolution of my thinking about earthquakes: I go into seismology to predict them, and I come out of it saying there's a fundamental randomness to when they happen that will never be predicted, and how do we live with that? And I really do think that's true. And so this talk was really interesting to see; I was thinking I might try to hunt her out—the professor who gave the Watson lecture [Jennifer Jahner, professor of English]—and see if she's willing to have some discussions about this.

Aspaturian: She's over in the humanities. I'm sure she'd be happy to talk to you.

Jones: I think there's something really important in there about the evolution of thought about randomness and uncertainty. And of course it's not restricted to Western thought. Aristotle got us much more focused on cause: I think there's a reason why the scientific method got going here first.

"All cultures have an approach to assigning cause to random disasters"

But all cultures have an approach to assigning cause to random disasters because we just can't bear to think how disasters are completely random, because that means you are in danger at all times, and that's just a horrible feeling. So in Eastern thought, it's around yin-yang balance. This is actually something I did for my senior thesis in college.

By that time I knew I was going to MIT in seismology. And I was taking this Methods of Sinology class, which was basically on how to do research in Chinese, and Brown had an incredible collection of old classical Chinese texts. [*See also Session Two*] So I looked at earthquakes in Chinese history and Chinese thought in the *Luxuriant Dew of the Spring and Autumn Annals,* which date back to the second century BC. It's a commentary on a Confucian classic—although not by Confucius—that talks about how to be a good ruler and makes the point that natural disasters are caused by an imbalance of yin-yang.

So there's this whole philosophical way of how they approached it. It's definitely there through Chinese thought, definitely there through Japanese thought: Prime ministers who've taken responsibilities for disasters and committed seppuku or something. But it's the same thing.

Aspaturian: It's the East Asian analog to Aristotle's insistence on cause and effect.

Jones: What also happened in the West with the Jewish tradition is you have earthquakes when God's angry at you.

Aspaturian: Yes. The idea that there's an ethical component to all natural disasters.

Jones: Right. Because we have an individual relationship with God, and individual sin is the way in which we focus on it. So earthquakes happen when you're bad. Psalm 18 tells us how the Lord was angry, and the mountain shook. In the East, the cause was that we are out of balance, and therefore things are disrupted. So it's a sort of communal ideas versus the individual idea, but they both have this in common: *I* did something wrong, or *We* did something wrong.

Aspaturian: They're subjectively centered in some sense.

Jones: They're a cause. Right? So you can make yourself safe by not repeating those mistakes. It's a pattern that lets us feel like we can be safer by choosing not to do those things. It's very human but very, very—

Aspaturian: Flawed.

Jones: It's a very deeply instinctual approach that that when we're faced with danger, we try to be safe. That's how you live. The problem is we don't have an internal fact-checker. If the threat is something like a predator coming at you, you're going to run up a tree. You respond to the danger by figuring out the pattern that since he can't climb, you've made yourself safe. When it's these impersonal natural phenomena and there isn't actually a causal relationship, we create one anyway. Did being a moral person make you safer? It probably did in the long run.

Aspaturian: In some contexts.

Jones: Right. There are plenty of contexts on the individual. When you're a better person, you are probably less likely to be murdered by a bad person. So that that drive to be moral might well have peripherally led to increased survival. Anyway.

Aspaturian: A really interesting question is how some segments of humanity or at least certain gifted individuals managed to start freeing themselves from these preconceptions and start taking a different view of nature, which led to the emergence of modern science.

Jones: In the West, you can see the Great Enlightenment.

Aspaturian: Yes, of course.

Jones: There's the Renaissance and you know—

Aspaturian: And the [Protestant] Reformation, which played an enormous role in all of this.

Jones: As an example, in 1755 there was a devastating earthquake that destroyed the city of Lisbon.

Aspaturian: I read about this in your book [*The Big Ones: How Natural Disasters Have Shaped Us and What We Can Do about Them*].

Jones: That was a turning point for looking at this issue of natural disasters as caused by God. Voltaire wrote a famous poem on the disaster in which he really attacks the idea that God would be—

Aspaturian: In control in this way.

Jones: Well, gosh, if I could remember the line. Something—

Aspaturian: Something about how does killing innocent children gratify God.

Jones: Yes. And he asks, Are the people of Lisbon any more corrupt than those in London or Paris? [The lines, quoted in *The Big Ones* read: "What crime, what sin, had those young hearts conceived/That lie, bleeding and torn, on mother's breast?/Did fallen Lisbon deeper drink of vice/Than London, Paris, or sunlit Madrid?" –*Ed.*]

So that really did reflect a turning point in recognizing the need for a fact-checker. And I think that currently in our society, we have walked away from the need for fact-checkers. It's not just in science. We have these discussions, and hear this phrase, about how you need to "believe the science," and it makes the scientist into some sort of priest instead of conveying the idea that you need to have impartial analysis to determine what the facts actually are. I don't think our current focus on "believe the science" is the right approach. "Trust but verify" seems like a better one now.

The media, the public & the need to effectively communicate earthquake science

Aspaturian: This raises for me another interesting question. Dealing with the public, so many segments of which are avid for some kind of reassurance, some kind of predictive power, how did you develop strategies for translating the science into concepts that could be properly understood? It must have been an evolution.

Jones: It was definitely an evolution. A lot of it was trial and error. It was having said something and realizing, "Oh, lord, look at how they took that one." Famous example for me was the Sierra Madre earthquake. It happened June 28th, 1991, and it was damaging enough—one woman died at the Santa Anita racetrack, and there were a lot of chimneys down across Pasadena. It happened three weeks after the eruption of Mount

Pinatubo, and we were doing a press conference that had been going on for a long time, like an hour or something. And this reporter said, "Mount Pinatubo erupted three weeks ago; did that cause this?"

Aspaturian: A reporter actually asked that?

Jones: Yeah. And so, "No, you need to understand that earthquakes of this size happen somewhere in the world a couple of times a week, and so we really don't see a correlation with a volcanic eruption three weeks ago halfway around the world." And he proceeded to argue with me that the eruption was such a big event, and this is such a significant earthquake. I said something like, "Well, the earth doesn't care that there are people nearby. On a global scale, this is a puny earthquake." I made soundbite of the day. And on some of replays, they cut off the words "on a global scale." So, "This is a puny earthquake."

Aspaturian: Oh, lord.

Jones: Oh, yeah, I was on national news.

Aspaturian: *Trivializing* the earthquake.

Jones: For the poor people who had been suffering through this, how could I possibly be belittling their suffering! So I learned not to lose my temper and say things like that.

And I think it's also the case that when you go through enough earthquakes and all the extra questions they keep on asking you, you realize what they're not understanding. One of the learning times was Northridge. Thirty-five people dead, collapsed buildings, large parts of the city badly damaged, and the aftershocks are continuing. People were really scared. It was bad. So by now we've got it pretty well down how we do the aftershock probabilities. So I started giving out these daily briefings on them—saying, for example, "we expect to have 20 magnitude 3s today," and that turned out be something that people really, really wanted. And I started realizing that I was providing a level of certainty in saying "There are probably going to be 20 magnitude 3s; and we don't know when, but probably sometime this week, we'll have another 5. So doing those briefings really helped me see how much that information mattered to people.

The other part of it is that of course, there was no mapped fault for Northridge. The other scientists were insisting that we not name the fault because there wasn't a surface structure. We could see the structure, but it doesn't come to the surface. Usually, the names of faults reflected their relationship with their surface structure so we shouldn't name the fault. That's when I realize that we give the quake a name, we give it a number, we give it a fault, we help the public to feel better.

Aspaturian: Yes.

Jones: And I could see how upset people were because we wouldn't name the fault. I can remember someone coming up with the joke—I can't remember if any of us ever shared it with the media—"Hey, I'll name it Fred, okay?" [Laughter] And there was a reporter who was just so freaked out about this, he asked: "how many unknown faults are there?" But—it obviously doesn't make any sense to ask how many unknowns are there; it's a Rumsfeldian unknown unknowns. [Reference is to a non-response response former Secretary of Defense Donald Rumsfeld once gave at a DOD press conference concerning the prospective invasion of Iraq: "There are unknown unknowns." –*Ed.*]

But it really brought home to me that all these questions and responses were because there was so much uncertainty. We had a lot of scientists at that time saying, "We've got their attention! We need to use this as a teaching moment. This is our chance to explain the scientific method!" And I ending up arguing back, "No, this is a time when people need to know that we know what we're talking about." It was really Northridge that helped me see this and helped me see that the idea that we should not be teaching the scientific method when people are too scared. Give it a couple of months or even a year to calm down and then we can teach the scientific method.

Aspaturian: Not this moment.

Jones: Not this moment. I'm trying to remember what all happened. There were things going on that were getting people more and more upset, and Hiroo Kanamori, who was director of the lab at the time, ended up decreeing that only Kate and I would give media reports from the podium in the media center, because to be at the podium was to be officially representing the USGS-Caltech consortium, and what we said had to be pre-approved. So Kerry could go and talk to reporters all he wanted about his theories, but it wouldn't be from the podium. And when things were said at the podium, it had to be things we all agreed on. Actually, Kate and I did most of them; there were others, but any statements had to be approved by Hiroo.

Aspaturian: Why did Hiroo primarily identify you and Kate as the speakers? Did he think you had a better rapport with the press?

Jones: For Kate, it was her job. I don't know quite why he designated me. He might very well have done that in consultation with Tom Heaton.

Aspaturian: I'm wondering listening to all this if you had over the time frame between Whittier Narrows and Northridge developed a somewhat better sense than a lot of your colleagues of what was required to both reassure and educate the public? You seem to have thought a lot about it while all this was going on.

Jones: I think so. I definitely thought about it, but also because I was doing probabilities, which is mostly what the media wanted information on, so it was my research that was the primary thing we were talking about. There was also this whole thing that we talked about last time about Loma Prieta. and that reporter who made a mess of it.

Aspaturian: The rogue reporter.

Jones: Ken Reich. He ended up doing a profile in the *LA Times* on me later. Let's see; I was pregnant, so it would have been in May of 1990 or something like that. I remember him saying something about how the reporters liked me because I gave the impression that I would say what I was thinking, so that people would trust it.

Aspaturian: They saw you as a truth-teller.

Jones: I was a truth-teller; I was a truth-speaker. And part of it was because I had the numbers at hand, so I felt confident being able to do it around that.

Aspaturian: Still, a lot of scientists have numbers, and they basically lack the ability to convey the significance of that in layperson's terms.

Jones: And that was probably a factor. I think Hiroo got less grief for things I said to the media than what some of the other scientists said. That probably was what it came down to. There was one occasion—this would have been after Landers—where I said there was a 50-50 chance of another magnitude 6 aftershock. I can't remember if it was the day of the quake or a within a couple of days. But there was another article in the *Times* about this being the perfect moment in earthquake prediction: "No matter what happens she's right." [Laughter]

But the article wasn't putting me down; it was a complimentary thing. So yeah, I guess I was getting the experience, and the public was benefiting from it. They were listening to what I had to say and coming away from it saying basically the right things. Because you know we depend upon the reporters to convey the information. If we haven't communicated well with them, it's going to be wrong.

Aspaturian: The public will get fed misinformation. So Northridge basically kind of brought an end to what we think of as the earthquake cycle that began with Whittier Narrows. I have one question, and then we'll kind of round out this session. Looking back, are there takeaways for you from that period that we haven't discussed?

Jones: We covered a lot of them, haven't we.

Aspaturian: Yes, it's been a very interesting session.

"The idea that you can have it all is bull. And why is that only the women's problem?"

Jones: The one other thing that happened personally is that I went part-time at USGS.

Aspaturian: Because you had two boys.

Jones: I had the two boys, and three days after Landers—where both Egill and I were at work 'til 1 in the morning, and, as I said, my aunt came over to take care of the kids—our older son completely lost it. And we realized that we couldn't keep on doing this. We made a decision then that no matter what happened, one of us was home for dinner. Period. And we kept to that through 60,000 aftershocks to the Landers earthquake. And Sven had actually just finished kindergarten at that point and was really struggling. And it turned out that there were learning disabilities, and over that year after Landers, we recognized that he needed to have special ed. He had been in private school, and we left there and put him in public school, and I started working part-time so that I could pick him up at 2:30 when school was out.

Aspaturian: Was that hard for you as a scientist?

Jones: We really had to think about it. But no, because it so clearly had to be done.

Aspaturian: But that doesn't necessarily mean it was easy.

Jones: Right. What made it a lot easier was that Mary Lou Zoback had done it before.

So I knew that you could do this successfully. I got paid less and did just as much work.

Aspaturian: I'm familiar with that phenomenon too.

Jones: I would get up and get into work by like 7:30, and Egill would get the boys ready and take Sven to school and Niels to daycare, and then I would leave work at 2:30 and pick him up from school and spend time working with him on his reading; he was dyslexic. And I also volunteered to be in his classroom. When I knew I only had this limited time, I was efficient.

Aspaturian: I bet you were, sure.

Jones: These papers that we're talking about—

Aspaturian: They were written on your half-time schedule.

Jones: Three-quarters time, I guess, I officially. It wasn't an easy time of life. But I stayed with that schedule for ten years. I think I did the best I could for both the career and the kids. And the other thing that I think came out of this, and that I spoke about in some of the talks I mentioned earlier was this idea that we've accepted women working *without accepting the consequences of women working*. The idea that you can have it all is bull. Somebody's suffering; and when you pretend the kids don't exist, you just create problems for your kids, and that's creating problems for society. And why is that only the women's problem?

Aspaturian: Maybe, as you also said earlier, the pandemic will cause a shift in thinking.

Jones: Well, it was also clear to us that if my husband had given an interview after Joshua Tree carrying the baby, it would have been professional suicide. It was not a time when a man could have done it. And actually in this talk I gave, I said that if we ever reached the time when he could have done that and have the reaction be "what a beautiful picture," then we'd have gotten somewhere.

Aspaturian: Do you think that time has arrived?

Jones: I've noticed in the past couple of years, there seem to be sports figures—have you noticed?—giving interviews after the game holding their kids.

Aspaturian: I'm not exactly into sports.

Jones: Well, neither am I but I've seen the pictures, and I'm "Oh, I recognize that, right."

Aspaturian: It has changed a bit, yes.

Jones: That part's changing, but it was really clear at the time that my husband could never have done it. And I think that's a really important part of this. I think that now it is better, but he couldn't have done it then; it had to be me. It would have been much more difficult for him to try and take time off for the boys. It was also true that at the time, I could do more for Sven than he would have been able to do. So it was the right thing to do. Being able to be there after school—it's only a couple of hours, but it makes such a difference.

Session 6, May 4, 2022

Father's reaction to daughter's emergence as "the earthquake lady"

Aspaturian: Today is May 4th.

Jones: May the fourth be with you.

Aspaturian: A question actually occurred to me as I was driving over to campus in my car, which is that your dad had such an influence in many respects on your early life. Was he still alive at the time that you began to come to prominence in the earthquake context?

Jones: Yes, yes. He died in 2007. He actually died during the preparation for ShakeOut. I was getting ready to go down to Sacramento with Dale Cox [USGS, Colorado] to brief them on ShakeOut [*Session Eight*] and try to engage the Marines in participating in the first drill when I got the phone call that he had been in the hospital, and that it was liver cancer; and he was dead five days later. But he saw a lot of it. He had seen me doing all the interviews and at that level.

Aspaturian: What was his reaction?

Jones: Oh he was proud of me, of course. Though he would tease me that I always had this ability to sound very sure of what I was saying, whether I was or not. [Laughter] And that that had contributed clearly to my success in communicating about earthquake hazards.

Aspaturian: Well, he knew you.

Jones: [Laughter] Yes, he knew me very well. And he was very proud of me. When I went to MIT, he was really proud—

Aspaturian: Of course.

Jones: I think that mattered as much to him as anything else. He was more interested in the scientific side of it than the public side. The public recognition didn't really matter to him, but that I was a research scientist at Caltech meant a lot to him.

Relationships with Caltech colleagues: "USGS–Caltech collaboration is maybe unique"

Aspaturian: Speaking of Caltech, I wanted to ask what your relationships were with your colleagues during the '80s and '90s in this earthquake–intensive environment. You mentioned Kerry Sieh, Hiroo Kanamori, Tom Heaton.

Jones: And Egill Hauksson [laughter] my husband.

Aspaturian: Of course. We've talked about him!

Jones: We have talked about him. So the USGS–Caltech collaboration is maybe unique. USGS is at quite a few universities and has offices on their campuses, but the level of collaboration here at Caltech is probably stronger than at most of the others and much more of a real partnership. I mean there are places like UW [University of Washington], where the USGS office is quite small, and when you hear about earthquakes there, all you hear about is the scientists at UW. Whereas in the Bay Area, USGS works with Berkeley and with Stanford, but it's always USGS who's reporting on earthquakes.

Here at Caltech, it's been much more of a Caltech-USGS partnership. I think this is partly because the USGS office wasn't too large when I arrived—I was the fifth research scientist to join the office, and I think we got up to a maximum of twelve scientists at one point. It's below that now. But it was very much a partnership with Caltech. I was offered a visiting research associate position at Caltech in 1984, a year after I got here, because I'd already started working with Caltech people, and I had been coauthors with quite a few of them—Kate [Hutton] obviously and Kerry and Hiroo and Egill and Tom Heaton, although I think some of my papers with Tom were after he moved to Caltech because he was with the USGS when I arrived.

And so that collaboration as scientists is strong and always has been. It goes back and forth; it's fluctuated over the years. When I first got here, there weren't many people actively involved in local earthquakes. You know, Kate was hired to sort of keep the media from bothering the professors, and of course she was not trained as a seismologist; she was a—

Aspaturian: Astronomy, I believe.

Jones: Yeah, radio astronomy. But she ended up doing quite a few seismology papers, especially on the data from the networks. She had incredible perseverance and skill at making sure that very complex data sets with lots and lots of data points on hundreds of thousands of earthquakes got done right and were kept in place. Clarence Allen was the professor in charge of the network in the '80s, and I'm coauthor on a few things with him; and somewhere in the late '80s, early '90s, Rob [Robert] Clayton took charge of the network, but Rob never did a lot of work with the network data. Clarence did more, and then Kerry of course is a geologist, not a seismologist, but we coauthored several papers where we were putting the two types of data together.

Being a researcher here has been a special situation where I could dedicate myself to the public good. I really took to heart the old USGS motto "earth science in the public service." That mattered to me. I wanted to do work that benefited the public, but by being here on campus I could do it surrounded by and working with incredible researchers. So I always thought it was a really good collaboration. It also meant that when we were dealing with different groups on the outside, we could choose whether we presented a government face or an academic face. When it came to getting seismic stations at China Lake, being a government agency really helped. There were plenty of other situations where if we're coming in as government, they aren't going to want to talk to us, but with academic support, they're happy to partner. So I think in that sense we did a good job of being what we needed to be for whomever we were working with. As an intellectual environment in which to do this, there's nothing better than being here.

Aspaturian: As personalities, what were these individuals like to work with?

Jones: Oh, the guys who did the local earthquakes I sometimes think were the nicer ones because historically, the most prestigious geological research at Caltech was focused elsewhere—[Beno] Gutenberg was all about the deep earth, right? And we used to joke about the scientists whose attitude, you know, was "Once the studies got shallower than 400 kilometers down, who cares?" So there was a little less prestige, if you will, associated with doing local earthquakes.

Aspaturian: Even after the late '80s–early '90s sequence began?

Jones: Well, Northridge [1994] helped turn things around.

Aspaturian: I would imagine.

Jones: Actually, it was before that. Because a lot of it was Hiroo, who—

Aspaturian: This is Hiroo Kanamori.

Transformative impact of seismic network broadband digitization & TERRAscope

Jones: Hiroo Kanamori is a self-effacing sort of guy, a very quiet guy with vision. He and Tom Heaton were the ones who saw the possibilities in really digitizing our network and going to broadband seismology. Traditionally, seismology had separate instruments for looking at distant earthquakes at really long periods and a whole different set of instruments for looking at local earthquakes at high frequencies. When you digitized them, if you actually had a digitizer and a computer at the station, you could record everything and then be able to filter out the parts you didn't want. And the reason behind this is that there's a very large signal all around the world at around six seconds. It's called the microseism. It's probably related to wave action around the oceans, but the earth sits and vibrates at a period of about six seconds, and there's enough noise at that level that it's really hard to detect anything else.

Aspaturian: I see, I see.

Jones: So we have the long-period networks to look at longer periods and the short-period networks to look above it, and then you looked at local earthquakes with the short periods and at distant earthquakes with the long periods. If you can filter the microseism out digitally, you can just record everything and not worry about it.

You do have to have a very good digitizer. One of the big deals at that time was the amount of data we collected. When we'd been digitizing the local short-period seismic network data, we had 12-bit digitizers because the network still produced an awful lot of data that had to be stored at a time when data storage was expensive. I think I mentioned how at one point, I had a hard drive on my government property list that I'd inherited from somebody else that was 450K of memory and was supposedly worth $22,000. [*See also Session Four*] By the time we got rid of it, it was worth nothing. [Laughter] You could do what it had done with a thumb drive. But the limitations of data were really big in that move into the digital world. And so when we went to our first broadband sensors in the mid-'80s, late '80s —

Aspaturian: Was this part of the TERRAscope project?

Jones: That was TERRAscope. In the mid-'80s, the Streckeisen seismometers came out. They were high-performance broadband seismometers developed by people in Switzerland; I think it was $25,000 for one seismometer—way more expensive than the other things we were doing. And they produced these signals over all the frequencies, and you digitized the signal with a 24-bit digitizer. So they produced far better resolution than what we had had on the old systems, but also generated a huge amount of additional data. Hiroo Kanamori was the one who had this idea of "we've got to bring these seismometers in."

And he and Tom Heaton came up with the idea for TERRA-scope.

I think the first proposal went in right around '87, maybe after Whittier Narrows. And the LK Whittier Foundation ended up funding this. By 1994 we had seven of those instruments out.

ASPATURIAN: So timing was so fortuitous.

JONES: Of course now we have hundreds of them. By the time Northridge happened, we did have several of these instruments, and we had another one that was sitting in the subbasement at South Mudd getting ready to be deployed out in Glamis, which is near the Arizona border along the Colorado River Project.

ASPATURIAN: Glamis? Like the Scottish castle?

JONES: I guess. I've always thought of it as that station out in eastern California. It's an old Caltech station, and we were going to switch it over to a TERRAscope station and thereby achieve a much bigger spatial range. And so that instrument was sitting in South Mudd when Northridge hit.

The Northridge Quake strikes, January 1994

The “earthquake lady” and Caltech/USGS colleagues, including geology postodoc James Dolan (top left photo, at right) and staff seismologist Kate Hutton (bottom right photo, at left) deal with seismic data and media mayhem following the Northridge quake. *Caltech photos by Robert Paz*

Actually, when Northridge hit, Egill, who was running the seismic network at that point, was in Boston at a meeting of seismic network operators who were starting to move to these types of systems. We were probably one of the largest users at that point, with our seven stations. So if you remember, it was at 4:30 in the morning when Northridge hit—

Aspaturian: I remember it well.

Jones: Which is 7:30 in the morning in Boston, where Egill was. He had just had breakfast and gone back to his room to brush his teeth before packing up and going to the second day of the meeting and planning to come home that night. He turns on the *Today Show*—this was back in the days when the morning shows were where you got breaking news, and— "Los Angeles destroyed by earthquake." So he goes running out to the airport: "I'm a Caltech seismologist! I've got to get back!"

They had just shut down LAX because of the damage in the control tower, so United Airlines actually put him on a first-class seat—the only available one into Ontario—*to get the Caltech seismologist home* And so he's on the plane, and if you remember in those days there were phones on the back of the seats on some of the planes. And so he called me with some ideas because he's sitting there doing nothing on the airplane as we're all in Pasadena going bonkers trying to make sure the data's coming through. He had time to think, which none of the rest of us did.

Now, my mother had died in 1989, and my dad had remarried in 1992, and my stepmother had moved in with my dad in Westchester, but she owned a house in Calabasas which at the time was sitting empty because she hadn't figured out what to do about it. Her name's Lucy, just to make things confusing; her last name is Kluckhohn, and we used to call her Lucy K. So Egill gets this idea and calls me on the phone: "Do you think we could ask Lucy K if we could put the TERRAscope station into her garage?" Because at the point, we needed power, and we needed phone lines, and that all takes time to arrange. It would have taken us a month to put it out as a deployment in a regular site.

I called Lucy, and she was like, "Go for it." Twenty-four hours after the earthquake, we had it installed in her garage in Calabasas. Calabasas of course was sitting there on top of the Northridge aftershock zone and being able to get that one extra station there was this really big deal. It was hooked up to her phone plugged into her wall, and it ran for several more years. Even when she rented out the house, she kept it off limits to the tenants: "You don't get this part of the garage." And we kept on running the station there for I don't remember how long, quite a while. So the TERRAscope was changing how we did seismology. And that point, we were the only local network that was running broadband, and it was considered sort of unnecessary: "Why do you bother with that; we don't need that!" But of course, new data's always going to get you new things.

Aspaturian: When you say local network, do you mean Southern California or statewide?

Jones: All right, so the networks in California traditionally were divided between north and south. We had our

Gutenberg-Byerly discontinuity. [*See also Session* *Four*] So the USGS was of course involved in both, but there was the office in Menlo Park, which ran one network up there and partnered with Berkeley, and then the office down here, where Caltech and USGS ran the network together. But in Northern California there was basically a Berkeley network and a Menlo Park network; down here they were much more a shared network. There were still the stations that traditionally belonged to Caltech and were still maintained at that point by Caltech technicians, and the USGS ones maintained by the USGS technicians. But the data were all recorded together and processed together. We really operated as one network, and that was what made us different.

Aspaturian: I imagine there were enormous advantages to having this kind of local synergy.

Jones: Oh, the local synergy was here. We've already talked about how it made a difference in how we were able to work with partners on the outside from either the government or academic standpoint, depending on which would work better. But the partnership also led to innovation. It wasn't somebody from the outside coming in trying to convince the USGS to change the way they were recording data; it was us here. We had to fight Menlo Park on this to a certain extent.

Caltech–SoCal USGS partnership propels innovation & reshapes relationship with USGS

Aspaturian: Ah, that brings me to another question, based on what you said about SoCal USGS traditionally being kind of a junior partner to Menlo Park. I imagine things must have changed during the late '80s and '90s.

Jones: Only in the sense that [laughter] we partnered with Caltech and did what was good here. So we're part of the same organization within the USGS; back then it was called the branch of seismology, and now it's the earthquake science team, or earthquake science center, I think. The first time any head of that group was out of Southern California was just a couple of years ago, after I had retired—and it was only an interim head. So we always reported to Menlo Park. We were not only smaller; our bosses were always in Menlo Park. Menlo Park was big enough that they didn't really listen to much of what we were saying about how these sorts of things would be run. And especially early on. We got listened to more once we created TERRAscope with Caltech, when the attitude was like, "Oh, okay, this is where things are going." That changed the dynamic by quite a bit. But during the '70s and '80s, there was one scientist put down here to supervise technicians and do what they were told by Menlo Park. As the group grew down here, we became more independent.

Then, having Tom Heaton come in, first to USGS, later to Caltech—he's someone who does what he thinks is right, and he worked very closely with Hiroo. Jim Mori we hired in or about 1987, and he, Tom, and I were sort of the senior scientists. There had been a couple of others; they ended up leaving. So it was sort of the three of us that were running things. Somewhere in that period—maybe '88, before we'd really started up with TERRAscope—we took a strong motion instrument—the ones that were traditionally run at really low gains to look at the strong motions that happen in a magnitude 6 or 7—and we hooked it into the network. We did it by just putting it into the basement of our building on campus.

Aspaturian: South Mudd?

Jones: No, the USGS office, at 525 South Wilson. It went into the basement of that old building, and we glued it onto the nice concrete foundation there and put it straight in to feed into the computers. In a local network, the standard short-period seismometers are amplified to see the small earthquakes. With only 12-bit digitizers, the dynamic range is small, so those recordings are always clipped in the big earthquakes—you can see the station moved a lot, but you can't see how much.

It was so useful to be able to see the bigger motions from that strong-motion instrument that we put in a second one. I'm trying to remember where the next one went, but it was actually on a phone line, and we got ordered by Menlo Park to disconnect it: We couldn't waste the money on a phone line for an instrument that would record so little data. We were ordered to take it out—and we ignored 'em. Or we pretended like we had, and we just kept it because we could see that being able to actually not just see when the earth was moving but how much it was moving was important.

Aspaturian: Yes.

Jones: And it was part of that technological evolution down here, and then we got the Whittier money and we put out the Streckheisens and connected them to the local network, and we started getting these really high-quality data that traditionally would have been in an observatory. And it just meant that you could do a whole bunch of new things.

Aspaturian: Yes. Major shift.

Jones: It was a really major shift. And it happened because Caltech and USGS were working that closely together. And I don't think it would have happened—it didn't happen—in a place where it was only the university or only the USGS. So Caltech–USGS not only wrote papers together, but we also were doing this technical development and envisioning what the future could look like. All of this at a time when there were enough earthquakes to show what the value could be.

Southern California Earthquake Consortium [SCEC] established to coordinate regional earthquake research & response

Aspaturian: Stepping away from Northridge, in 1995 you wrote "Putting Down Roots in Earthquake Country."

Jones: That's not stepping away from Northridge, actually.

Aspaturian: No, but it marks a bit of a shift from you in terms of—

Jones: No, it doesn't. Actually, it was a one-off at the time. There had been a public insert that went into the newspapers in Northern California. I can't remember what it was called but they did something like an earthquake probability report. And then the other big thing that happened was that in 1993, we started the Southern California Earthquake Center (SCEC). This was at a time when the NSF had just started funding these science and technology centers—S&T Centers. These were sort of big efforts that would go for five years and then be renewable for a another five and would allow you to do larger-scale projects. At that point Caltech already had one center here—I can't remember what it was in—and a second one that they were half part of.

So we started talking about whether we could do this for earthquakes, and Don Anderson, who was the director of the lab at that point, had some discussions with NSF, and they were really clear that Caltech wasn't getting another center to itself. But we wanted to have an earthquake S&T Center here. So here's another northern–southern USGS dynamic. In Northern California there's the USGS in Menlo Park and Berkeley and Stanford. In Southern California, obviously Caltech's really big in earthquake research, but so is UCLA, USC, and UC San Diego. UC Santa Barbara—they were there early, too. So down here a lot of other schools were engaged in this. The head of the earthquake program for the USGS at that point was a guy named Rob [Robert] Wesson. And discussions went on with him and with Don Anderson around how to do this.

One of the issues for the USGS was that in its Northern California branch, if you want to say something, you say it and nobody else is talking, right? In Southern California because the USGS office is very small and the research is funded externally, there were all these different groups involved. The message was less coherent to the public. and that was recognized as a problem.

Aspaturian: Recognized by—

Jones: The USGS in D.C.

Aspaturian: I see.

Jones: So the way things sort of played out down here is that there would be rumors about predictions, or papers concerning predictions, that were written and hyped by their authors— or by their authors' universities, more likely.

Aspaturian: Too many cooks, in other words.

Jones: There were lots and lots of cooks, and it was a confusing thing. It was also obvious with all these earthquakes happening that we wanted to be doing more in Southern California. We had Whittier Narrows, we had Upland, we had Sierra Madre. So there's all this earthquake activity going on, all these different players, and a really small USGS office here compared to Northern California, and the USGS in DC realized that we needed to be doing more here in dealing with all of these different groups. And Columbia University, which is not even local, was also active doing research here. So a meeting was held in late-winter, early-spring 1989 to talk about how we would do this. It was up at Lake Arrowhead at the UCLA center, and I remember it because Egill and I were the youngest people there, and we had a two-year-old that we brought with us, and my parents came and stayed in a condo and took care of Sven when we were in meetings.

But the discussion centered on, How do we do this? Well, USC had just hired a really famous seismologist called Kei [Keiiti] Aki. He had been at MIT, and USC bought him away. He had this vision of creating this master model of earthquakes that would pull together everything we know as a possible path to earthquake prediction. It was like, all right, Prediction is getting to be something we're getting embarrassed about, but is it because it's really fundamentally unpredictable, or is it because the data's so complex, we can't see the big picture of how it all fits together. And the only way we're going to find out is if we try to integrate all this stuff. You've got data from geologists, you've got data from seismologists, you've got data from geodesists. All of them are getting different types of data that are important for this problem, so how do we pull it together?

And so, we put together a proposal for an NSF Science and Technology [S&T] Center. Then in 1990 we had the NSF site visit, and I remember it because I was pregnant with my second kid— that's how I time these things. It was at USC because they were the lead institution for the proposal. NSF took its time, but USGS supported the idea, and so the beginning of SCEC was actually funding from the USGS. They put up a million dollars. And throughout all the ups and downs with NSF, the USGS always put in at least a million dollars every year to get this going. So that dynamic is also going on.

But then you've got the dynamic of having all these different local universities, and how are you going to organize and co-ordinate this? And everyone sort of always waits for Caltech because Caltech leads in earthquakes in Southern California, but at this 1989 meeting Don Anderson had said, "NSF made it really clear, we're not getting an S&T center, so no we don't want to lead this."

And now Kei Aki was at USC, having just been hired away as the superstar coming to a place that really wasn't that big in earthquakes up to that point. But it had the LA network, and it was trying to move up. I don't know if it was Kei or the administrators above him, but USC agreed to pass-through S&T Center funding without taking overhead, which was huge. None of the UCs were willing to do it; Caltech wouldn't have done it. But USC realized, and they were absolutely right, that they would be hiring the center's core staff and that they would be getting a lot more staff in now that they had Kei, and they'd get their full 60-some percent overhead on that staff.

Aspaturian: That was shrewd.

Jones: It was a very shrewd move because then everybody else was supporting USC as the lead. And today USC is a really big leader—

Aspaturian: —A big player.

Jones: A big player in the earthquake field because they were willing to do this. And so the proposal went in: I think we had eight core institutions and another six participating institutions. The core institutions had to put up their own resources —$35,000 per year each, and they had to document $35,000 a year of work they were doing. And, as I said, the center got funded at first only by the USGS. I'm trying to remember exactly when the NSF funding came through, probably in '91 or early '92.

And so by the time Landers happened [1992], SCEC was up and running. It changed the way we responded to really big earthquakes because now we had a mechanism to coordinate our activities. And for the USGS it was brilliant. You could now have SCEC coordinating the academic people, getting one message out of the academic people, and one place to deliver funding to them. It changed the dynamics of earthquake work in Southern California, and it became the place that academic people wanted to work because they were welcome. And we then had annual meetings that became so good and wonderful that Northern California decided to have Northern California meetings, bringing in all the individuals they were funding, because they saw how well it was working down here. I'm not sure where it is now, especially with the pandemic, but the last SCEC meeting I went to, I think had 600 people.

Aspaturian: What year was that?

Jones: 2018. But establishing SCEC was a real important development, and it was centered on this idea of the master model focused on how do you integrate the data together. And then when Kei retired, it almost fell apart.

Aspaturian: When did he retire?

Jones: This would have been about the time I took over scientist in charge, so '97, '98. [It was 1995. –*Ed*]

Aspaturian: The date I have for you is 1998.

Jones: Yeah, that sounds right. After that, the emphasis wasn't quite the same: It got a little focused on doing just the hazard assessment—getting almost too practical, so it wasn't a place that inspired science.

Aspaturian: It got away from the fundamental research in other words.

Jones: Yeah. And then in 2000 USC hired Tom [Thomas] Jordan to take over running it. He was able to get more funding coming in. He got SCEC to grow, and then he just retired a few years ago.

Aspaturian: Where did he come from?

Jones: MIT. [Laughter] Another California transplant. I think it's partly that MIT's got great seismology, but it doesn't have its own earthquakes.

Aspaturian: Of course.

Jones: And there is a point in your career as a seismologist—

Aspaturian: Where you want to be where the action is.

Jones: You want your own earthquakes! So both Kei and Tom came out because of that. All right, so this is all going on, and the first thing that SCEC did was create a model of the risk in Southern California. The distribution of a similar model spurred the publication in Northern California that I mentioned earlier. So this is a long way to get back to "Putting Down Roots."

Communicating seismic hazards & preparedness strategies in "Putting Down Roots in Earthquake Country"

ASPATURIAN: This is all the context you're giving me.

JONES: Right. So in 1993 we now have the first unified picture of seismic hazard risk analysis—a map for Southern California. And then in '94 we have the Northridge earthquake, and we have everybody really scared about it. Actually, maybe the model came out in '94 after the earthquake; I can't remember exactly when the science got done. But it was sort of Phase Two of the master model—all our geology and geodesy put together. Phase One had been some summary we put together after Landers. It was what we call time-independent: There was no way of saying this earthquake's more likely now than it would be at any other time. So, on a fault that has an earthquake, say, once every hundred years, a time-independent model gives the chance of an earthquake in any one year as one percent. Now, with a time-*dependent* model, that gave us some justification for saying if it's been 150 years since the last earthquake, and if the average is once every 100 years, we are now somewhat more likely to have the next quake. But then the question was, How do you quantify how much more likely? The first time-dependent model didn't come in until like 2014, I think. And even then, people still argued whether or not it was justified. So this was a time-independent model, but it was integrating all these things that SCEC had been doing and pulling together a baseline that we could improve from there.

Then the question became, How do we make a public version of this? The need was so clear—and nobody was stepping up. So at the time, with Northridge, I was doing aftershock probabilities. I used to give a daily report on, say, "the number of 3s expected today" as the aftershock sequence was going on, and then the number of 3s expected in the next week as it gradually died down. So I was in that role of publicly talking about probabilities, and there's the need for this report, and I finally, I said, "Okay, I'll take it on." I actually connected with a woman that I had met personally through the kids at school, but she also had done contract work as a designer for Caltech—Margi Denton.

Aspaturian: Oh, of course. Margi did some design work for our office when it was Caltech public relations.

Jones: She'd worked for Caltech, and, as I said, I knew her through the kids, and we ended up contracting with her to put a publication together. I spent quite a while—it was months—working with her on it. I would go to her office, which was in her house, and we would sit down and lay out ideas, and I would write something and then we would talk about what graphics needed to be done. I remember at some point having discussions about how I always wrote as a scientist, where you have to explain why something's true before you give the answer, because otherwise, your colleagues are going to jump down your throat. The last thing you say is the most important—that's always the punchline. She was actually saying, "I'm going to take your last line and put it in red because you don't put it until the end, and yet that's the part that everybody needs to pay attention to." And we'd then work together on how to reorganize the pages so that that punchline was a little more upfront without violating my feeling that I hadn't explained why.

ASPATURIAN: So there was a learning curve for both of you, it sounds like.

JONES: Yes. We both changed a lot. Margi was like, "White space! white space! You've put too many things in here, and they aren't going to see it if it's that crowded." And then she got the cover idea of a house growing roots to show stability in earthquake country. And we were looking at that artwork one day when some other client of hers came by—I don't even remember the other person's name or what it was she was working on—looked at it and said, "Putting down roots in earthquake country." And we're like, "Oh! There it is!" And that's what it became. It took as much work as any scientific publication I've ever done.

ASPATURIAN: Of course; it would have.

Jones: And I worked a lot with the social scientists. I started off by going to Dennis Mileti, who was a very famous guy in sociology of risk communication.

Aspaturian: Where was he based?

Jones: University of Colorado. He was head of the Natural Hazard Center there at the time. At some meeting—I can't even remember what meeting it was—I sat down with him, and he just sort of spewed out at me all these ideas, and I was writing down so much that I actually filled up the cardboard backs of my paper pads—filling them up with all of these little tiny notes. I've still got the notes in my files about the things he said we should be trying to do, like "Never pose a problem without offering a solution."

Aspaturian: Was this your first major exposure to this kind of feedback?

Jones: It was the first time I tried to understand what the social scientists had to say. So I wrote a first draft and sent it to him, and he wrote back, telling me that I was doing everything wrong. Basically he was regurgitating what he told me at that first meeting: It was as though he criticized it for doing all the bad communication he knew that scientists did, without really reading what I sent him. But I made him sit down and go over it with me in more detail and I got him on board with the project. We got that first version of what came to be called "Roots" out explaining the probabilities, but I refused to call them probabilities. Instead I called them "expected number per century." So what we plotted was the number of times per century that an area was expected to receive intensity 7 shaking.

Aspaturian: Something much easier for people to understand.

Jones: I thought so. When it was so successful, and they decided to do a version for Northern California, those people overruled me and turned the language back into probabilities because they knew they were right. And in fact, they're exactly the same thing. It was just they were adamant that "we need to talk about probabilities." And now with time, I can also see that calling it "probabilities" is focusing on what we don't know whereas using the words "on average twice a century" focuses on something much more concrete, and that changes the emotional reaction.

Aspaturian: Of course it does.

Jones: So.

Aspaturian: Probabilities are very abstract for most people. They tend to ignore them.

Jones: Yeah. So this project was a diversion from my usual research. Also by this time, I'm working part time—I went to part time in '93.

Aspaturian: Yes, you mentioned that last time.

Jones: Right. So working on "Putting Down Roots" was a big chunk of my part-time work. I got a couple of other papers out in that time period. So during that time, it was being part of the team that was growing TERRAscope; it was going out and doing this public outreach; it was doing more research on seismotectonics, and what was going on there. And then Jim Mori got a job in Kyoto, and I ended up being convinced to take on being USGS scientist in charge in Southern California.

Aspaturian: This was in 1998.

Jones: Right. Being scientist in charge for the USGS office and doing everything related to management—budgets and so forth—it's sort of accepted that you don't get nearly as many papers out. And so I spent the next eight years in my management stint. One thing I got accomplished was an expansion of our office: I managed to double the size of the staff because I knew the need was there. Physically, we got our office enlarged—we negotiated a new agreement with Caltech to get more space. But I also then in 2002 got appointed to the State Seismic Safety Commission.

Aspaturian: And that is an obvious stopping point for today.

Jones: I think that would be great.

Session 7, May 11, 2022

“It was satisfying, getting stuff done”: Pasadena USGS scientist in charge, 1998–2006

Aspaturian: When we left off last time, you had just started to mention that in 1998 you became scientist-in-charge of USGS here in Pasadena, and that reminded me of something I'd been meaning to ask you: What is the demarcation geographically between Northern and Southern California USGS?

Jones: We call it the Gutenberg–Byerly discontinuity after the original heads of the Caltech and Berkeley labs. [*See also Session Four*]

Aspaturian: And for us uninitiated, where is that located?

Jones: It's a line basically from San Luis Obispo, angling north up to—where does it really go now? Mammoth is considered to be Northern California, but just barely. The dividing line used to be horizontal, going straight east-west, but then the 1946 Walker Pass earthquake happened on the east side of the Sierra, and Berkeley couldn't get to it—you couldn't cross the mountains in winter in 1946. And so they tilted the line counterclockwise so that now all of Parkfield and the central California area goes up to Northern California, but then the eastern Sierra are part of Southern California. And then also University of Nevada Reno got started, and they are responsible for Nevada. So there is another boundary at the California–Nevada border.

Aspaturian: Has there ever been a quake where the jurisdiction has been in dispute?

Jones: There have been plenty at the border—for instance, Walker Pass [1946]. That was sort of the first time where we really said, "Okay, here's the border, and we're going to shift this." Parkfield is an interesting case. It falls within Northern California, but in 1857 it turned into the southern San Andreas earthquake. So it's like the foreshock would be theirs, and the main shock would be ours—that would be an interesting one. Never happened—yet. I guess the San Simeon quake in 2003 was clearly Northern California. But, if the quake's south of the Tehachapis, there's no question. And you know Kern County [1952] was never considered Northern California at all.

Aspaturian: So that was a southern one as well. Now how did you become Southern USGS scientist-in-charge?

Jones: I ran out of excuses. [Laughter] The standard in the USGS is what we call rotational management. None of the scientists are willing to be supervised by nonscientists. And none of the scientists are very good managers. And none of the scientists want to keep on doing it, mostly. So the usual thing is rotation. Here in Southern California it began with Gary Fuis, then Caryl Johnson, and then Tom Heaton took over in '85.

Caryl left in ’86 (she moved to the USGS in Hawaii), and then in ’92 when Tom was really saying, “I’ve had enough,” Jim Mori took it on, even though he was younger and hired after me. I had a one-year-old and a five-year-old then, and I basically said, “I can’t, I can’t.” But then Jim—

Aspaturian: He went to Japan [Kyoto University], I think—

Jones: Yeah, but he was also at the end of his rotational stint, and they’re just saying, “Really, you’ve got to take this on.” At that point, the boys were older and in school, and so then I did.

Aspaturian: You went back to work full-time at that point?

Jones: Actually, no. I still did it going home at 3 o’clock. They were older by then, but we still wanted them to be able to come home after school. And basically I put down leave without pay as needed to fill out the times that I wasn’t working.

Aspaturian: So what was that like for you, moving into a management position and taking on a new portfolio of responsibilities?

Jones: It means I didn’t get much research done. There’s a couple of papers that should have been finished up then and never did get finished. And I was reasonably good at it, but being a manager is much more tiring than being a researcher in ways that surprised me.

Actually, I talked with Paul Jennings about it. I think he was provost at the time, and I had just taken the job on, and maybe it was because we were negotiating expanding the property that we needed to have. Our office was growing, which was great; USGS was finally putting in resources down here, but we needed space. Paul, with his experience of management, said that it was the process of the imminent displacing the important. [Laughter] I think that's really it. As a scientist you're working on one thing for six months. As a manager, you've got six different things every day. And so that sort of regular disk-fragmentation was probably the most stressful part of it.

But I'd also been here fifteen years at that point and could really recognize the pretty severe imbalance that USGS showed in not putting the resources into Southern California that they did in Northern California. I remember having arguments with Menlo Park about it. The funding of earthquake studies was classified as fundamental studies or regional studies. But they would insist that somebody working on some earthquake physics problem in Northern California is an earthquake physicist doing fundamental studies, but someone doing the same thing down here, was a Southern California specialist. All the money going to SCEC was classified as Southern California regional studies, even though most of what was going on at SCEC was earthquake physics. I could see this, and I was frustrated by it. And so one of the things I could do was try to fight for more resources down here and as part of the management team I could be part of that decision making process and get new hires. I *was* able to double the scientific staff.

Aspaturian: Does USGS funding come exclusively from the government?

Jones: Well, you can get an outside contract, and at times it's encouraged and at times it's not. But then that's also pulling you away from whatever your government tasks are. Because with a contract, you have to hand over your deliverables. So we would do things like getting the Naval base at China Lake to give us $25,000 a year because we were running stations there, on the base. We actually made several of those arrangements. So when it came to the network, we might be more aggressive about finding other sources.

This is also the time when TriNet was being proposed, and we got FEMA funding for the development of TriNet. Jim Mori was part of really getting the idea started. I was just about to take over as scientist in charge—I'm trying to remember the exact timing. I was at a meeting in D.C., and I remember talking with Frank Press, who was at the National Academy of Sciences at the time, about the pitch we could make about why FEMA emergency management money could be going to running a seismic network. And he did end up going in and advocating for us. To actually get it to happen required a lot of political negotiations—not quite a shift in priority, but a lot of people had to agree along the way in nonscientific circles.

Aspaturian: Was this your first sustained encounter with policymakers at the national level?

Jones: Well, no. Back in 1992, I was appointed to the Board on Natural Disasters [BOND] in the National Research Council. I had been on another NRC panel before that, in 1988, but on regional seismic networks, where it was a one-time thing of writing a report on their status.

Aspaturian: But that was largely in a research capacity—

Jones: And on BOND it was completely a research capacity. I was there as a subject matter expert. NRC committees get funded by different government agencies, and BOND was funded by FEMA. I went to two meetings a year, and actually Bill [Wilfred] Iwan [professor of civil engineering, emeritus; d. 2020] here—

Aspaturian: Of course, I remember him.

Jones: He was chair of BOND for the second half of the time I was there. So that's what really exposed me to all of the policy aspects. Ellis Stanley, who was at the time head of emergency management for Fulton County, around Atlanta [Georgia], was put on BOND, and I got to know him. And then he ended up being hired to become the head of emergency management for the City of LA, and he was here for ten years. We had become friends on BOND, and I interacted with him a lot here because of that.

So BOND was the beginning of the process. Actually when I took my government job in 1998, I resigned from BOND because now I was management in the government and therefore not there as a subject matter expert. And at that point, I was traveling; and adding two or three BOND meetings a year when you've got small children was a stress on everybody. I did it, but we were all aware of the challenges of that.

Aspaturian: Was it in your view sort of serendipitous that the major earthquake clusters had seemingly halted, and you did not have to divide your time, as you might have, if we had had more events like Northridge?

Jones: Well, remember Hector Mine [1999] was about a year and a half after I took over. And that was a magnitude 7.1, which was huge. I mean it wasn't as much policy driven—

Aspaturian: Exactly.

Jones: But scientifically, it was really big. And in fact one of the things I'd done—I'd forgotten this—is that after I became scientist-in-charge, my relationship with SCEC changed somewhat. I was then both on the Board and the planning committee of SCEC.

Aspaturian: Yes, which we discussed last time. [*Session Six*]

Jones: The head of the USGS office always had a significant role there. USGS had coordinators to manage funding in different regions of the country, and the head of our office here was also Southern California coordinator and thus also on SCEC, and that first year that I was managing the office we were saying, "What are we going to do if there's an earthquake? We've now got SCEC; it's grown up"— they had developed their outreach program over this time. And we actually went through a planning process with SCEC for earthquake response—not the practical emergency management priorities but our priorities as scientists. Aftershocks are the only earthquakes where we know where they're going to happen. Aftershocks give us a unique opportunity to get records very near the epicenters, because we know where these earthquakes will be located, and so now we're asking, What is it that we're trying to achieve out of this process of recording aftershocks?

And so we actually set up a scientific plan, or we were working on it—we had drafts—when Hector Mine happened, and the result was actually very impressive. We were able to coordinate our response. It actually helped that the earthquake's fault was on a military base. It was in the Twentynine Palms Marine Corps Base, which is where they teach their recruits how to drop bombs. The place is littered with unexploded ordnance and active ordnance. So you couldn't just go out to the fault. You had to wait until there was no testing going on and then you had to be escorted by the Marines.

But it was good. It meant that we were able to be much more organized about how we responded, so we divided up the fault: "You've got 36 hours that they're not bombing," and we lined up ten or twenty geologists, and they'd get in all the mapping that they could in this timeframe. You didn't waste time duplicating efforts, and then you had to actually coordinate how you published. All of those were good things. And also because Hector Mine was in the middle of the desert in a bombing range, the damage was very, very limited.

Aspaturian: Yes.

Jones: And so it didn't require so much social response, but it was a huge scientific experiment. And I think we handled it pretty well. But from my point of view, it took up just as much time as if there was the public angle.

Aspaturian: You mentioned expanding the staff. From what to what?

Jones: From five to ten PhD researchers.

Aspaturian: You doubled it, in other words.

Jones: Yeah. I got new hires coming in and made sure that when, for instance, Jim left, they didn't take his position away. I was able to rehire for it and then add more staff. There was one position where we had—this is actually sort of tricky—two really, really good candidates, and there was one that was much preferred by Menlo Park, and our committee chose the other as the top one, and so the only way Menlo Park could get the one they wanted was by hiring both. That worked. Various ways in which to try to strengthen the office. So it's a really different activity.

But it was satisfying, getting stuff done. We needed new offices for the new people we were getting, so we ended up expanding the house that we rented from Caltech and renegotiating the contact.

Aspaturian: This is the one on Wilson?

Joining State Seismic Safety Commission (2002) marks major move into public policy

Jones: Yes. And all of that took time. I was finally sort of settling into it when the head of the governor's office of emergency services [OES] asked me if I would be willing to be nominated for the State Seismic Safety Commission.

Aspaturian: This was around 2000, I believe?

Jones: This was 2001, 2002. I actually went on the commission in 2002. So I didn't actually apply, even though that's usually what happens for these political appointments.

Aspaturian: Gray Davis was still governor.

Jones: I was appointed by Gray Davis, yes. That was very much an eye-opening experience.

Aspaturian: Do you want to talk about that?

Jones: The commission at the time had seventeen commissioners. And they were designated for certain fields. There's one seismologist, one geologist, one geo-technical engineer, one structural engineer, one building official, and also officials from county government, utilities, emergency services. Those are the sort of categories. Also a member of the [California state] senate and a member of the assembly were both members, but they usually delegated their positions to somebody else. So I was the seismologist.

In the beginning, when I first got on, the chair was Bruce Clark, who is a geologist; he was CEO of Layton & Associates, and active in Democratic politics. His wife's a lawyer. Bruce is a wonderful guy; he helped me get my feet going here and understand more of the political side of science. I wrote half a dozen of the commission reports, which turned out to be most of the writing I did while I was scientist-in-charge. These were committee reports, but I usually ended up doing all the writing because I was used to it. One was on seismic safety in California schools, another on tsunami warnings and the tsunami system in California. I also wrote the seismic safety research plan for the state of California.

Aspaturian: Was this your first time working with state policymakers on this long-term basis?

Jones: Oh yeah. There were several things that shifted in me because of this. Part of it was just recognizing that even on a commission of professionals who are deciding state seismic safety policy, Bruce and I were the only ones who knew that earthquakes didn't happen at epicenters and who realized that they happened over surfaces and faults. Or who knew that the magnitude of an earthquake was determined by the area of the fault. So there were these discussions:

"How do you make your decisions without some of those really basic pieces of information?"

"Well, it's just random, you know; we don't know what's going to come."

"Well, yes, we do know it's going to come—not when, but we know what."

So that was a shock, but it was also seeing how making policy plays out and how decisions were made.

ASPATURIAN: Can you give a couple of concrete examples of how this played out?

JONES: Well, one thing was recognizing that when the San Andreas breaks, everything crossing the fault has to break too. That having a magnitude 7.5 on the San Andreas means I-15 will no longer be straight. That's a really basic thing, and it was not known to most of the commissioners or to just about anyone else in state government; it was clearly not being used as we got talking about policy. [*See also Session Eight*]

Aspaturian: I'm stunned. They didn't, for example, have research scientists on staff who looked into this sort of thing?

Jones: They had a staff with structural engineers.

Aspaturian: Different. It's different.

Uncovering flaws in the Field Act to strengthen earthquake safety in CA public schools

Jones: Right. And because it was seismic safety, one of the things I did was quite a bit about the Field Act because the Field Act was under attack.

Aspaturian: The Field Act came out of the [1933] Long Beach quake, is that correct? Had to do with schools?

Jones: Right. The Field Act passed exactly one month after the Long Beach earthquake, and the intent was that schools have to be better—less likely to fall down in an earthquake—than other buildings because you don't have a choice about putting your kid in a public school. And to accomplish this, they had structural engineers established in the Division of the State Architect to review the plans and approve them, and to provide inspection as the schools are built. One part of the Field Act is continuous inspection. Its implementation has evolved over time, given that seismic standards have changed. At this point, it isn't asking for any stronger buildings—it's not like the standards are higher, per se. It's more that during construction it's reviewed by an engineer saying, for instance, "You've got a flaw here." And they're pretty conservative. You have to not take risks.

But the structural engineer on the commission, Dan [Daniel] Shapiro, was the one who got me to understand that the real power of the Field Act is that it provides for continuous inspection. So when you build a public school in California, there is an inspector on site all through its construction. Who says, for example, "The plans say you're supposed to dig four feet; you've only dug three; keep on going." Or to monitor how many nails went into the school's foundation. And that's something that an inspector coming in once a month isn't going to see.

And actually, in the 2003 San Simeon earthquake, some relatively new houses collapsed. Afterward, the inspection showed that there weren't enough nails in the foundation. Basically a contractor with a bad back didn't want to bend over that much, and by the time the inspector was on site, the nails were covered up and couldn't be seen. That's the type of thing that can't happen in a Field Act building because there's an inspector there all the time. And that was Dan's big point. He and I and two other people on the commission ended up being put on this committee on seismic safety in California schools, and I wrote our report.

One of the things we did was go and talk with building departments on how they conducted inspections and dealt with schools, because there's also the private school seismic safety act of 1986. It says that the intent of the California legislature is that private schools should have the same level of safety as public schools—but it did not set up independent inspection. So they're still being inspected by local building departments, and one of the things we did was talk to a half a dozen building departments, asking them, "Do you know you have this responsibility? Did you know about the 1986 act?" And they don't because it was in the state's educational code, not its building code.

Aspaturian: The message had never gotten through.

Jones: Right. They didn't know. And so this law is there, with no way of enforcing it. It's not like it accomplished anything.

The other irregularity in the way California does this is that the Field Act only applies to K-12 schools. Now the community college system developed after the act, but it evolved out of the trade schools that were connected to the high schools. So the community colleges follow the Field Act; UC [University of California system] and CSU [California State University system] do not. And more than that, Los Angeles can't tell California what to do. So UCLA, even though it's in the city of LA, does not fall under the jurisdiction of the Los Angeles building department. It falls under the state's. But California doesn't have a building department. It has the Division of the State Architect, which by the Field Act oversees public schools, but it doesn't have authority over UC.

Aspaturian: You discovered all of this when you joined—

Jones: Yup, when I joined the commission. It ends up that UC and CSU do their own inspection, and the project manager tasked with bringing the project in under budget has to decide how much inspection gets done And guess what—you got a lot of problems! By contrast, the community colleges have to go through the Field Act. And they're really upset that they have to spend more money and go through more hassle—it takes more time to get plans approved and construction completed when governed by the Field Act. So there have been repeated legislative attempts to say community colleges should be treated like UC and CSU while glossing over the fact that doing that would really mean weakening their construction standards.

One of the things I did was go and talk with legislators and just tell them these facts about how construction of schools happens, and we got that bill that tried to remove the community colleges from the Field Act defeated. So realizing how much seismic safety was affected by the type of knowledge at all levels of government was eye-opening. Writing these reports and being able to hand the correct scientific information to legislative staffs and tell them, "This is what you're dealing with"—that felt like it made a difference.

Dealing with “culture shock” as a scientist in the political arena

Aspaturian: Coming from a scientific mindset, did you have kind of a culture shock, dealing with these politicians?

Jones: Oh, absolutely. There was one member of the commission who basically tried to threaten me into giving him my vote. I don’t mention names, but he wanted to be commission chair, and nobody wanted him, and he tried to threaten me—I can’t even remember what it was he thought he would do. That sort of thing was definitely a culture shock. The reality is that USGS has politics, we have arguments; and when you look at how we try to get information, mostly our experiments aren’t that simple. We can interpret them in multiple ways and have arguments about the interpretation and have one side that thinks this, and the other side saying, “I don’t believe that; I’m going to do this experiment that shows that it’s not what you say.” And we’ve got some process that goes on for several years while we figure it out. But the fundamental difference in science is that in the end, data trumps everything.

Aspaturian: Ideally, it should.

Jones: It should. I mean there are some rare cases— My thesis advisor told me a story about a 1968 AGU meeting that was a really big deal for the acceptance of plate tectonics. There was this data that had been collected across a mid-ocean ridge that was just so clean you could absolutely see the symmetry across the ridge and that supported the plate tectonics model for creation of new sea floor. It's magnetic data, so you've got your magnetic orientations that flip back and forth, and this data set showed that the pattern was a completely symmetric distribution across the mid-ocean ridge that could only be there because the seafloor was being created at the mid-ocean ridge.

Aspaturian: It left no doubt.

Jones: It left no doubt. The data was from a particular ship track —recorded when a ship crossed the ocean, pulling a magnetometer behind it. Everyone was looking at the data when a Harvard professor who'd been one of the famous opponents of the idea of plate tectonics came into the room, and everybody goes quiet to see what he's going to say. He looks at it and finally goes up to the author and says, "Congratulations," leaves the room, leaves the meeting, won't talk to anybody. It wasn't that he wasn't human and really upset about it, but *the data won*.

Aspaturian: It was unanswerable.

Jones: There isn't the equivalent in politics. There are no data wins. For me, recognizing that seismic safety was being driven by that was upsetting. But the other part was realizing that I was popular enough at this point that I could sway a discussion: I could go in and talk to a legislator and explain why certain things are really true.

Aspaturian: They know who you are and what you represent.

Jones: And I could change the direction of the decision-making. I did think, "This feels wrong—that shouldn't be how the decision gets made," but I also, you know, am asking myself, "Don't I have an obligation to do this if I can see that it should be done?" There was enough going on with the commission that it seemed important to do this to the degree I had any time that wasn't part of my management role here. So that tended to be what was taking up my time rather than any particular earthquake. As I said, I had a couple of papers that sat there and languished and didn't get finished.

I also got sent in the summer of 2001 to the Federal Executive Institute for a month of executive training, which was another really useful, eye-opening experience about how to be a manager and how personality plays into it. Fascinating use of Myers-Briggs and understanding how it affects how you interact with people.

Aspaturian: Myers-Briggs is interesting.

Jones: Part of the training focused on the differences between introverted and extroverted managers. A good, introverted manager has the open-door policy of "You can always come talk to me." A good, extroverted manager goes out to the people they supervise; goes into their offices and visits. And there I was, realizing that there was someone I was supervising, a very good guy, and we were both such extreme introverts that we were sitting in our offices waiting for the other one to come by. That was just one little practical thing, but it was illuminating Another was meeting other people in senior executive service in the government. And this was August of 2001, so some of my classmates were in the Pentagon on September 11, a month later. The one I'm thinking of happened to be in a meeting when his office was destroyed by the plane, so he wasn't in it.

I think that was part of what was really good about the executive training— understanding how the civil service operates within the executive branch of government and what role politics has and doesn't have in being a government scientist, and I found it really useful. It also helped me see the relationship between management problems and the way we structured things. I got the operation reorganized. I actually had a proposal for the reorganization of the whole Earthquake Science Center [which included Menlo Park, Pasadena, and Seattle], which didn't get accomplished, but I got my part changed. So it was a very different experience than being a scientist.

"A real shift": Establishing TriNet & the Advanced National Seismic System (ANSS)

Aspaturian: I believe you also set up or you oversaw the establishment of the California Integrated Seismic Network [CISN] during this time.

Jones: Well, I was part of it. That was in 2000. So TriNet had—

Aspaturian: For the record, do you want to say what TriNet is.

Jones: TriNet was a grant that we got from FEMA out of the Northridge earthquake mitigation money. So it came actually through the state of California. It was some pretty large sum—$30 or $40 million over five years; or maybe it wasn't that large—maybe it was about $5 million a year for five years, something like this— that allowed us to go from having seven broadband stations at the time of Northridge to 150 by 2000. But the funding was only for Southern California because at that time, the Northridge money was restricted to the counties affected by the Northridge earthquake. We—the USGS—did TriNet in partnership with the California Geological Survey and Caltech. That was the Tri—the three different organizations. We created the first broadband regional seismic network and defined a lot of it.

During that time, there was also a big push to try and get an advanced national seismic system created. I mentioned that I'd been on another NRC committee on seismic networks back in 1988: The end result of that was to create ANSS—the Advanced National Seismic System. That had been with John Filson, who was then head of the earthquake program. It was his big thing, and he really pushed it to get it through. And then we created the model for an advanced national system with TriNet, and the USGS is like, "Oh, yeah, I guess that is what we're going to have to do," especially after the data TriNet was able to get from Hector Mine.

Explaining the seismic data depicted on the TriNet ShakeMap of the Hector Mine earthquake. *Caltech photo by Robert Paz*

So, looking back, the NRC report comes out in 1990; the first funding for TriNet came in 1997; and then the first official actions for ANSS came in 2000. By then we had already installed 150 broadband stations in southern California. The original model for ANSS was based on the idea of the short-period regional networks sharing data and software in a coordinated way. By the time the statement about ANSS came out in 2000, it specifically called for merging the capabilities of the short-period and strong-motion networks.

The challenge in creating ANSS was, How do you deal with the fact that there are already existing regional networks all over the country and turn those into an advanced system? You don't just ignore them and build a different one. That's what Menlo Park did with Berkeley. But this time they wanted to do it with the existing networks and work it out so that each network would be a coalition of regions, as well as having some function for the national system. All of that had to be negotiated, so the question was, How do we design the regions and put together these coalitions of regions? Is it each individual seismic network? What about Northern California and Southern California; they're big, right? Are they each a region, or could they be combined into a single region? What about the Boston network or the one that's out of New York, and how much do they overlap? Or the Idaho network? There were all these little networks around the country.

So we went to this meeting called by the USGS, and it was Tom Heaton, representing Caltech and me representing USGS in Southern California. We talked with Dave Oppenheimer, who ran the Northern California USGS network, and we proposed that we be one network for California. The state didn't want to have multiple organizations to deal with. I can still remember, the USGS leadership asking, "You guys really want to agree to this; you can do this?" The three of us were like, "Yep, we're saying we're merging into one region." And Berkeley came in too—I think Barbara Romanowicz was the head of the Berkeley Seismographic Station at the time. Nationally, the impact was, If Northern California and Southern California—the two biggest networks—can agree to work together, none of the other networks had a leg to stand on in insisting that they should be on their own. Eventually it ended up with seven regions in the Advanced National Seismic Network.

But we then had to negotiate exactly how we were going to do this. I spent a chunk of time working with Barbara Romanowicz at Berkeley; and I think by this time it would have been Jeroen Tromp for Caltech, Mary Lou Zoback for Menlo Park, and Jim [James] Davis, who was the state geologist, for the state. They were the "leadership team"— the heads of the organizations that ran the networks. And we worked out that there would be a chair and a vice chair and that it would rotate back and forth from north to south.

Aspaturian: How long was this period of negotiation?

Jones: Months.

Aspaturian: So it took a good while.

Jones: It wasn't years. I mostly remember it because San Diego wanted in. They have a regional network—UC San Diego—and we had to tell them, "No, this isn't about every player making sure they get their pot of money. This is about how you do this for California." But I remember the head of the Scripps Oceanographic Institute seeing our CISN Memorandum of Understanding and saying, "This reads like a bunch of people who don't trust each other." I said, "Bingo. If we're going to get this to work, it has to all be laid out. You're right. Nobody trusts each other. We've spent 70 years managing the [north-south] boundary. Now we're talking about getting rid of it. You know."

This agreement included two people from each of the five institutions I mentioned—the five heads of the offices or departments—but the real work was done by the five people actually running the networks: Egill at Caltech, Doug Given for the USGS at that point, and Linda Gee for Berkeley, Dave Oppenheimer for the USGS Menlo Park, and Tony Shakal for the California Geological Survey. We called this group the PMG—Program Management Group.

Aspaturian: Doug Given at northern USGS?

Jones: No, here. I was the head of the office; he was the head of the network.

Aspaturian: I see. Slightly different.

Jones: As I said, we had ten people involved, two from each of five organizations—Caltech, USGS Pasadena, USGS Menlo Park, Berkeley and the state of California—and the program management group (PMG) was set up to make the arrangement work after all this had been negotiated. And this was a challenge when for years there had been an "us vs. them" mentality. For instance, there had been a time when Tom Heaton discovered that Menlo Park had managed to put a whole bunch of Northern California telemetry charges onto the Southern California project. We had hired somebody to be our local administrative assistant, and she dug into the numbers and found it. So, no, in the beginning we didn't trust each other. [Laughter]

It was a process. I actually remember there was a point when the PMG was having a meeting in Southern California. My husband, Egill, was, I think, its chair at that point, and we invited everybody over for dinner; I was there as his wife, not as the head of the office And it was in our dining room that I realized that through this group, "us" had become CISN. "Them" was now the national program. It was probably two years after we had started the process that I had this light come on—the realization that "this is a real shift." It took a lot of time. But what it meant was that we were in a position to do statewide early warning.

Aspaturian: I was going to ask what some of the advantages of this new coupling were.

Jones: The state started giving us money. One of the odd things before early warning and CISN was that all funding for the seismic network came as research support. You know, the federal government is used to funding scientific research.

Aspaturian: Yes, of course.

Jones: But a large part of what the networks do is supporting emergency management. And emergency management had never given us any funding until we got that money from FEMA to establish TriNet. And that process helped shift the funding policy. And of course the early warning money isn't seen as funding research; it's seen as support for emergency management functions. But since it's not research, that raised the question, "Why are we still running a network at Caltech?" We had debates about whether we should we be doing this, whether we should we create a separate nonprofit that runs the network: USGS funds it and Caltech gets the data, but we work for the nonprofit. We never did it, but there would have been advantages.

The whole thing of having this system within the government — it's not straightforward how you do this, and in the beginning with these seismic networks, you ran them in an academic setting because that's the only way you got access to the data. Now, the data's all out through the web. Everybody gets it. The network people are not the first ones to get it because they're too busy making sure that the data's working, so somebody else is in there grabbing it out. I don't know if in the long run it'll stay operational within Caltech, but not everybody who's in a position to determine that thinks, "What would actually work here and work best? So, okay; I changed some things as scientist-in-charge because I sat back and was able to see some of the problems. But—

Aspaturian: It's hard to be strategic when you're dealing with tactical issues on a daily basis.

Jones: That's right. And scientists—research scientists—tend to not be strategic thinkers. Those are independent skill sets, right? You don't need that strategic picture—well, to run a big lab and bring in lots of money, you do, but many researchers don't do that. They're perfectly good scientists without doing that.

How fellow scientists viewed career shift into public policy

Aspaturian: That brings up an interesting question for me. Was there a sense among any of your colleagues in—probably not seismology—but in the geology or geophysics community that you were in some sense selling out, that you had abandoned a thriving research career to move into a—

Jones: Ah.

Aspaturian: You were embedded in policy making by this point.

Jones: I'd actually say I saw more of that among the seismologists.

Aspaturian: Really.

Jones: Yeah, because from a little distance you can see—"This is helpful, this is useful, this is positioning, this is getting us funding, this is creating new programs." The people who might be writing the papers I didn't write would view me more critically. Because by doing that, I was saying, "I don't care about traditional scientific achievement." At that point in my career, I'm 50 years old; I've got the equivalent of tenure in the government; I've had my GS-15 for a while; I'm never going to get promoted higher. So now, I should be trying to become a fellow at AGU, I should be trying for these scientific accolades —that's sort of what you have left at that point. And instead, I'm saying, "I'm not going to bother publishing more papers, I'm going to be doing this other work," and it's sort of like the way some people are threatened by seeing us with masks because we're undermining their world view that the Covid-19 pandemic isn't that bad.

In a sense by saying "I'm not going to bother with writing more papers," I'm undermining the community's world view that the scientific achievement path is the only thing to value. So I think the ones closer to me were more likely to feel that I was lessening myself by doing this.

Aspaturian: Sounds like it did not bother you.

Jones: I noticed it. You know.

Establishing California's Urban Earth Initiative & FOQUS LA

It actually came to a head in 2005 when I got asked to be on a national USGS committee to develop a hazards research proposal to get more funding. The USGS had a new director under the Bush administration, and each year, there'd be like 35 proposals for half a million dollars here or a million dollars there. They were proposals for little amounts; they didn't seem that important; they tended to not get through in tight budget times. And especially post 9/11, the government was putting a lot of money in other things—

Aspaturian: Its priorities were elsewhere.

Jones: Priorities had shifted. And so the idea was that this committee was going to write one big proposal that would cover a lot of things. Water availability or hazards were sort of the finalists for the overall topic. And in 2003, 2004, there had been something like four hurricanes and the San Simeon earthquake and the mood was like, "Okay, somebody's trying to tell us something—we're going to do it on hazards." It was a weird process where it took four months to choose the committee members and then the committee had four weeks to do its job and come up with this big overarching framework. We were told, "Everybody write your own little corner because we don't have time to create a really integrated proposal." Okay.

At this point, the USGS had designated state representatives for this, and in every state this was the head of the Water Science Center, because that was the only function in the USGS that was in every state. For California, this was Mike Shulters. He and I had been looking for ways to collaborate between Earthquakes and Water and meeting to discuss various topics. One of them was realizing how many of our water monitoring stations were close to seismic stations. We had actually at one point talked about trying to co-locate them as we were putting in our new CISN stations. Turns out that's almost impossible. Their needs and ours were basically incompatible. Water monitoring stations are going to be in a stream. Streams make lots of noise. GPS might work, but GPS needs sky view, and stream gauges tend to be down in canyons. We did try.

Aspaturian: A lot of diametric priorities in other words.

Jones: And then we just realized we weren't the only USGS scientists working in southern California with potentially overlapping interests, and so we talked about trying to create an initiative where we could describe what the USGS did in Southern California as the impact of the earth on the city and of the city on the earth. "We're doing water contamination; we're doing earthquakes; we're doing endangered species. All of them fall within this category, and we should at least know each other. We don't have any funding or anything, but we'll call it the Urban Earth Initiative," and we invited all the USGS scientists working in Southern California to get together for a meeting and just share what they were doing. We spent a day just hearing what other people were doing. It was fascinating, really cool research in all these different areas.

Aspaturian: Are we still in 2005 here?

Jones: This is actually 2003. I'm sorry; I jumped back. But that is why I got asked to be on the natural hazards committee two years later. So we had already gotten together and then the 2003 wildfires affected everybody: They're going to cause debris flows, they're going to be contaminating the water, they're affecting the local wildlife— and so we put in a joint group funding proposal to USGS. It ended up not getting through because everybody was still working within their own areas, and USGS didn't know how to handle the cross-discipline stuff. We got a little bit of funding, but it was given to the individual programs.

The situation was sort of like the academic divisions at Caltech. The biologists and the water people in USGS occasionally work together because they're both dealing with fire-related issues, but not that much. So that had been going on, and I already had all these contacts, and Mike and I had been exploring ways we could work together—not all that successfully, but we were trying.

We did get a project funded, that's true: FOQUS LA. That was actually really good. "Framework of Quaternary Stratigraphy for Los Angeles Basin." It involved earthquake people because the earthquake faults were creating the folds forming the stratigraphy and by looking at the stratigraphy we could understand the underlying faults; energy people, because stratigraphy describes the folds where all the oil pools, and in the LA Basin that was still a big one; the water people because their biggest source of funding came from the South Coast Water District, which was having a huge problem with sea water intrusion.

They had begun to realize that the model of the subsurface stratigraphy that they were using to decide where to inject fresh water to block the seawater from contaminating the groundwater clearly wasn't right because it wasn't working. And so they funded us, which enabled us to get cores—this was drilling into the ground, a kilometer or more deep, and preserving the core so we could see the layers of soil and rock.

There were other groups involved as well, and it was an earthquake guy, stationed in Northern California but who reported to me, who led it—a guy named Dan Ponti. That was the most significant joint project that we did together.

Launching USGS Multi-Hazards Pilot Project: rationale; funding & bureaucratic issues; impact of Hurricane Katrina

So I was already supervising this interdisciplinary work when they asked me to be on this hazards committee in 2005. And there as I said, we had four weeks to get the whole thing done and not enough time to do it right. So, the USGS leadership said, "Hey, you and Mike have been working together; give us an interdisciplinary proposal for a pilot project of multi-hazards in Southern California." I said, "Okay, what's the goal of it?"

"We want it to be interdisciplinary."

"That's not a goal, that's a process. What's the goal?"

"To be interdisciplinary."

"How about a goal to demonstrate how hazard science can improve a community's resilience to natural disasters?"

"Fine, go for it. It's interdisciplinary, right?"

So Mike and I literally sat in my dining room one day and wrote this proposal.

Aspaturian: Again, was this for the USGS?

Jones: For the USGS. This is all internal at the USGS. And it was to be part of this big effort, where we said we needed $300 million dollars to do hazards monitoring right. And the response from above the USGS leadership was basically, "Really, guys. Can you be a little more realistic?" So the USGS leadership took my pilot project for southern California and wrote up a proposal for $5 million, and we wrote it up with this goal of how we were going to work with the community.

Aspaturian: Was it hard for the USGS to get its head around this broadened vision of what it should be supporting?

Jones: Yes, and it's still an issue. But now Dave Applegate, who was my advocate and sponsor at headquarters, has just been named to be director. Twenty years later, I think we're finally getting there. He was head of the national earthquake program and then in 2012, the USGS reorganized and created associate directors for mission areas, and he became the associate director for natural hazards, and my project moved into there, and I then reported to him instead of the western regional director.

So the USGS wrote this proposal to the Department of Interior, basically saying "we want to improve our hazards research, and we want to begin with this pilot project," with the idea that usually when you have a big initiative in the government, it takes years before it gets through Congress. But because it's a pilot of a bigger effort, we could describe the whole program in what's called in-target budget language. So that's why USGS did it this way: No intention at all of it getting funded. But it went to Interior, and when it came back they had cut the $5 million down to only a million and a half dollars. The rest of it was to be redirected funds, and there also was this half a million dollars added on to be sent out to FEMA for some mapping project. There was some politics going on with that.

Aspaturian: Naturally.

Jones: We were like—whatever. So this goes to the Office of Management and Budget from Department of Interior. One week later, Hurricane Katrina hits. So as the Bush Administration is dealing with a rather poor showing in Katrina—

Aspaturian: I remember it well.

Jones: —And figuring out how to look serious about hazards, here's this proposal to use science to improve the community's resilience to natural disasters. *And we got in the president's budget* *the first year*. Nobody could believe it; there had been no idea that this would happen, and now here it is in the budget. We're like, "Okay now!"

So this is now 2006, and the president's budget goes to Congress in February. At this point, I have been scientist in charge for seven and a half years, and the USGS senior management leading this hazards effort are saying, "If this gets funded, you gotta run it."

"Yeah, but—how's that going to get through Congress?" "We're going to try to do it anyway." So we proposed a reorganizing of this project where I would report to the USGS western regional geologist, a really nice guy based in Arizona named Wes [Wesley] Ward, and so separate off from the earthquake program, go a step up, and somebody else would then take over as scientist-in-charge in Pasadena.

Aspaturian: Who succeeded you?

Jones: Sue [Susan E.] Hough. She hadn't shown it until she became scientist in charge, but she really didn't like me. Working with her was a continuing challenge as I went through doing it. But we don't need the details.

Aspaturian: I'd like to talk more about your multi-hazards work and your work with the city of Los Angeles next time, but I have a few other questions.

Jones: Okay, sure.

Growth of women in seismology & own impact as role model

Aspaturian: One of the things that occurred to me listening to you and thinking about it in preparation for this interview, is that the number of women in seismology really seems to have grown quite a bit between the early '80s and the late 1990s. I mean, half the people you mention in this description of your move into public policy were female. Do you think you served as a role model in some cases for this?

Jones: I do, and I do know that when we hired two people in that job search I described,

both of them were women and one of them specifically told me she'd grown up in Claremont and seen me on TV when she was a child, and that's why she went into seismology.

So, yeah, that was a factor. The other thing is to remember that I was on the cusp. Female students in my year in high school would have been actively discouraged out of math, and then you don't have any path in science open to you. Girls born a few years later were in middle school when the women's movement got started, and then they weren't at least actively talked out of it. And so only two years after I entered MIT, the incoming class in geophysics was one-third women, whereas I'd been the only one in my class. Well, there was another woman, who went over to Woods Hole.

And then you've got all the other changes that need to happen to make this transition successful. I would give more of the credit to the changes in the field and society, but I'm trying to recall who the second woman was to come into Pasadena USGS. That would have been Donna Eberhart-Phillips, but she came in for just two years. Over time, we had a lot more women here than USGS did in Northern California at the time. Mary Lou Zoback was up there, and she was my boss and, as I said, she was my inspiration for going to work part–time and all that. Our office, of course, is a lot smaller, but Los Angeles is really a lot bigger.

And most of us women along the way were married to other PhDs—I think I was the only one married to a seismologist. Donna came down here because her husband was an epidemiologist and got a CDC appointment at LA County. Sue Hough's husband is a biology researcher at UCLA. Morgan Page's husband is a professor here at Caltech, a physicist. So there were quite a few of us who came here because our husbands had jobs locally— there were more high-level academic positions available around here than in the Bay Area. Especially in the '90s— Donna was here '91 to '93; Sue got hired in '92; Morgan Page and Karen Felzer got hired in 2000. By then I think the gender ratio's getting a little more even.

Aspaturian: Clearly the numbers have changed a lot since you began your career here.

Jones: I think it's reflecting the larger society.

Caltech earthquake engineers & seismologists: collaborations & challenges

Aspaturian: What was your relationship with the earthquake engineers at Caltech like? You mentioned Paul Jennings, you mentioned Bill Iwan.

Jones: It was always very supportive and collegial. The department was always really nice with us. I never wrote any papers with any of them— the type of work we did didn't overlap.

Aspaturian: Did they ever come to you for data or insights?

Jones: George Housner [Braun Professor of Engineering, Emeritus; d. 2008] came to me for some Chinese translating when I first got here. [Laughter]

Aspaturian: I believe it.

Jones: He had some group that was visiting from China, and he had me go to lunch with them and be a translator. Historically, seismologists and earthquake engineers have no overlap.

Aspaturian: That surprises me.

Jones: They study different things. Engineers are studying buildings, the geologists are studying rocks, and they're in different divisions. That's a pretty fundamental thing here, right? Everywhere. And there are problems because of it. The engineers need ground motions, and they can model them in horribly empirical ways that don't take into account what the ground motions are going to be. That's what seismology is, figuring that out. There were definitely frustrations that there wasn't more overlap. I think—well, I know—that here at Caltech there has been a better connection since Tom Heaton was hired jointly in seismology and earthquake engineering [1995], and he is the first person I know of in the United States to have that joint position. Even though it should be more common.

It was sort of traditional that seismologists gave information to engineers, and engineers worked with the public. Paul Jennings always was really supportive of us being there in the civil engineering department, and of course when he became provost, he then really worked to help us. So I know everybody, but I never wrote a paper with anybody—I did write some reports with Bill Iwan. But what I discovered later, as I got more involved both with the Seismic Safety Commission and then ShakeOut and then Resilience by Design [*Session Nine*] is that much of the earthquake engineering community doesn't really trust the Caltech earthquake engineers. They think that they're too theoretical; they don't want to know what it actually means to build a building. And partly that's because Tom has strong ideas; he's very outspoken about them. Instead of saying, "Have you ever thought about reevaluating X?" it would be, "You're just wrong, you're ignoring a lot." He had a way of putting people's backs up. Even though I am certain that his ideas are correct and important.

So there's more of a divide than there should be. And while Caltech is great about not

doing that, their graduates often don't go out to get jobs building and designing buildings. They get jobs teaching and doing more research. That's a pretty significant divide.

"Confessions from a Magnitude-Weary Seismologist": the backstory

ASPATURIAN: You wrote an article in 2000 whose title caught my eye, "Confessions from a Magnitude-Weary Seismologist." I did not read the article. I thought I'd ask you just to talk about it.

JONES: Essentially, we should never be using magnitude to describe—

ASPATURIAN: That's what we all relate to!

JONES: No, but you *don't*. You think you do. It's what everybody *knows*, and they get it wrong. Who understands a factor of 32? The energy in an earthquake goes up by a factor of 10 to the 1.5 power for each unit of magnitude. Try to explain that to the public. Moment is a much more accurate measure of the earthquake size, but we convert it back to magnitude because we think that that's what people understand. That and trying to explain 1.2 times 10 to the 26 dyne-centimeters at a press conference—having tried to do this—is not easy. It doesn't get across. One of our proposals over time has been trying to establish a unit for moment. Especially now with computers where people are comfortable with knowing about kilobytes and megabytes and gigabytes and terabytes.

We actually proposed Akis because Keiiti Aki first proposed seismic moments. [*Session Six*] And so a magnitude 5 would be one Aki, and a magnitude 3 would be one milliAki, and magnitude 1 would be a microAki. The biggest earthquakes we know are going to be gigaAkis. And then you'd start getting a feeling of how big the range actually is. Magnitude is usually gotten wrong; it's not understood and then it's assumed to be describing what you feel. "Oh, this feels like a magnitude 3"—well, it depends on where you are.

ASPATURIAN: That's right.

JONES: I've had magnitude 6.5s that I barely felt at all. So magnitude's not what you feel. Intensity is what you feel. Richter developed magnitude because people who perceived different intensities thought they were feeling different earthquakes, and he was trying to express the idea that you may have felt different things but there's one size to the event. But now we've gotten to the point where magnitude is thought to be everything and it's back to thinking it's what we feel. But changing that thinking is awfully hard.

ASPATURIAN: Well, we can't even get this country to go from Fahrenheit to Centigrade.

Jones: Well, right. When you can't use the metric system, I don't think we're ever going to be using other units.

Striking a meaningful balance between science & public policy

Aspaturian: There's another comment you made that caught my eye. Let me see— "Scientists' egos can sometimes get in the way of actually getting the science used." Is this something you've experienced in the course of your career?

Jones: Well sure. When did I say that?

Aspaturian: I don't know. The attribution is in my notes at home. It may have been in the AGU interview you gave a couple of years ago.

Jones: Oh, maybe. Okay. I think the fundamental idea was— when I made that decision to go ahead and lead the multi-hazards project, we didn't know if it would be funded. So there I was, asking myself: Do I take it on? I actually was talking with a very wise person and trying to talk through what it was that I felt about it. I was 50 years old at the time. If I go back to research after my management stint, it will take me a little to get going, and I've got fifteen years before I'm 65, so maybe I'll write 30 papers. And if I'm honest with myself, five of them will be read and two of them will matter. Right? There's a lot of those research papers; you accumulate this long list—

Aspaturian: In every field.

Jones: And if I didn't do that, I could see those bright young rising stars who were going to write the papers that would matter, and they'd probably do a better job of it than I would. New computer tools and statistical analyses were now available to them, so I'd really have a learning curve to get back up to after seven years of not actively doing research. And if I didn't do multi-hazards, it wasn't going to get done. And so I saw it as making an explicit decision to walk away—I wasn't going to try to become a fellow of AGU; I wasn't going to write a bunch more publications. I was going to give that up, get the science used, make some good come out of this in the end.

And the funny part of it is that after five years of multi-hazards work, I ended up being author or co-author of 40 publications. Because it turned out there was a lot that needed to be done when you got in there. And because of my work with Los Angeles, I was given the AGU Ambassador Award, which includes being an AGU fellow. Essentially I got all those things that I said I was walking away from because these other goals were more important. So by recognizing and saying, "I don't care about my ego," I both got more done and still got those awards, and it was a big surprise to me that it came out that way.

Aspaturian: You may have already partly answered this question, but looking back, do you ever regret your shift away from science into the realm of public policy?

Jones: Not regret it; I miss it sometimes. But that's actually one of the advantages of my marital situation: I have an active researcher at home! I often give him the first review of his paper, and there are papers where I've helped him out and I'll be the second or third author. So I get to dabble in the research without giving it up completely.

Aspaturian: The best of both worlds in a sense.

Jones: Yeah, and I feel like I've gotten more done in terms of good in society by doing this, and it also provides an example that you can go different ways. One of our sons became a high school chemistry and physics teacher for a couple of years before he realized he wanted to be a researcher and went back to graduate school. But I think it was when he was student-teaching, his master teacher asked him. "So what does your father do?" "He's a seismologist at Caltech." "Oh, does he know that Lucy Jones?" My son said, "Yeah, you could say that." So she was very excited that he was my son and wanted him to talk about us, and he ended up doing it as part of ShakeOut. It was great—how really different outcomes can come from the same degree. Egill and I started out by getting the same degree five days apart. We wrote all those papers at the beginning, many together, and we've each contributed to society but in very different ways. I was rather impressed that my son was able to turn this into a positive.

Personal reflections on quake-centric public recognition

Aspaturian: How do you feel about the fact that your name has come to be indelibly associated with Southern California earthquakes? Or perhaps with earthquakes period?

Jones: It just feels weird. I mean it isn't real—not to me, if that makes sense. It does help me see the superficial nature of most public recognition. I've had people introduce me as the most important seismologist in the world. It's like, No, I'm not. I'm far from it.

It's sort of more the experience of seeing other public figures and wondering who's the human person behind the image. My public image is not what I came out to do. I would gladly give up all of that public recognition where people recognize me in the grocery store, because I feel really awkward when people come up to me, and I have to face up to them. I remember Kate Hutton at one point talking about being in some place in Northridge when somebody came up to her, saying, "Aren't you Kate Hutton?" and he looked really dicey. His fly was down or something, and she was like, "No, no: I've just been told I look like her."

Aspaturian: Have you ever felt tempted to give that response as well?

Jones: Oh, yeah. And I will have times when people recognize me but don't recognize me: "Where do I know you from? Are your kids at St. George's?" Then I can just say, "I'm a seismologist," and they'll go "Oh."

Aspaturian: How about the knowledge that you've probably inspired a lot of little girls and young women to think about careers in science? I think that's indisputable.

Jones: That part I'm more proud of. I think it's also recognizing that what we think of as necessary to have a career is only part of it. Yes, there's the technical skills I learned and the ability to write these papers. But it's become really obvious to me that there's also the whole soft skills part, and I wouldn't say that I started out great at the soft skills. I could be pretty clueless about what people around me were thinking. That is why that training stint at the Federal Executive Institute was so important to me. It came at a time of life that allowed me to really incorporate and use it. I've gotten lots of interviews where I'm asked, "You're a woman in science. What have you suffered from? How have people kept you back?"

It never felt like I was really being held back, but I've realized that part of that is because I was pretty clueless about what other people thought and tended to ignore it. I was one of those people to whom it didn't matter. But I can recognize that for most people, or for many people, how others see you and whether or not you see yourself in a community does matter. I would come into a room where there were only men, and think, "Boy I must be really special that I'm making it here," instead of "Oh, I don't belong here." But that's a personality thing, and you shouldn't be driven out because you feel like you don't belong somewhere. That's a pretty common human lot. I'm just saying I don't represent that very well. As I say, I'm rather clueless about what other people are thinking of me.

Aspaturian: It seems to have worked to your advantage to some degree.

Jones: Well, it made me get through challenges at a time when I otherwise wouldn't have and provide inspiration to help be part of changing that picture. Yeah, I'm glad that I've been able to do that.

Session 8, May 18, 2022

Mentoring experiences with students

Aspaturian: We're going to talk today about your work with the multi-hazards demonstration project, which led to ShakeOut and a number of other hazard preparedness initiatives. But first, I noticed that in a number of self-descriptions in your resumé, you talk about supervising graduate students over the years. I wanted to ask about that experience.

Jones: It's actually relatively little. Because I was a government employee, I've never been in the formal role of having graduate students. But I've ended up having interns, and I've had a couple of cases of students wanting to work with me. I was once sort of a co-lead with someone at ETH Zurich, but I was also then on their committee and coauthor on papers. That's probably the closest I've come to working closely with a graduate student. I haven't at Caltech.

Aspaturian: How about undergraduates? Any SURFs?

Jones: A long time ago, [geophysicist] Susanna Gross worked with me. At the beginning of my work with multi-hazards I got SCEC interns. As we were putting together ShakeOut and trying to assemble all of that information, I think I might have had seven interns that one summer. It was really, really useful. There was a lot of data that needed to be assembled.

Aspaturian: I see, so that's what they were doing.

Jones: One of them was studying economics; he was a rising senior at Occidental. He worked with the USGS economist up in Menlo Park, looking at economic impacts of things like business disruptions. One of them was a geology undergrad at a community college. He was quite a bit older than your usual undergrad because at eighteen, he'd started to work for the sheriff's department, and actually worked at LA County Jail. When he decided to become a sheriff, one of the things they do is put them through a process to make sure they can kill someone. You have to be able to shoot your gun, and he couldn't, so he couldn't go on as a sheriff and ended up becoming a geologist. He ended up getting a master's at, I guess, Fullerton. He was just starting to go into geology, and actually one of the issues we were looking at was, If you're taking a comprehensive look at the impact of natural hazards on society, what happens if the jails are damaged? What are the authorities ready to do? So he helped put together some of that. Others were more into traditional geology and working on various pieces of ShakeOut. And because they were a group, they could then support and help each other, which always makes doing internships a lot easier.

So I had a variety of interns and actually when I was in LA City Hall, working with the mayor, we ended up having four interns. One of them was a master's student who had been working on urban flooding in a resilience program. The other three were all undergraduates. One in geology, one in engineering, and one, whose parents were both seismologists, was studying political science. And boy, they really helped with all of that work, too. Oh, and Debbie Weiser was one of the ShakeOut interns. She was a geology undergrad at Occidental and ended up getting her PhD at UCLA. She actually worked for me for part of the time when she was at UCLA, and I was on her thesis committee too.

Aspaturian: Where is she now?

Jones: She's with a private company. She ended up doing a split thesis on the science and policy of geothermally induced earthquakes.

Aspaturian: Sounds like a very interesting topic.

Jones: It was, and it was a topic where you could do two chapters each on the earth science and the policy.

Aspaturian: The topics converged naturally.

Jones: Yes, but finding a thesis advisor willing to do that was tough because, you know, most

straight geology people will say, "That's not what we do." It ended up being Dave [David] Jackson at UCLA,

So, as I say, I've mentored quite a few young people, but it's only twice that it's been a formal graduate student where I'm advising with their committee.

Aspaturian: Do you have a mentoring philosophy?

Jones: I suppose it's supporting the students to find out what it is that they want. Especially when I myself have taken an unusual path.

And yet at the same time, recognizing that you can't just jump to what I did. I'm successful today because I was a successful research scientist. If I had tried to go straight to the policy side, I wouldn't have had any impact. So part of it's just helping them understand that. And actually I have a young woman working for me now who began as a summer intern when she was a geology undergrad at Brown. She's back working with me while she decides what path she's going to take going forward. We've had some talks about it: Do you go for the geology PhD—there's a lot of power that you get out of having that. Is it worth the time and the strain?

Aspaturian: She's also working in the realm of public policy, I imagine.

Jones: She very much wants the public policy. She had a dual undergraduate major of geology and visual arts. So she's begun doing design work for me as well.

I think that's the closest I've come to a mentoring policy: Support them in finding out what it is they want to do.

Multi-Hazards Demonstration Project: making the case & securing federal funding

Aspaturian: So in 2006, you became the chief scientist for the multi-hazards program. Was this part of USGS?

Jones: Yes. And so as we talked at the last session, there had been this hazards initiative— And the multi-hazards demonstration project was the one part that went forward within the in-target budget language, never expecting that it would actually be funded. And it got into the president's budget because of Katrina. [*Session Seven*]

The other aspect of it was a really good legislative liaison. The USGS has staff that is dedicated to working with the legislature, and this woman, Linda Smith, was assigned to help me figure out how to do this. She came out here, and we went and visited every Congressional office in Southern California. We drove from San Diego to Santa Maria, explaining what we were doing.

Aspaturian: Now these are the federal legislators?

Jones: These would be all members of the House of Representatives for Southern California.

Aspaturian: Okay, got it.

Jones: There's like 30, a whole lot of them. I think we visited 27 offices. For some reason, that number sticks in my brain. There's a very fine line here when you work for the government —if *I* were to lobby Congress to fund something, I would go to jail for using federal resources to influence Congress.

Aspaturian: I see.

Jones: However, it is our obligation to keep the legislative branch informed. NASA traditionally finds itself much closer to the lobbying side of things, while USGS tends to find it very hard to do. For instance, you cannot as a USGS employee talk to a member of the Congressional appropriations staff without a member of our budgeting department being with us.

Going and explaining what we are doing is legit.

Aspaturian: But you can't intimate that you could use more support.

Jones: Right. If they ask you a direct question about it, you can answer, but it's a very fine line, and you always have a member of the office of communications with you when you're talking with Congressional representatives. And this was Linda for me, taking me around to all of these representatives. But she also then came up with this idea of inviting the staff of the appropriations committee to come and do a tour in Southern California.

Aspaturian: The House appropriations—

Jones: Actually the Senate. She started with inviting the Senate because there was a woman on staff there whom she knew. We were going to take them to Universal Studios because we had been working with the emergency responders there, as well as with emergency managers in LA, on various things that people had approached me about while I was scientist-in-charge in Pasadena.

Aspaturian: I imagine this is mostly on earthquake preparedness.

Jones: It was all on earthquakes at this point. And there were two women who were the appropriations staff for the majority party in the Senate, which were the Republicans at that point. Somebody once said to me, "There's three types in Congress: Democrats, Republicans, and Appropriators." I think it used to be true; I wonder how much of it is now. But anyway, so we had this set up, and then the majority staff head for House appropriations said, "I want to see this, too." "Okay, sure c'mon."

Then it turned out that the appropriations staffer on the Senate side had to cancel, but the House appropriations head, Chris [Christopher] Topik, was still coming. Remember I said you're not supposed to talk to the appropriators? Well Chris calls me up and says, "Do we have to do this shit with the Universal Studios?" and I said, "You're the only one coming; you tell me what you want to do." So we had a talk on the phone, figuring out what he'd like to do; and afterward I immediately called the USGS budget people to let them know—"I didn't initiate this; *he called me*!"

So we set up a two-day trip. The head of the Western Ecological Research Center, a guy named Steve [Steven] Schwarzbach, came down, and Mike Shulters, the head of the water science center, who I'd been working with, came down too, and we took Chris on a multi-hazards tour in Southern California.

Aspaturian: So, he was staff, not the congressional rep—

Jones: For Appropriations that funds Interior, so it's the Interior subcommittee. [Topik spent 15 years as majority professional staff for both parties on the House Interior and Environment Appropriations subcommittee. *–Ed.*] It turns out he had a PhD in ecology or biology, and he had been with Fish and Wildlife. We went hiking up a canyon in the San Bernardino mountains where there were endangered frogs. A big debris flow in 2004 had wiped out the pools that the frogs lived in because it filled up the pools and destroyed the trees, and the frogs all disappeared; and the next winter there was a really big storm and the pools got reestablished, still without trees. It turned out there were frogs in isolated spaces up in Refugio Canyon that then repopulated down the canyon. So it was like this whole, cool endangered species story, and that was Steve Schwarzbach's area, along with, of course, debris flows and floods. We ended up doing this four-hour hike.

Aspaturian: He was in his element, obviously.

Jones: *And he loved it*. We took him out to see the strain meter that runs along the Glendale Freeway. Then we ended up going out to dinner at a Mexican restaurant—that's what he asked for—and it turned out that they were giving free meals to first responders. They recognized me, and said, "You count!" and we got a free meal. He's watching all of this going on. And then the next day, the head of emergency management for LA gave us a helicopter ride, so the three of us and Chris took two hours flying up over dams that had been damaged in earthquakes and braided streams, where you could see the previous floods and debris flows over the San Andreas and the trenches for figuring out exactly where the San Andreas was, and areas affected by wildfire, and it was fabulous. Two fire chiefs and the head of the fire department came with us.

So we were able to show him that USGS matters to Southern California while also showing him all this cool science. Actually, there was a point when we were flying over a place where a developer had basically cut off the mountaintop and was putting in houses, and you could see the braided streams coming into and out of the cutoff area, and you know there is going to be a debris flow in the future. But his comment was "You know I thought of LA as a done city like New York. You're still growing. Look at how it matters!"

So the next year was the first time ever that Congress did not pass a federal budget, but instead funded the agencies with continuing resolutions through the year. The first time I'm in the president's budget is the first time that it's ever thrown away! So in March of 2007, there was the continuing resolution to complete the 2007 fiscal year. And Chris got the multi-hazards project put into the continuing resolution. So not only did we get funded the first year, we got funded in a year when there wasn't a budget. And there we are, with now half the year gone, and we've got this money.

Aspaturian: How much funding did you get? Do you recall?

Jones: \$2.05 million. It was supposed to be \$1.5 million, and then there was this \$550,000 that had been added on for FEMA. So he gave us the full amount, but because it was done in continuing resolution, the language to send it on to FEMA disappeared. We got this bonus, which wasn't assigned to any discipline. So it was actually able to be used by—

Aspaturian: It was unrestricted.

Jones: Yes, basically unrestricted—and on a really multi-disciplinary project like this, where everything else went to a specific discipline, that \$550,000 was the pot we used to actually get it going. Through all of this, the way it happened in the USGS was that it was supposed to be interdisciplinary, and when we knew we were in the president's budget, which would have been February of 2006, I got moved out of the discipline structure to report to the western regional geologist.

Aspaturian: Because, yes, you were working on a larger canvas at this point, basically.

Steps down as USGS scientist-in-charge to map out Multi Hazards strategic plan

Jones: Right. And so I think that happened in July, and also that summer I ended up with bronchitis, because I'd been doing too much, and it was also a time when the family was challenging. So we made the decision to move me out of scientist-in-charge. Sue Hough took that over; I reported to Wes; we started organizing, and we were actually able to get a little internal funding to do a strategic plan—also something not common in the government. Really useful. And we met with our constituents. We met with emergency managers, with financial interests, with land-use planners. And we asked them, "What do you need from science about disasters you're not getting?"

Aspaturian: What was your portfolio in terms of natural disasters?

Jones: Earthquakes, floods, debris flows and other landslides, tsunamis, wildfires, coastal erosion, and I'm missing one. There were seven. Maybe no, that is it. That was our portfolio, basically—all the natural disasters that affect Southern California. And so all of the different disciplines in the USGS were part of this. This process of bringing in the stakeholders wasn't common in the USGS at that point, for sure, but I got a little funding for it, which I actually used to hire Monica Kohler when she was at UCLA; she's now here in civil engineering [research professor of mechanical and civil engineering].

Aspaturian: Yes, that name's familiar.

Jones: She helped me do the work of actually putting it together. And we met with all of these stakeholders. We had three different listening sessions and were rather dismayed that the really strong message—especially from the emergency managers, but actually all of them—was "we want scenarios. We know we need to get ready for a big earthquake, and we don't know what we're getting ready for." So this is why ShakeOut happened. It was not that I went in there thinking I'd do it. I went in there thinking I'd be doing other things.

ASPATURIAN: So were the emergency managers specifically asking about earthquakes? They weren't interested in floods or wildfires?

JONES: Oh, they were interested in all of them. The reason that we started with an earthquake scenario was because Dave Applegate, who was the head of the earthquake program, was really behind the idea and committed to putting in USGS resources to do it. The earthquake program in the USGS was willing to divert resources and say from the top, This is going to be done. Mike Shulters and Steve Schwarzbach and I were sort of this leadership team.

ASPATURIAN: You were the troika.

JONES: We were the troika. And then I was the one who got moved out to lead it, maybe because I was ready to move, etc., and I was the only one stationed down here. They were both in Sacramento.

ASPATURIAN: Do you think they also picked you because you had such public name recognition?

JONES: It was a factor, and I had to then choose whether to accept it.

Aspaturian: Yes, you talked about that last time; it was a bit of a process.

Jones: So the other thing about starting with earthquakes was that we could include everybody in that scenario. Earthquakes cause dam failures. They trigger landslides. There's a potential for triggered tsunamis.

Aspaturian: Also fire.

Jones: Right. Fire. So the funding went to creating a number of things. We formed working groups for topics like wildfires and debris flows, and we had them work together. And then there was this integrated piece of the scenario, which happened through me.

SoSAFE: Understanding the dynamics of the southern San Andreas

On the earthquake side, there was another piece of money that created SoSAFE—the southern San Andreas fault evaluation project. Most people maybe weren't aware, but at the time—it's a little more resolved now—it was really unclear how the plate boundary motion divided between the southern San Andreas and the San Jacinto fault.

Aspaturian: Where does the San Jacinto run in relation to the San Andreas; are they perpendicular to each other?

JONES: They're parallel. So as the San Andreas comes down out of Northern California and goes along the north side of the San Gabriel Mountains and then comes into Cajon Pass, somewhere just north of Cajon Pass—we've never actually found it—the San Jacinto splits off. So as you get down into the Inland Empire, you end up with the San Andreas to the east. It's on the eastern side of the Coachella Valley, and it runs up through the San Bernardino Mountains—it's almost east-west through the San Bernardino Mountains—and you can't see it clearly at the surface, so in a sense, the San Andreas disappears in the mountains. You can see it going in on both sides; you can't see it in between. Clarence Allen spent a lot of his career trying to put all those pieces together and showing it really doesn't show up there.

And then while that's all going on, to the west, you have the San Jacinto fault. It's like the San Jacinto fault is straightening out the San Andreas. We think in the long run, the eastern branch of the San Andreas will die out, and the San Jacinto will take over as the big plate boundary. The question is, Where are we in that process? When the 1906 earthquake happened, and geologists first mapped the San Andreas, they came down from Northern California and found these two faults, and there was a big debate over which one to give the name "San Andreas." The eastern one that is called the San Andreas was more clearly expressed at the surface – meaning the offsets were clearer. And that's because it has been the main fault boundary for a long time so lots of motion has occurred across it. But it's being twisted by the rotation that's happening through the San Bernardino Mountains, and San Jacinto is forming to cut off that twist—but how far along in the process are we? There were legitimate models that had a factor of two variation on how active the San Jacinto fault is, which implies twice as many earthquakes right? And here we have the biggest fault in the biggest city in the United States at risk from earthquakes, and we don't know something that basic?

Seeing the world through a geologist's eyes; ongoing modeling of the San Andreas fault

Aspaturian: I want to interrupt with a question, having watched you describe this. Are you naturally a strong visualizer, or did you have to learn this process?

Jones: I don't think I'm the strongest visualizer around. I don't know. My son can visualize anything. He's a real 3-D visualizer; I realize I don't have that. Some geologists can just automatically jump to where the three-dimensional picture of the faults are. I had to learn that. That said, I was always really good at puzzles and things like that.

Aspaturian: Can your husband do this? Because you mentioned your son.

Jones: Oh, yeah. I think mostly if you're bad at it, you won't end up in earth science. If you're really, really good at it, you end up as a mapper. I'm some place in between. I do realize: I look at maps, I live with maps, I've been working from maps for 50 years, and you see them in a different way than a lot of people do.

Aspaturian: You probably see nature in a slightly different way than a lot of us do.

Jones: Oh, we all do.

Aspaturian: I've noticed this with geologists.

With sons Sven and Niels in southwestern Iceland, site of many volcanic eruptions, in 1999, and with Egill at New Zealand's Bay of Plenty, 2023. *Photos courtesy of Lucy Jones*

Jones: I just did my last podcast on that topic. *Getting Through It, Episode 100:* "The Magic Eyes of the Geologist".

Aspaturian: Did you? I should listen to it.

Jones: Since it was our 100th episode, we went out to the Sierra Madre fault and did it from the field and just talked about what it is you look at to find a fault by JPL. Anyway.

Aspaturian: San Jacinto, San Andreas.

Jones: And the fact that we don't even know which one is which. So the main thing funded by the direct earthquake program was this SoSAFE project to get more trenches dug out there and finally get the data to figure out—

Aspaturian: Once and for all what the relationship was. Was that ever resolved?

Jones: Once and for all, right. Being science, the question still has ambiguities, but we have much more data now. And that money actually ended up going to SCEC, not the USGS, which had people in the USGS office here in Pasadena angry that they weren't getting the money out of this.

Aspaturian: How is it that it went to SCEC?

Jones: That was Dave Applegate's choice. He really wanted to bring in the rest of the community to support what we were doing.

Aspaturian: He didn't want it siloed.

Jones: So when we did ShakeOut, we went to the SoSAFE team and said, "Tell us what a reasonable model of what the geologic slip will be." So there was a meeting about it of 60 geologists—all of the specialists in Southern California—and they came up with the slip model for us. And we were also able to go to SCEC, which had got funding from NSF basically to do verification of codes—

Aspaturian: Meaning?

Jones: SCEC got NSF money to look at seismic simulations verification. This is, you've got a simulator and a model, and you say "Here's what the ground motions are; what are the chances the model is right? How do you test that? Where are your mistakes?" So they funded four teams to do these, gave them exactly the same input—I convinced them to use the ShakeOut scenario for that—and then they'd be able to compare their results. So we got the SoSAFE model, and then this NSF funded team did all the seismology simulations and then we were able to say that if some feature showed up in all four models, we're reasonably sure that that feature is real. We didn't rely on something that didn't show up in at least three of the four models.

Conceiving ShakeOut: "Using the best possible science" to depict catastrophic impact of major San Andreas quake

Aspaturian: When did the formal work start on ShakeOut?

Jones: Actually the day we heard about funding, so in March of 2007.

Aspaturian: So you had about eighteen months to put it all together.

Jones: Our goal was to get the ShakeOut scenario together in a year, which is pretty crazy actually. We'd gone through this planning process before we got funding. We were going to go ahead and start talking about how we would do ShakeOut, and literally the day of the meeting we got the word that we were in the continuing resolution.

Aspaturian: Let's give a brief overview of what ShakeOut is and what it was meant to accomplish.

Jones: When the end result of our listening sessions and talking with our stakeholders was "Great, all that information sounds really important, but we want scenarios," we couldn't not do that after we said we were going to listen to our stakeholders. We then had to figure out how we would do it, so we hosted a meeting. It was up in the Salvatori Room at Caltech. The state geology group came—quite a big contingent from CGS, the

California Geological Survey—and a bunch of people from SCEC and the various universities, and Mike Shulters was there. I have, looking back at that meeting, described it as a Tom Sawyer experience: I felt like I was convincing everybody to come in and whitewash the fence with me. We literally found out at the meeting that we were getting the federal funding: CGS said that they would put up $50,000 and commit people to the project.

That was a really strong commitment, and so ShakeOut when we got it done was a joint publication of CGS and the USGS. Other groups committed to various things. We had various teams outside of earthquakes from the USGS too. So ShakeOut in this sense was that asked-for scenario—a document using the best possible science to describe what a big Southern California earthquake would be like.

Aspaturian: How did you define a big earthquake?

Jones: Well, right, it's actually a really good question because there are going to be earthquakes that are going to cause a lot more damage than the San Andreas. So the decision to base it on the San Andreas was made for several reasons. First, it was obvious it was going to have more traction with the public because they knew about the San Andreas. Second, it is the fastest moving fault and so an event on the San Andreas is the single event most likely to happen, although probably not the next one because there's a hundred other faults that could go first. Third, it also affects the largest area. The fourth and to me, biggest reason was really driven by my recognition, which came out of my being on the seismic safety commission, of how policy makers did not understand what it meant to have fault offset—that there are all these lifelines coming into Southern California crossing the San Andreas fault. People were thinking of the earthquake in terms of shaking damage, and maybe they thought about it causing fires. They didn't think about it as the disruption of the infrastructure.

Aspaturian: They saw it in isolation from all these other things.

Jones: Well, many people don't understand what fault offset is. I once dealt with this many years ago with an engineer at Army Corps of Engineers, who was—they were—trying to build a dam one mile from the San Andreas fault up in the San Bernardino Mountains. They said, "We've got this handled; we modeled this with the epicenter right in the mouth of the river. What's the chance that the epicenter is actually going to be there?" And I'm saying, "It doesn't matter. The fault offset is there." This engineer, planning dams, didn't get it. The fact that a magnitude 8 quake on a fault means that 200 miles of the fault is offset and that everything that crosses it is broken was not in the public consciousness at that point. That's where it felt like having the ShakeOut scenario was a chance to really have people think about how a major quake is going to affect everything. For example, how do you get mutual aid to come in and to help fight the fires? The roads are all broken. All these things—

Aspaturian: The water—

Jones: *Every drop of water that comes into the LA area has to cross the San Andreas fault. It was so not understood how crucially important that is*—that was the driving force to me. [*See also Session Nine*] But there were all these other aspects too. Everybody could be involved, and, as I said, we considered it the most likely of the big earthquakes to occur.

Aspaturian: Did you also see it as the means of refocusing the public's attention on quakes and the potential damage? After all, it had been a number of years since Northridge in '94.

Bringing together multiple stakeholders to craft ShakeOut

Jones: That came later in the process. So as we're working on this and putting it together, we then had to say, What do we do with it? How public do we make it? The main constituents were these emergency managers. How important was it to get the information to the general public?

Aspaturian: Yes, I see.

Jones: It was sort of the ultimate in serendipity: A bunch of other pieces came together. So I'm spending this year-long effort trying to herd all my scientific cats and get them headed in the right direction. That was a really big challenge because first you have to do the geology, the geology goes to the seismologist, who do their models, and then those go to the engineers to have their say—that's 17 different engineering groups to say what the impacts were going to be—and then it goes to the public health people, then to the emergency managers, and then there's the business disruption piece. And every time a geologist or a seismologist took too long to get their piece done meant that the people at the other end of the process were not getting the time they needed for their input. We had to really hold people to the schedule.

We were also doing secondary geologic hazards—the triggered landslides, the triggered liquefaction Again, not really understood. And then the fire analysis had to wait for the engineering analysis to be done first. There were two people—I won't say names—who weren't getting their pieces done, and I finally said, "You've got two weeks to get it done. Three weeks later, I said,"Okay, we're going on without you." A week later, their stuff came in. But it had to get down to that for some of them. So that process is all going on, and I'm overseeing that with the goal of trying to complete it within a year, and we were struggling to get it done. We didn't make it by then—by March of 2008, we hadn't gotten it done. And then if you remember, in early May 2008—I can't remember the exact date [May 12]—there was the Wenchuan earthquake in Sichuan province in China. It killed something like 70,000 people.

Aspaturian: Oh, yes. That was a horrifying tragedy. All those schools.

Jones: Right. And the earthquake was almost exactly the same as the San Andreas scenario we were doing for ShakeOut. It was a magnitude 7.8 on a strike slip fault. And so all these reporters are calling us, asking what it would be like if that had happened here: "Yeah, we got the answer, but it's almost not quite there; we gotta get this done!" So I think it was ten days after the Wenchuan earthquake that we held the event to release the scenario. Mayor [Antonio] Villaraigosa [LA mayor, 2005–2013] came to it. We had other local leaders like that, and we had our team; we had a series of presentations, and we took people through it.

Genesis of ShakeOut drill & proposal to hold international earthquake conference in LA

So that was the release of the report in May of 2008. By this time we had decided to do the ShakeOut drill. So, to step back a bit, when we were working on ShakeOut, LA City councilman Greg Smith came to me. He represented District 12, which is the one that had both Northridge and San Fernando, and he wanted to do an earthquake conference. He was trying to get through a retrofit of concrete buildings and being blocked left and right. The head of the building department didn't want to do it.

Aspaturian: Too expensive? Too much trouble?

Jones: Yes. Both. Too much trouble, and politically it made the developers mad at him. He had a nice, cozy relationship with the developers, and there were others too. So Greg, the councilman, is trying to do this, and he came up with the idea of holding an international earthquake conference in Los Angeles to try and focus attention on this and bring in experts from places like Japan.

So he came to me to talk to me about this. In fact, by that time we also had the Art Center College of Design involved. After seeing what had happened with Hurricane Katrina, they had approached me in late 2006 about wanting to collaborate with us in using design to encourage earthquake preparedness. It was a very widespread thing, looking at Katrina and realizing that could happen to us with earthquakes—everybody in LA understood that. And in fact there was a big radio program on it. KNX and KFWB [the two Los Angeles 24-hour news radio news stations at the time] did a joint program where they broadcast exactly the same thing for an hour. It was called "There but for the Grace of God," and it looked at Katrina and at our earthquake issues and drew the parallels. It was with me, Peggy Brutsche from the American Red Cross, Sheriff Lee Baca, and Fire Chief Don Manning, who had been the head of LA City Fire during Northridge. And they had the four of us as panelists to talk about what the impact of a major San Andreas earthquake would be.

Art Center had a program—they still have it—called Design Matters, focusing on the social impact of design. I met with the school's president and the head of Design Matters, and they said, "We want to do something about earthquake preparedness." So, we're having these discussions, the city comes in wanting the conference, we're doing the ShakeOut scenario, and we realized—somehow we've got to put all of these pieces together.

We had a meeting in November of 2007, and at this point, the seismology part was all done; we were finishing up the engineering; and we're still asking, How are we going to do this, how are we going to release it, when are we ever going to get it done? We got it done. The fact that we got it done in one year and six weeks is astonishing—March '07 to May '08. The next scenario we did took two and a half years, the scenario after that took three and a half years, and the one after that I think more like five years. Anyway.

Aspaturian: Sounds like everyone's adrenalin was pumped for the first one.

Jones: The adrenalin was up, and I didn't let go. I'm not sure everybody was happy with me in the process—

Aspaturian: Probably not.

Jones: Once we'd engaged the users, which we did, you can't wait for another two years. Their attention span doesn't last that long. So, there was this meeting in November. Remember the Great American Smoke Out? It was an annual event that the Lung Association would do to try and get people to not smoke on that day.

Aspaturian: Oh, yes.

Jones: We're having this meeting on that day, discussing how to release our scenario, and that's where actually the word ShakeOut comes from. You have Smoke Out; maybe we can have a ShakeOut.

Aspaturian: Do you know who said that?

"Using a public drill to get the focus" on the SoCal ShakeOut earthquake scenario

Jones: Me. [Laughter] Yeah. We also knew about Earthquake Day in Japan, which commemorates the 1923 earthquake that destroyed Tokyo, and they would have this huge public event. We said, "Let's try to do the same thing. Let's have a huge public event." It turns out that Earthquake Day never had more than a million people participating, and our first ShakeOut had five and a quarter million people. So we already had this idea of having the conference, along with some sort of public art event, and then we came up with the idea of the drill. There were actually two parts to that: There was the Golden Guardian exercise, which is an annual state emergency management exercise, which they did on ShakeOut day that year, and then we did a public drill in addition. So there was basically a professional emergency management drill as well as a public one.

The idea of pulling all these four activities together began at that November meeting and evolved pretty quickly, and we ended up with a steering committee with representatives from these various groups and from OES—oh, and State Farm Insurance. Somebody on the seismic safety commission was a State Farm executive, and I've never quite figured out why they did this, but they called me up and said, "We have this young executive, Monica Buchanan, who we're wanting to give more opportunities to; have her work with you on this." So Monica came to work with us, which is a large part of how ShakeOut was successful because she was much more organized than any of us wild-eyed scientists, and the pieces came together because of this.

We ended up with an executive committee of me, Monica, John Bwarie, who was a staff member for Greg Smith representing the city, and Mark Benthien, the head of outreach for SCEC, and the four of us did a lot of the direct planning of how we were going to pull the ShakeOut drill off. The larger committee had people from the Art Center, OES, and Ken [Kenneth] Hudnut from the USGS, so it was a combination of the subgroup and the larger group coming up with the various ideas. And then from November 2007 to November 2008, we planned the various pieces of it. It's interesting because originally ShakeOut to me was always the scenario—with the drill as a one-time event to explain the scenario.

That was my goal: trying to use the public drill to get the focus on the earthquake, and really drawing on insights from social science, which I'd been pretty exposed to by this time. The ShakeOut has continued. It was partly because we ended up asking, "Well, how do we let it grow? And we decided we needed to find other people to take the message out. So I developed this slide deck and narrative that was all about explaining the scenario—here's what the ShakeOut earthquake's really like—which I then gave to groups in San Bernardino County designated by their emergency management and city council, and they would then share it with others. We went to every county in Southern California to do this in some way. I was focused on getting the ideas from the scenario out to the public.

"Drop, cover & hold" goes viral

But we hadn't quite figured out what the drill itself would be. By this point, the scenario's done and had been released in May 2008 at this big public event with the mayor and other officials. In July, Mark Benthien and I went and met with emergency managers, and I think some business managers, from cities in Orange County at a Red Cross facility, I think in Yorba Linda. So a roomful of emergency managers from a variety of cities and I think some businesses were there, also the Red Cross.

And we told them how we've created this earthquake scenario because there haven't been a lot of earthquakes recently, and people have forgotten what it's like. A synthetic earthquake—

Aspaturian: To keep it in the public consciousness.

Jones: A way of raising awareness when we don't have the big earthquakes to do it, because it's really been too long since there's been one. Literally two minutes after I said this, the Chino Hills earthquake began. Ten miles away from us, magnitude 5.7. Everybody looked at me, and then everybody went under the table—because it was a roomful of emergency managers. Mark Benthien had the presence of mind to pull out his phone and get some pictures of this. We couldn't get through to anybody on the outside, because it had been a big enough earthquake that none of the cellphones are working. I was actually close enough to the Chino fault that I went out and looked at it; it didn't have anything on it.

Afterward, in the video footage on TV of people reacting to the earthquake, nobody did drop, cover, hold on. Everybody ran. And so our committee started thinking about how the only time anyone ever really hears about how to protect themselves during an earthquake is in school. Mostly elementary school. And how many people living in LA actually went to California public schools? It was really appalling that people were putting themselves at a lot of risk. So because of that, we decided to focus the drill exclusively on *drop, cover, hold on*. You've seen the graphics that we have now for drop, cover, hold on.

Aspaturian: Yes, of course.

Jones: That was all created by the designers from the Art Center and some other professional designers we had working with us. All the orange and blue graphics that you've seen for ShakeOut was done by students at Art Center. And we actually had a Beat the Quake game that you can still find online.

California Earthquake Authority put up a chunk of money to fund publicity about the drill, which helped pay for all this. So as I said, it's Tom Sawyering. We didn't pay for most of this. It was a process, a lot of serendipity, and just all of these things coming together.

Aspaturian: The timing was very fortuitous in a number of ways. Not for those in China who experienced the Sichuan quake, but yes.

Jones: Starting from Katrina. Everything about multi-hazards seemed to me to be a successful pivot on an unexpected event.

Aspaturian: That's a good way to put it.

Jones: And then it worked. Part of it was we really did explicitly work with the social scientists—guys like Dennis Mileti, who had a heavy involvement on this. In fact, he and Jim [James] Goltz from OES wrote one of the chapters of the scenario, based on studies of how people respond to disasters and what is likely to be the public impact. So we had lots of discussions with Dennis on what makes for successful outreach on risk.

There are several aspects to that; one is that when we're given information about a risk, we need to talk about it with people we care about. It's a process the sociologists call milling. So part of the idea of having everybody do the drill on the same day was to encourage milling. You get home, "Mommy, I had an earthquake drill at school today." "Oh, I had one at work, too." And dad says, "Yeah, I had one, too." And you all talk about it. We explicitly scheduled it for ten days before Thanksgiving—

Aspaturian: Because people get together for Thanksgiving.

Jones: Right, right. The other thing that the drill gave us had to do with how people respond to visual cues. What's your visual image that somebody's preparing for an earthquake? It's very hard to visualize. Well, doing drop, cover, hold on, you look a bit like an idiot. It's a pretty obvious moment; it's visual reinforcement. We were sort of explicitly using all of these different types of ideas from social science as we planned it.

Aspaturian: There was kind of a virtual reality component really, with the sort of simulated earthquake activity.

Jones: Right. We actually had that first drill in the middle of this international earthquake conference in downtown LA, and before the drill we had a press conference. There were 27 news cameras in the press conference. This was 2008, so it went out on Google, and we had over 10,000 news reports about the ShakeOut. And being able to see that —

Aspaturian: Of course.

Jones: All right, so there was another problem. There was a point at which we said, what are our goals; what are we actually trying to get out of it?

Aspaturian: With the drill you mean?

Jones: With the drill. This would have been back in the spring or early summer. First, we were trying to get people to participate. Second, from that participation we wanted to build greater awareness, and third we'd be wanting to actually bring about change—getting people to mitigate. How do we measure any of that? So I remember Mark Benthien saying, "We've got to think big; we need 200,000 people in the drill." And I said—and I was joking— "Ha, I was thinking 5 million." But then we talked about it—that's one-quarter of the population of Southern California, and why don't we think big? But how do you possibly even know that you've got 5 million participating?

And that's where Mark came up with the idea of setting up the website and getting people to register. I can remember when LA Unified signed up, and we suddenly had another 800,000 people participating. The five and a quarter million people who were registered also included every student at Caltech because Caltech signed up. Did they all participate? They were all exposed to it. We ended up having, I think it was, 5.24 million registered to participate. And 10,000 news stories.

Aspaturian: It went national.

ShakeOut's evolution & ongoing legacy

Jones: It was really successful. We had lots of foreign media at that press conference. Remember, this was planned to be a one-time event to explain the scenario—and to me that was the function. It was science communication using these sociological tools, but the point was to get the people to understand what was in the scenario. And then, the next year I remember somebody from Northern California coming to me and saying, "We want to do this."

Aspaturian: I was going to ask what they were saying in Northern California, watching all of this.

Jones: I can't picture what they said at the time, but afterwards it was like, "Okay, we want to do this." So the next year ShakeOut became a statewide event, which right there means it's not so focused on the individual [southern San Andreas] scenario.

I always did push trying to make sure there was some science connected to each of these events, but as it grew, it became less and less that. All of the sociological trigger points are still there. But it became something different than I had originally been thinking of and creating. When it went statewide in 2009, we moved it to the third Thursday in October. That was at the request of the schools because they were our largest single group. And that was really good because when I was little, to the degree that anybody did school earthquake drills or gave out earthquake information, it was in April. April 18th was the date of the 1906 earthquake, so April was earthquake month in California. At the USGS in Pasadena back in the 1980s, everybody had to agree to do at least two public lectures in April because that's when everybody came and asked us to talk about earthquakes.

The problem with that timing is the kids have been in school for almost the whole year and just before they're getting ready to leave, you teach them what to do in an earthquake. So, working with a wonderful guy, Bob Spears, who headed the emergency management department for LA Unified, we tried to make the date most useful for schools by moving it to October.

Aspaturian: There's still semi-proximity to Thanksgiving; you're about three to four weeks out.

Jones: Yeah, it's a month before, and it was driven by the needs of the schools. They couldn't do it in September when they're first getting settled. Now, after 9/11, September became emergency preparedness month, and that hadn't quite gotten started yet when we did ShakeOut. Maybe we should have done it in the last Thursday in September, but anyway. ShakeOut continued to grow, and I no longer was directly involved with it; it's all run out of SCEC, and so Mark Benthien found his life's calling in being part of this, and he is the one who got it going nationally. In 2019, there were 62 million participants registered worldwide. Obviously during the pandemic, the numbers have fallen way back, and I'm not quite sure how it's going to go in the future.

Aspaturian: Has this served as a model for similar activities elsewhere regarding other natural hazards?

Jones: We've talked about it. There've been various discussions. There's the Tsunami Walk, an attempt to raise awareness of tsunamis in coastal communities. Actually, for a distant tsunami, you *can* walk away. You can get out of the danger zone walking.

Aspaturian: As long as you know it's coming.

Jones: Once you've got the NOAA [National Oceanic and Atmospheric Administration] warning, you just have to act on it. Tsunami inundations are so much smaller than people imagine. I've had people worrying about tsunamis in Sacramento. Or in Pasadena.

Aspaturian: It's not going to happen.

Jones: Ain't going to happen. We are 600 feet above sea level. There's no 600-foot-high tsunami that's going to make it in here.

Aspaturian: It's not like you're living on Venice Beach.

ARkStorm: Modeling a California megaflood

Jones: On the science side, the next scenario we worked on was ARkStorm [Atmospheric River 1000]. This was because the USGS water program looked at ShakeOut and realized the visibility and success, and how it communicated.

Aspaturian: So what was ARkStorm?

Jones: A big flood for California. We had our first organizing meeting for ARkStorm three weeks before the ShakeOut drill because Dale Cox, who was sort of my deputy on multi-hazards, had originally been deputy to Mike Shulters. Our thinking was, We can't let the momentum drop, and we need to move quickly to keep the momentum going. So we had this meeting, again in the Salvatori Room, thinking about what constitutes a big flood. That was the first time I'd heard about the flood of 1861-62. There was this argument going on between hydrologists, and one big-name researcher in water science insisted that we needed to do 1938— "We know what happened in 1938, and we don't have the information for 1862."

And I was like, "If we know what happened, we don't need to train people on it. They already have that. It's using the science to figure out what's the more extreme event."

"Well, we don't have details."

"So we try to figure them out."

So ARkStorm became a model of what a big flood would be like in California

It was a really interesting process. I'm there as the head of the project—Dale was the project manager for ARkStorm—but it struck me just how similar the whole process is of trying to understand what the hazards are, whether it's a major earthquake or major flood. They are infrequent events, and how do you express both the uncertainty and also the long-term certainty that these events will happen again. And yet hydrology and seismology treat them very differently—or approach the problem very differently. So it was a whole fascinating time for me seeing how that was. We had some great people who worked with us, like Mike [Michael] Dettinger at USGS and Marty Ralph with NOAA and Scripps [Institute of Oceanography], and they really did the science of putting together the storm. It's interesting because basically the meteorologists are better than the seismologists at predicting, right? You can do a lot more there.

Aspaturian: That's true.

Jones: However, they had never tried to do synthetic storms—they were always trying to predict forward—so it was a whole different process for them. The thing is, to model these things, you need a grid. For earthquakes, we had a grid of shaking. They needed a grid of temperature, wind speed, humidity, rainfall conditions—they measure those all the time. That's what you're trying to predict with weather forecasts, but the goal there is always to predict something that's actually happening right there or about to happen. How do you pull all this together for a simulated storm? We really only have more or less complete storm data dating back to the '60s.

So what they did was to look over all of the available data from that time on. There was a really big storm in 1969 in Southern California, where a debris flow killed 35 people Glendora. I remember it as a kid; it was just raining forever. There was another one in '83 that was more in Northern California, but again, it was another one of the big events. There are just those winters when there's lots and lots of rain.

Aspaturian: I remember that winter, yes.

Jones: Well, when they looked at all of these data, they realized that most of the rain in '69 was in January and in '83 it was February, and they just had to do a very small amount of fudging to stitch the two together. Basically the atmospheric conditions were the same. So ARkStorm is really 1969 and 1983 happening in the same year.

Aspaturian: Sort of simulated.

Jones: Yes. They were both such big storms that the simulation gave us up to twelve feet of rain in the Sierras, and then whatever the equivalent of snow was. Then you ask, where does the rain go? That turns out to be a really challenging issue because where it goes depends on what the flood managers decide. If you get too much rain, you have to allow a flood somewhere. You have a certain amount of choice about where you let the water go, out of which dam and flood control system, which is one of those dirty little secrets nobody wants to have talked about publicly. Do you choose to flood poorer areas because then the total financial losses are less? There're all sorts of social issues that come up with this. So we had many of the same team members developing ARkStorm as we had with ShakeOut, but completely different problems to solve when the storm ends. And then actually we worked with FEMA and the FEMA flood maps to come up with a map of probable inundated areas. And the amount of water was such that we flooded 24 percent of the state.

Aspaturian: Oh, my gosh.

Jones: Well, 24 percent of the buildings. That's different. Twenty-four percent of the buildings get flooded or have flood damage. Then we went through a whole engineering analysis focusing on the fact that we have all of these 19th century levees that were thrown up by farmers and they're not properly engineered at all, and what are they actually going to be able to withstand, and how many places do you have built under the levees. The politics was very different than with earthquakes.

Also, we got a fascinating environmental study done out of it. In the USGS, we have geo-health studies sort of assessing the impact of geological and environmental events on human health. It evolved out of our mining program where the issues were the impacts of mining residue on human health. That was a whole research area. And those people got involved in ARkStorm and looking at what happens when, for example, you flood an animal feed lot. Can you get the antibiotics fed to those animals out of the way before the floods come through? What happens when you flood and kill 10,000 head of cattle and that then flows into the rest of the water supply?

Aspaturian: A lot of toxins get released.

Jones: Geoff [Geoffrey] Plumlee from our geo-health program got the EPA database of toxic deposits in California and how many of them would get flooded, and I think there were something like 7,000 EPA-recognized toxic sites that would be underwater in the ARkStorm scenario. Then there's debris flows, which depend on what and where the fires have been the year before. We made this agreement that whatever fires we get in 2009, that's what we're going to use because it was 2009 when we were planning this out. Whatever fires we get this summer and fall—

Aspaturian: That was a big year for fires, too.

Jones: That was the Station Fire!

Aspaturian: The Station Fire; I remember watching it burn in the Verdugos.

Jones: I was evacuated for the Station Fire. We lived up in La Cañada then, across the highway from the golf course, and we were the first block evacuated. Our debris basin came within eight inches of overflowing—it became a little more personal than I would have preferred. So we ended up with a pretty substantial debris flow issue within our ARkStorm. And then we used the same techniques that we had used in ShakeOut on the engineering and the business disruption and economic consequences to estimate the losses. We didn't try to estimate human death toll because that completely depends on how evacuation is handled. We did estimate numbers of people evacuated from flooding, and it was one and a half million people.

Impact of disaster research & modeling on personal outlook

Aspaturian: What effect does working on all of these disaster scenarios have on your mental outlook?

Jones: It gives you balance, right? There's all these possibilities, most of which aren't going to happen in my life. We did end up leaving La Cañada during the process of making ARkStorm. There were a lot of reasons—mostly the kids were no longer in school, and we moved to the current house where we could walk to Caltech. But I found I didn't want to be that close to the fires and the debris flows. That was a factor in that decision. Along the way as we were working on ARkStorm, Dale got pulled back by Mike Shulters, who had been named the new western regional director and needed Dale. I remember I had this argument with Mike. He was like, "Don't worry, you can backfill." "There's nobody like Dale! That's why you want him! I've never met anyone like him, … except John Bwarie," the guy from LA City.

This was also after the crash of 2008. The city was in financial trouble, and I went to Greg Smith, the councilman, and we arranged an interagency personnel agreement—an IPA—where the USGS sent Greg the amount of John's salary, and he sent John to me. This way he didn't have to lay anybody off, so John got to keep his pension, etc., and I got John for three years. He and Dale filled the same role but did it in very different ways. John's a community engagement specialist, and a political staffer was incredibly important in getting ARkStorm communicated and sold to the state. But then the emergency managers wouldn't use it for planning.

Aspaturian: Why?

Jones: It's too big.

Aspaturian: Do you think that was a legitimate reason or an excuse?

Jones: That's really what they felt. And we're saying, "You know it's got the same probability as the earthquake, right?" And they're telling us, "No, that can't be." I do think you can't drill on something that's so devastating there's no way left to deal with it. But they also didn't want to believe because—

who's afraid of the rain? It can't be as bad as the major earthquake, can it? Using the same economic modeling, it cost four times as much as the earthquake—in large part because it affected the whole state, while the ShakeOut earthquake's just in Southern California. But also flooding is damn dangerous and really expensive, and we haven't really acknowledged that. So a lot of that, I think, was pretty deep psychological stuff about how we feel about rain and how we feel about earthquakes.

Integrating social psychology research into risk response & disaster management

It led me to discover some really fascinating research in psychology on what makes you afraid of risks, by a guy name Paul Slovic, who I'm now working with on other projects. [*See also Session Nine*] He wrote in a book called *The Perception of Risk* that we are more afraid of things we can't see. We're more afraid of things that aren't predicted. We're more afraid if we believe the scientist doesn't understand it. So these are all the things that make nuclear power seem really, really frightening: It's coming to get you; it'll kill you with radiation you can't even see. Earthquakes present us with a lot of the same sort of thing. We don't know when they'll happen; they come out of the ground—all of that stuff. With rain, it's predictable, and you see it coming, and both of those things make it feel much less dangerous.

Aspaturian: And we also feel its benign effects so often. It's hard to see it as adversarial in some sense.

Jones: Right. Paul's research I just found really fascinating on explaining what I was experiencing. In fact, I'd met Paul when he gave a talk with the National Academy [NAS] at about this time—I'm trying to remember exactly where. I reached out to him asking for his help as we were trying to start an earthquake early warning system, which by that time was also going on.

Aspaturian: Yes, I was going to ask about that next time.

Jones: The communications aspects—having seen what a difference it made working on these with the social scientists for ShakeOut, I realized how much we needed to do that for earthquake early warning. Paul at the time said, "I'm too busy, but here's this former student of mine, Timothy Sellnow; he and his wife, Deanna Sellnow, are specialists in crisis communication." We ended up using them to help us develop earthquake early warning messaging.

Aspaturian: Do you think that in general the scientific community would have an easier time getting its messages across to the public if it was more invested in establishing relationships with good social psychologists?

Jones: Obviously I do and that's why—

Aspaturian: Obviously you do.

Jones: You know, physical scientists tend to be somewhat snobbish about social scientists. In fact, we talk about social scientists and *scientists*. One of the things I try to make sure I do whenever we're writing about something like that is to make sure we say, "physical scientist" rather than just "scientist." And you know, there is social science that's done that's really stretching the mark to call it science. But there's also really important research out there. When we get to my post-government life next time, I'll talk about how I'm becoming more and more connected to that. [*Session Nine*] USGS Menlo Park has finally hired a social scientist in the earthquake program; she's been given a pot of money to give out, and she's actually funding social science research. We're getting better at it. Not down here yet; none of the social scientists are here in Southern California.

Aspaturian: Maybe that should be your next book. We've talked about your writing something on probability.

Jones: I'm actually thinking of looking at our feelings about randomness. What it means physically within the natural hazards context, and how that then has affected our emotional ability to cope with them.

Aspaturian: That would embrace the social psychology, of course.

Jones: I'm debating whether I should try to talk Paul into co-authoring a book with me, rather than trying to do it on my own. We'll see. Maybe the subtitle should be "Why We'd Rather Feel Safe Than Be Safe."

Aspaturian: That's a good subtitle.

Jones: I'm not sure what the title is. It might be "Death by Design." That's derived from Dennis Mileti, who I was talking about earlier, who famously wrote a report called "Disasters by Design." So when I went to work for the city of Los Angeles [*Session Nine*], we called the program Resilience by Design, which was definitely a play on Dennis's book. Dennis's research was on risk communication—what makes people believe or not believe and act or not act. And at the beginning of the [Covid-19] pandemic, he was quoted in the *New York Times*, saying "This is a really horrible experiment. Everything we say you shouldn't do is happening. The consistency of messaging is really important. If you get to choose which one to listen to, you're going to choose the one that makes you feel better. And that's what's going on." It's as if we took all of Dennis's work and said, "Let's see what happens if we do the opposite in the US pandemic response." And Dennis died of Covid the week before he was scheduled to get his first vaccine.

Aspaturian: How tragic. How old was he?

Jones: Seventy-five. So that's where "Death by Design" comes from. I don't want to make the pandemic a whole theme: You can't write about the pandemic outside of the politics, and I don't want to get too invested in all the politics. So, anyway, how humans handle risk is a place I want to go. I see it as being tied into our fear of randomness.

Aspaturian: Our inability to cope with it very well.

Jones: Yes. We don't believe in it. We are fundamentally driven to ask "why?" Scientists don't like randomness. We created random statistics to try and figure out what information we can get at when it is a random process, because we always want to know *why*; we want to know more. We're driven to think that way at a deep level. That human need for "why" distorts how we respond to truly random events.

Establishing SAFRR—Science Application for Risk Reduction—& modeling additional disaster scenarios

Let me just wrap up this whole scenario thing. After ARk-Storm we did the tsunami scenario. Then what do we do? We'd developed a good program with demonstrated success, and by this point USGS was reorganizing. Dave Applegate had gone from being head of the earthquake program to being the associate director for natural hazards, which included many of the pieces that had been within multi-hazards, so we then created SAFRR—Science Application for Risk Reduction.

Aspaturian: This was in 2011, I believe.

Jones: Right. Multi-hazards started in Fiscal Year 2007 as a 5-year project; October 2011 was the end of that five years. We created SAFRR as a national extension of the MHDP, and I then reported to Dave. We tried to keep pieces of this going. It was complicated politics within the USGS because the way it was left is that money was given to disciplines, and then they had to give it back to SAFRR.

Aspaturian: They weren't happy about that.

Jones: It always made it feel like they are giving some of their money away. Dave thought it was important to have the disciplines' buy-in, that they were funding it, that this was something they owned. I saw it from the other point of view, of resentment. Dave was boss; that's the way it went. And it continued to be this challenge, and then as programs shifted or if money got cut, where do you cut from.

Aspaturian: A lot of politics.

Jones: A lot of government politics. The SAFRR tsunami scenario was the next scenario we did, and we released it in 2013; ARkStorm had been released in, I guess, right at the beginning of 2011, and we did a big conference in Sacramento on flood management. It was well attended. It took a decade for it to connect, but now ARkStorm is really being used for flood management, and it was Dale, really, after I left the USGS, who continued to keep things going. And he continued to keep it going when he was there in Mike Shulters' office, where it just gradually grew. Then we did the SAFRR tsunami scenario, and we released that by having workshops focused on the emergency managers in every coastal county

Aspaturian: Statewide?

Jones: Statewide. That was a very successful thing. I remember this road trip to all the different counties and helping their emergency managers use it. I'd get the inundation maps out, we got some planning done, and there's now the Tsunami Walk every March inspired by ShakeOut. Then we tried to do a wildfire scenario, but there were a lot of complications with that. For one thing, the emergency managers don't need to be told what it is. It would be more around prevention. It got really convoluted by USGS politics and never completely went through. It went on, but without me being directly involved, and it became something different. It didn't turn into a publicly visible scenario.

Aspaturian: It didn't follow the same model as its predecessors in terms of disaster management.

Jones: And then the last one with SAFRR, which I wasn't directly involved with because I was actually working down in the city of LA, was the Haywired scenario, up in Northern California for the Hayward fault. Haywired because we were looking at the effect of the quake on the digital economy.

Aspaturian: Very cute.

Jones: Haywired has taken a very long time to complete, and it's coming out more in pieces. A lot of interesting studies that have been done with it, but there's less of a coherent scenario picture.

Aspaturian: Of course, ShakeOut continues.

Jones: ShakeOut continues as a drill, mostly managed by SCEC with FEMA funding. We're actually now talking about what updates we can do to the scenario. We're still waiting for that quake. I'm down to just hoping I live to see it. Anyway.

Aspaturian: Let's end on that note.

Session 9, May 27, 2022

Abortive firing by Governor Arnold Schwarzenegger; encounters with other notables

Jones: So we were just talking, before you turned on the recorder, about who all I've met.

Aspaturian: Yes. And now let's put that on the record.

Jones: So the discussion began with Prince Andrew [of the United Kingdom], who came a couple of times. The second time I was scientist in charge. I spent a long time showing him around the Seismo Lab, along with my husband, Egill.

Aspaturian: And you said he asked reasonably intelligent questions.

Jones: His questions were quite intelligent—a lot better than from some people I've met over the years. I've met the last four mayors of Los Angeles: Eric Garcetti, Antonio Villaraigosa, Ken [Kenneth] Hahn, and Richard Riordan. I never did get to meet Tom [Thomas] Bradley.

Aspaturian: That's too bad.

Jones: I know. That was before I was too active. I did meet Gavin Newsom but not Arnold Schwarzenegger. Schwarzenegger had fired me.

Aspaturian: Uh—under what circumstances?

Jones: I was on the Seismic Safety Commission, and there was a big move in the state to eviscerate it. Several of the people who worked for Schwarzenegger had worked for Pete Wilson, who was governor during the Northridge earthquake. The commission wrote a critique of how the state handled Northridge—with 140 recommendations. You should never have more than ten such recommendations, but it also really upset the governor's office. They were trying to either eliminate the commission or make it so it wasn't independent—by making it part of the Department of Consumer Affairs! It all came to a head at the centennial of the 1906 [San Francisco] earthquake when there was a conference with OES, the Seismological Society of

America, the Earthquake Engineering Institute, and the Seismic Safety Commission, So everybody's at the meeting.

The safety commission had met the day before, and I did say publicly in the meeting, "If you do this, there's no point in having the Seismic Safety Commission, because the point of it is to be independent." The next day—I guess it was in the afternoon at the big centennial conference—-I was talking with Jack Popejoy, the earthquake reporter from KFWB, and later KNX, and also Sam Blakeslee, a Republican state assemblyman, who is actually a seismologist out of San Luis Obispo, and my phone rings; I answer it: "This is the governor's office," and they told me I was off the commission. At that point I was up for—

Aspaturian: Renewal?

Jones: Renewal. Waiting for state Senate confirmation, and instead I'm hearing, "We've withdrawn your nomination. You're off the commission effective immediately." "Oh. Okay." I get off the phone; I'm looking bemused; and Jack and Sam both look at me: "What was that?"It was the governor's office; I'm off the commission," and Sam the seismologist went, "Oh, those idiots." And, of course, Jack Popejoy's a reporter; he's standing right in front of me.

Aspaturian: You've got the archetypal SoCal earthquake reporter right there in real time.

Jones: Three hours later, Schwarzenegger's appointment secretary called me and apologized. This was the boss of the guy who had called me earlier, and he begged me not to leave the commission.

Aspaturian: Had the news gone public by then? Sure; I bet Jack Popejoy broke it immediately.

Jones: Yeah, and it went around the commission, and I do believe Sam called the governor almost immediately. So my interaction with Schwarzenegger was very interesting, but it was never in person. [Laughter] Who else have I met over the years?

Aspaturian: You mentioned [pro basketball player; LA Lakers 1996–2004] Shaquille O'Neal.

Jones: Oh, Shaquille O'Neal, yes; he was doing an earthquake safety event, and he had me and Kate Hutton come down. I've got pictures of me feeling really—it's hard to make me feel really small—but he made me feel really small.

Aspaturian: So he did not come to the lab?

Jones: No, we went to an event that he was doing.

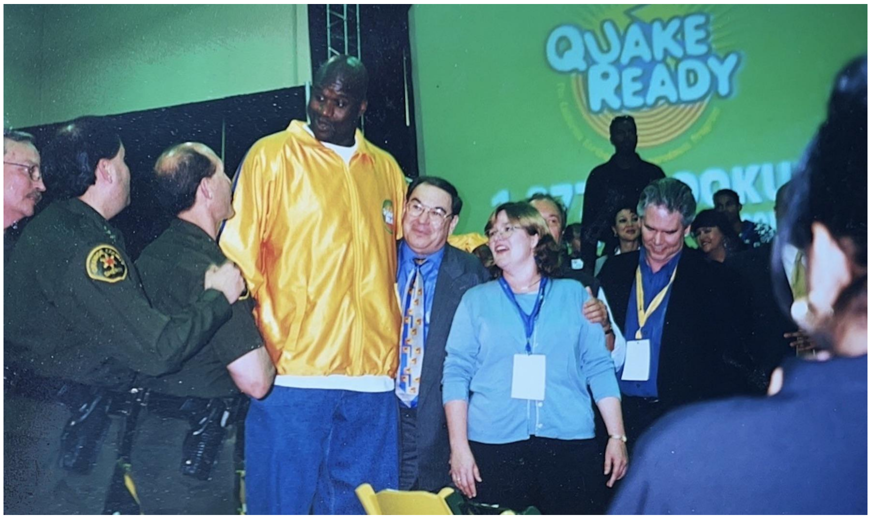


Photo courtesy of Lucy Jones

And of course there had been lots and lots of political figures over the years. I guess I actually did meet Pete Wilson; that was after the Northridge earthquake. At times, it starts all sort of fading.

Aspaturian: Jerry Brown?

Jones: No, never met Jerry Brown.

Aspaturian: Never met Jerry Brown. That's too bad. I have the feeling the two of you would have had an interesting conversation.

Jones: Yeah, he's one I would have liked to have met. I was at an event with Deukmejian, but I never spoke with him. And then lots and lots of congressmen and senators and such. And when we did the Multi Hazards Demonstration project, I was taken around to visit lots of different members of the House of Representatives about it. Maybe half the time it was the staff, and about half the time I met the member. And then Will Ferrell asked me to come and do a podcast with him. That was a couple of years ago, right before the pandemic.

Aspaturian: A podcast on?

Jones: Well, Will Ferrell has a podcast where—what is he is the character he plays—

Aspaturian: Oh, Ron Burgundy?

Jones: Ron Burgundy. So it's a podcast of Ron Burgundy doing a podcast. It was actually really funny, and some real information got out. I was really impressed with the acting ability. He became Ron Burgundy.

Aspaturian: Yes.

Jones: And then he became not Ron Burgundy when we talked after the podcast was over. It was really fun. It was an interesting thing. And I was on Conan O'Brien once.

Aspaturian: *Late Night*.

Jones: That's the only *Late Night* I've done. And of course I know a lot of reporters really well at this point.

Aspaturian: Yes, obviously. Obviously.

JONES: Brian Williams really liked me, and he was going to do a really long segment with me right before he got removed. I forgot exactly what it was that he did.

ASPATURIAN: He's reacquired his respectability.

JONES: That's right. It was a marginal truthfulness on a report. And he seems to be getting himself back. So, yeah, I've dealt with lots of them.

"Thinking of time in a different way" & unprecedented pace of climate change

ASPATURIAN: I wanted to go back to something I had asked you in our last interview about whether you, as a geologist, see the world a little differently from the rest of us. You had said yes and briefly alluded to your *Getting Through It* podcast on the magic eyes of the geologist, which I've now listened to. Listening to you speak about it, I was thinking how when you go out to a landscape, you can probably see a billion years of geologic history around you. How does that affect your outlook in general? It's different from how the rest of us perceive—

JONES: It is different. It's that function of time—thinking of time in a different way, I think, is a real shift in mindset. These last few days my husband, son, and I were up in Mammoth, and we went every day up a canyon. We hiked our way up until we ran into enough snow where we couldn't keep on going—which was way farther up than it should have been for May. But one of the things about those lakes is that they're all glacially derived if you're far enough north in the Sierra, and it's striking to me that I could see this progression. We were sitting up there having lunch and looking down on a pretty deep Alpine lake. We're on the east side of the Sierras but looking at the western end where the stream was coming down off the Sierras, and you could see where the sediment had been dropping from the stream and a marsh was starting to grow. And this is the process that goes on over time: The marsh gradually fills in; it goes into a swamp, and then becomes a meadow, and eventually it gets trees.

And so these valleys go through a progression that I remember learning about as a kid when I was backpacking up in the Sierras with somebody from the Sierra Club. And from where we were sitting, you could sort of see that process, and then looking around, at the southern end, there were the volcanics from Mammoth, which are mostly from within the last 50,000 years. And then there was batholith of the Sierras, which is like 40 million years old. On one of our hikes, we went farther north, and we were into the country rock, which is pre-Cambrian. I was thinking "I have to look it up and figure out just how old it is." You could really see the difference in the rocks.

Aspaturian: If you have the eyes to see.

Jones: My husband and I were talking about how we sort of go through this geology and how we're looking at it. I had been thinking about being up there as sort of a sensory cleansing of getting away from the noise and the artifacts of humanity, but it was also a temporal cleansing. It reminded me of these progressions of time—how those lakes are there for 10,000 years and then they fill up.

I'm sort of revisiting that process now because I'm also thinking about a talk on climate change I'm going to do. One of the things that's different about this climate change is the speed with which it's happening, and that's the human part of it.

Aspaturian: Yes.

Jones: And just how different from past climate-change events the speed is going to be. That's what terrifies me. It's what I think most people don't get because they don't think in those terms, on those timescales. How it took this long, long time to get us to here. It really struck me, that most people don't appreciate time. We live in the timeframe of our lives, our lifespan, but to understand the world, you've got to imagine a longer time span. And I think that's what geologic training allows you to do. I think at one point in my book, I write that I can talk about the last 10,000 years as "recent" without any sense of irony at all. Right? It's different in that it affects how you see these things. It affects how people see disasters. It's like if you haven't lived to see it, it's not real, is it?

Aspaturian: Often for many people, that's very true.

Jones: Well, for just about everybody, because in terms of what you plan for, you plan for what you've had, right? It's actually an aspect of globalization that now we're all personally experiencing disasters around the world. That means that we have within our lifespan that comprehension of what the different types of disasters are. I think that helps us recognize disasters, and I think it's also increased anxiety because we're not getting the right messaging all the time.. But for planners, I think it's important that we are now seeing these things globally because we can use space as a stand-in for time. In any single location, you only experience these disastrous events every so often, but if you have many more locations, you see them more often. We have to use that effectively, though, and that's what I've been trying, and I think that's what science, especially disaster science, gives us: The ability to extrapolate from other locations into one specific place.

Meets with Mayor Eric Garcetti to propose a seismic resilience plan for Los Angeles

Aspaturian: Very interesting. We can come back to this, but I want to return to the chronology for a little while. In 2013, I believe—I've seen 2014 in some spots too, so I'm not sure of the precise year—you went to work with the mayor's office.

Jones: Okay, so 2013 was when Mayor Garcetti was elected, and I was at this point doing quite a bit of work with John Bwarie, who I've mentioned before—

Aspaturian: Who worked at SAFRR.

Jones: Right. SAFRR and the USGS. John Bwarie had left us by then and had his own company, but he had all the contacts in City Hall because he had been a staffer for Councilman Greg Smith. So at the time, there was a project going on in Northern California—San Francisco, after ten years' work, had finally gotten through its first ordinance for mandatory retrofit of soft first-story buildings. It actually began before the 2006 earthquake centennial; it picked up speed with that, and still it wasn't until 2012 that they got the legislation done. It had been driven by the engineers, some seismologists, and one building official who was really passionate about it—it was a bottom-up process. But they'd passed it, and so John and I talked about it, and we said I should try and go and see Mayor Garcetti and say, "We gotta do the same thing." They got it done, right?

So John started trying to get us an appointment. We kept on not getting a meeting; we were struggling to find a time. At some point after several months, ShakeOut's coming, and we really think we should talk to the mayor before ShakeOut, so he has some idea of what's going to be happening, and his staff agreed with that. So we got an appointment two days before ShakeOut.

In a coincidence that wasn't a complete coincidence; the *LA Times* had been working for two years on a big story, an exposé, about the dangers of concrete buildings, and it ran the Sunday before ShakeOut. So I had an appointment, which we had set up weeks earlier, with the mayor two days after that publication. In fact, since the government had shut down at that point, I was technically not working, so I went as a private citizen. We had his attention because of the *LA Times* article, and we told him about what had been happening in San Francisco, and that we should really try to do the same thing. He says, "I want to do the same thing, but I'm not going to take ten years; I want it done in one year." I was like, "*There's a reason it took ten years*! There are all those people that had to be brought on board." But we left saying, "Let's figure out how we can do it."

Aspaturian: Were you surprised by that reaction?

Jones: I wasn't surprised that he was committed to it. He's a really intelligent man.

Aspaturian: I was going to ask next what your impressions of him were.

Jones: So intelligent. We have a lot of similarities actually. We're both fourth generation Southern Californians. He went to Columbia; I went to Brown. We're both musicians.

Aspaturian: Ah, that is a point.

Jones: People tried to get us to play together, but he's this jazz pianist and I'm a classical musician, and it's like he doesn't know what to do with written music, and I don't know what to do without it. We just weren't going to do that to each other.

I'm actually more comfortable playing jazz now; I started doing it some time ago.

Aspaturian: You can jam more easily into jazz than he could segue into classical?

Jones: Oh, it's very hard for a classical musician because we're trained to play exactly what we're shown. We do a really good job with that. The first time I played with a jazz group it was like "Here's chord progression; play what you want," and I'm like, "What do you mean, *play what you want?*" I'm better at it now than I was back then. That's part of leaving the government and having time for music.

Anyway, he saw things really quickly. I definitely felt he wanted to do what was right. Now, that's "right" within the constraint of political realities, and the constraint of political realities is pretty awful: Where exactly do you define that line between where you can go with expediency versus doing it the best way possible.

Aspaturian: And there's always money to worry about.

Jones: There's always money.

Aspaturian: And the voters.

Jones: So we left this meeting knowing that he wanted to do something, and I was going to work with his deputy mayor, Eileen Decker, to figure out how to do it. I actually came up with the idea of having me go and work with him for a year; we had it defined as a year. Eileen and I talked about this, and I then went to the USGS on how to be able to do this. What we ended up doing was creating a technical assistance agreement [TAA] officially between the city of Los Angeles and the USGS for a cooperative project.

Aspaturian: Was the USGS happy to have you do this? Did you encounter any pushback?

Jones: No, I mean they were shocked. [Laugher] It's not what scientists do. But by this time, my boss was the associate director for national hazards, a guy named David Applegate—

Aspaturian: Whom you mentioned last time.

Jones: He's just been nominated to be the permanent director, which I think is fantastic. And he had vision. He didn't need to keep the USGS scientifically pure. He's like, "Sure, go and see what you can do." He also, I remember, at my retirement party, introduced himself as my boss, "as if anybody could tell Lucy what to do." [Laughter] I guess his attitude toward me was to give me free rein and see what I could do. It was great working with him. So he didn't push back, and he was high enough up that it got done. Also the director at that point was still—was it Marcia McNutt? Yes. She was still director at that point; she's now head of the National Academy of Sciences. She's another person with vision, and she was fine with the idea.

So the only people above me in the chain were backing me, and the rest of it basically didn't matter then. But the agreement was that we would work on this cooperative project where the outcome was a seismic resilience plan for the city of Los Angeles, and the USGS would put up three-quarters of my time, which was officially worth however much money, and the city of Los Angeles would put up staff time to match that.

Developing seismic resilience plans for LA while maintaining scientific autonomy

Aspaturian: Were you back to working full-time at this point?

Jones: Oh, yeah, this was 2013 by this point. We negotiated the agreement in 2013, and it was announced and began on January 17, 2014.

Aspaturian: Sounds like quite a portfolio.

Jones: Officially I was three-quarters at the city, and one-quarter here—which meant that I was full-time with the city and half-time here. Definitely the most worn out I've been in a long time. But I had an office in City Hall—in the deputy mayor for public safety's office, and it was a really interesting process. Sometime within the first couple of months, there was some incident—I can't even remember what it was—and there was a head of communications telling me I needed to not say something or other, and I was like, "I don't work for you. I can say what I want." And he's like, "Ah, what?" "Go look at the agreement. You don't get to tell me what I get to say." He backed off, and it was important because it meant that I had independent credibility. If I had been in the mayor's office, if they were paying my salary, it wouldn't have worked.

Aspaturian: I understand what you're saying.

Jones: I was there as an independent scientist. That mattered hugely to the credibility of the project.

Aspaturian: Yes, as opposed to being seen as a political appointee.

Jones: Yes. And then we got to a point with the deputy mayor where we were starting to say, "We need to talk with people, we gotta figure out what's going to work." It's one thing to say, "Here's what scientifically is needed—"

Aspaturian: What was your agenda?

Jones: Oh, okay, that's true. It was in the TAA, so we defined it at the beginning. We were going to come up with resilience approaches to address three specific issues. Well, we wrote it out as four. One was the soft first-story buildings, one was the concrete buildings, one was the water system, and the fourth one was telecommunications. And the reason we called it telecommunications and not power was because we weren't sure we could get Los Angeles Department of Water and Power [LADWP] on the power side to do what was needed. And in fact we didn't. The whole system had collapsed in '94, and they were back on in 24 hours. Which was amazing. It made them think "we can handle whatever gets thrown at us because we handled Northridge, and that was the biggest thing ever."

Aspaturian: But, as you mention in your book, this kind of thinking is a fallacy.

Jones: Oh yeah. We think the worst that we're going to face is what we have faced already. It's the same idea—that we need to be able to see beyond that. Those were the defined objectives, set out at the beginning.

Defining success in political terms: a scientist's learning curve

It was interesting because Eileen explained the political reasoning behind the preset objectives and I was like, "I hadn't thought of it, but it makes sense." We as scientists would tend to say, Let's explore what are our possible solutions." With politicians, they need to show that they were successful. And so we had defined the problems in a way where we knew there was something we could accomplish on every one of them so that we were going to be successful by the end. But we still had the freedom to explore anything else that came in around it.

Aspaturian: Was this a new way of thinking to you?

Jones: To me, it was. Right. That political objective. You have to define your project in a way that allows success to be achievable. That's not how you limit a scientific research problem, but the political necessity of the approach was obvious once it was explained to me. It's obvious that's what you've got to do.

Aspaturian: Let me ask if the new awareness flowed both ways. Do you think some of the politicians and government officials you dealt with gained a new appreciation of how science works as well?

Jones: I do think so. Because one of the other things that Eileen was saying was, "We can't go talk to people until we know what we're going to say. When you go out there as an elected official's appointee, people want answers, and you've got to be ready to give answers. You can't get up there and say, 'I don't have an answer.'" To which I replied, "How can we know what to say unless we've talked with them?" We were sort of at this impasse. But then people outside of City Hall who were interested in the project started asking me to talk with them because they knew I was there, and John Bwarie did a certain amount of encouraging them to ask the mayor's office for access to me. So I ended up doing a bunch of meetings with people before we had anything to say, being able to frame it basically as, you know, "we're trying to find solutions."

Aspaturian: Who were the stakeholders you were dealing with?

Jones: A big one was BOMA—Building Owners and Management Association. Also the Los Angeles Conservancy, which is the historical preservation group, and Apartment Owners of Greater Los Angeles; those were some of the biggest ones. There were some other building groups in there besides those. The Central City Association of Los Angeles. The business associations and chambers of commerce across the city. And then the engineers because they have to buy into it to be able to create a retrofit approach that actually makes the building safer; and they of course have the expertise.

Aspaturian: These are the city engineers.

Jones: Well, them too. I actually meant the private engineers. The Structural Engineers Association of Southern California [SEAOSC] was a really big help, and also the Structural Engineers Association of California [SEAOC], which is an umbrella group over four regional ones. My work in City Hall essentially became several different projects now. Around the building issues, we formed a committee of engineers, and it included the head of the LA Department of Building Safety and two or three engineers from their team, as well as the president and incoming president of SEAOSC, the president of SEAOC, who was another Southern Californian, a couple of different academic engineers that had experts on these areas, a guy named David Cocke, who had his own company but was really an expert on retrofitting. Farzad Naeim, who has his own company, but is also on UCI [UC Irvine] faculty. I'm trying to remember all the people who were on it.

Aspaturian: Quite a cross section.

Jones: It was a cross section of academic, professional, and city engineers.

Aspaturian: Yes, that's what it sounds like.

Jones: And we had lots of meetings. Cause there's the technical issue of, How do you do this? The soft first-story problem involved lots of debates, but it was a bit more straightforward than the concrete buildings, which is really a tougher one. They're much deadlier, and they're much more expensive to retrofit. The whole issue around concrete is really difficult.

"Imagine America without Los Angeles"

So there was this process going on, centering on how do we do it? Then there was the process of meeting with all the various building owners. I developed a talk that I called "Imagine America without Los Angeles."

Aspaturian: I'm sure that caught their attention.

Jones: It did, right. It basically asked, "What would a big earthquake really do to us?" And looking at the current building code and asking, What's going to happen? It was very much built on the results of ShakeOut. I couldn't have done it if we didn't have that really technically solid piece—that bedrock—to work with and the good graphics—the animation of the shaking. So I could say to them "This means a quarter million displaced households. How many of them leave the region?

What's that do to the regional economy?" This was all put in terms of the economic impact, because I was talking to business people.

Aspaturian: Of course.

Presenting LA Mayor Eric Garcetti's proposals for improving city seismic safety to community and business leaders in September 2014. *LA Times photo*

Jones: And one of the themes was: Here's the science; here's what will happen; what do you think we should do about it? So yes, I was in that position of not knowing yet what we would recommend, but what happened then—and I think this is when the mayor's staff saw even more the advantages of this approach—was that by coming without *the solution* but with a framework in which to talk about possible solutions, we got buy-in.

Aspaturian: Right, you gave them a stake in the outcome. Of course.

Jones: Right. And that was a really critical piece of it. It's interesting. Recently I've been doing a bunch of work and discussion on the psychology of why people aren't looking at climate change. And it's given me a perspective to look back on what happened with the project with the city—and we'll come back to that, and essentially why I think it worked. So we had this whole project around buildings with the Department of Building and Safety, and then there was a whole water project with the water side of the LADWP. For the water people, it has been easier to see what the issues are, because they know they've got a problem. With every earthquake we break hundreds of thousands of pipes, right? LADWP runs its own aqueduct across the San Andreas fault, and all the other aqueducts cross the fault. There was a guy, Craig Davis, who had just done a PhD at USC while he was working there, looking at the vulnerability of our water supplies and what it would take to repair the different aqueducts—which is six to eighteen months for the four different water systems coming in.

So on water, we ended up having a team that was really within the department of water and power because they had Craig Davis, who really got it and wanted to do something about it, and he was passionate about it and about getting support from upper management to keep it going. And then there were actually some good things that came out of the telecommunications project. We ended up talking to all the cellular providers, and this resulted in an agreement that was written by the city—I believe this went forward—where all the major cell carriers agreed to share bandwidth after a big earthquake. So if you have a Verizon phone and the Verizon system's down, but AT&T is still up, you'll get to be able to use the AT&T system.

Aspaturian: That was quite an achievement.

Jones: Yeah, that was really Eileen Decker getting that through. There's a cyber security system under UASI—the Urban Area Security Initiative—that was created after 9/11. I talked with those people and raised some of the issues, including that one of the really major data centers for all internet traffic is in a 1964 non-ductile concrete building in downtown that's going to come down in significant shaking; it's not a good building.

I remember being with them at this meeting, and someone saying, "You don't understand, we can't lose One Wilshire." I replied, "*You* don't understand, *you're gonna lose One Wilshire*, so what are you going to do about it?"

Aspaturian: Has One Wilshire been— ?

Jones: I don't know what's happened with it because that's also cyber security, so that's information that wasn't out very publicly.

So we definitely had these different pieces that were ongoing. I had to work with all these engineers on the building project and pretty much worked with Craig on the water project, and both teams were really able to take it up. They were thrilled to have it as a mayor's initiative to get these things done.

Aspaturian: I'm sure.

Writing *Resilience by Design* & implementing its recommendations

Jones: And then there was work on other pieces that I was doing with the deputy mayor. And the end result was to write a report that had recommendations. The mayor's people wanted it to be the Jones Report. The problem was that if I wrote it as a USGS employee, it would have had to go through USGS review to make sure there were no policy implications. It is USGS policy that our reports cannot make policy recommendations. And I'm like, "Ah, guys—you look at the TAA you signed?" [Laughter] But if I was the author, it couldn't recommend policy. So the report came out of the mayor's office with my name in all these various places. I did write it, but the recommendations are officially coming from the mayor's office, not from me.

Aspaturian: What was the title of the report?

Jones: *Resilience by Design*.

Aspaturian: Yes, I have it here. Released in late 2014.

Jones: So in 2014, the 20th anniversary year of Northridge, the mayor announced that we were doing this, and then we released the report in December.

Resilience by Design report, released in 2014

The agreement was made in 2013, but I spent the year of 2014-

Aspaturian: Bringing it to life.

Jones: Which meant going to my office in City Hall for four out of five days a week for the whole year.

Aspaturian: And what concrete—no pun intended—outcomes did this lead to?

Jones: Seventeen recommendations, five of which required ordinances by city council, all of which passed unanimously. Because I went through this process of all of these various meetings, we were able to get BOMA to stand up with the mayor and support him doing a mandatory retrofit of concrete. We worked with BOMA before we actually released the report, and they asked that they could have 30 years instead of 25. I said, sure; we don't know when the earthquake's gonna be; let's get the damn thing started. And some ideas BOMA had about tax structures and whatever are included in it. Once I left at the end of 2014 I was not involved in deciding the wording of the ordinances. I am not sure exactly how all the pieces were laid out and what details might have gotten lost along the way. But we got all participants to agree to it.

Flanked by General Manager of the Emergency Management Department Jim Featherstone and City Attorney Mike Feuer (left) and LA mayor Eric Garcetti, Jones speaks in front of Van Nuys City Hall just after the LA City Council unanimously approved Resilience by Design's proposed retrofit legislation. *Photo courtesy of Lucy Jones*

I said five ordinances passed unanimously, and one of them was to change cellphone towers. Cellphone towers in most places are built to the same standards as any other building—that is, to make sure you can crawl out alive, not to make sure you can use it. So we got one ordinance to use what's called an importance factor of 1.5 on cell towers, which is the same increase in strength that's provided to a fire station or a police station.

ASPATURIAN: Very important, yes.

JONES: So that one was actually the first one to pass. And then there were the big ones—the soft first-story and the concrete mandatory retrofits—and those both went through. The soft first-story retrofits had seven years to get done; now, that's almost completed. And so there are over 10,000 buildings that are stronger now than they were because of this. And that's by far the most concrete result. What exactly is happening with the concrete retrofitting, I'm not sure; I haven't been in there and been involved. There were still some unresolved issues about exactly how the retrofits would be carried: What constitutes effective retrofit is a serious technical issue. But we came up with an ordinance, vetted by all these engineers, requiring this, and it got passed.

There was only one proposal that didn't get any traction, and it was on having a rating system for buildings. It seems like this great idea, making sure people understand that they've got an old building that's got problems. Couldn't a landlord ask for more money in rent if they're renting out a seismically safe building? The problem seems to be the timeframe for an earthquake. You know, when you're only renting, why spend extra for seismic safety when the big quake's probably not going to happen while you're there. It's hard to get that incentive, because there's such an emotional impact about earthquakes.

At first we were talking about a mandatory ratings system, and it was clear that was never going to make it. So what we proposed was a voluntary one, which would begin with the city ratings on buildings. The idea was to be motivational: If the city took that step, couldn't other building owners do the same?

The city was working on it by the time I left, and it took them a year to even figure out how many buildings they owned, because they were all maintained by separate departments. Getting a comprehensive list of all the city buildings turned out to be a huge problem. Once they did, they found they had somewhere around a thousand buildings. Ten of them would get the lowest possible rating, and the mayor's office started freaking out about saying publicly that they had these buildings that were really bad, because they couldn't do that unless they also said, "we're going to do something about it." The whole thing sort of came apart, and it never actually got through. So that's one part that really didn't get done.

Another thing that did come out of it was that part of the city emergency plans included using park facilities as shelters. But those ten very bad buildings were old masonry buildings in the parks! They were retrofitted back in the 1980s so they probably won't kill anyone, but there is no way they will be usable as shelters. The city emergency plans now take that into account and don't plan to use the masonry structures as shelters.

At DWP, some really important things have gone through to a commitment. Well, here's one of the best success stories: They made a commitment to use seismic resistant pipes going forward. And at the time when we were doing this, the only pipes that were really demonstrably better were quake-resistant ductal iron pipes from Japan, and they cost something like twice as much as standard pipes. But the city made a commitment to only use pipes that met these criteria going forward. There's a professor at Cornell, Tom [Thomas] O'Rourke, a wonderful engineer and the world's expert on pipes in earthquakes, and the city decided it would only use pipes that he had certified as being at some level of earthquake resistance.

Aspaturian: They took the threat to the water supply very seriously, in other words.

Jones: Yes, and because they did that and they're such a big customer, pipe manufacturers started coming up with other ways of meeting Tom's standards. And within a few years, there were eight different pipes that he had certified as being earthquake resistant, and the prices came way down. Now other districts can use those pipes because there's a process in place for certifying which ones are acceptable. I think that might be one of the most hidden but perhaps one of the most important things that came out of all this.

But there were other parts like how they were going to retrofit the Los Angeles aqueduct, which crosses the San Andreas fault in that 1908 wooden tunnel. Craig Davis has now retired, and I don't think that plan is going to go forward. And I think that when the big earthquake happens, that pipe is going to break.

Aspaturian: What are the consequences of that?

Jones: I think that all of our outside sources of water are going to be gone. I mean we can survive: There is local groundwater and whatever. One thing Mayor Garcetti always used to say is, "Never do something for just one reason," and one of the other things we did—and I think that this is going forward—was cleaning up the contaminated San Fernando aquifer. It used to supply a quarter of LA's water before it got contaminated with jet fuel post-World War II. It had to be shut down, and remediation of that problem is hugely expensive, but the city got like a billion dollars from the state to get going on it, and it's in *Resilience by Design* as a seismic resilience measure because if we have 25 percent of the water locally, we can live on that 25 percent. You don't get to landscape after the earthquake, but that's going to be the least of our problems at that point. Well maybe that cleanup would have happened without the *Resilience* report. I'm sure that the earthquake resistant pipes wouldn't have happened without it.

Taking *Resilience* on the road: securing policymaker buy-in & crafting legislation

Aspaturian: I'm wondering as I listen to you, how unusual was what you accomplished—a major public policy shift in response to scientific input?

Jones: I don't know of another one in seismic science. Every other major piece of seismic legislation has happened in response to an earthquake. All right, so San Francisco was doing this soft first-story retrofit without a triggering earthquake. This is bigger because it covered so many other things.

Aspaturian: Yes, exactly.

Jones: I don't know of any other case where this was done because of science. Our work had follow-ons with other cities. I completed it at the end of 2014, came back to the USGS and started working with the Haywired scenario going on in Northern California [*Session Eight*] and a variety of other things with Multi Hazards; and then Southern California Association of Governments [SCAG] came to me wanting to have help doing this for other cities. The USGS's attitude at this point was pretty much, "it was a great experiment, but now it's time to just focus on the science." That was sort of another big turning point for me: What's most important to me at this stage in my career? The opportunity came up to work with SCAG, and I decided I would rather see that happen. That's when I retired from the US Geological Survey.

Aspaturian: 2016.

Jones: End of March 2016. And then I set up this contract with the SCAG s to provide a program for 192 other jurisdictions in Southern California. I worked with John Bwarie on a project for two years, where the first year we gave talks for every jurisdiction—at least one talk in every county.

Aspaturian: This is all Southern California?

Jones: Yes. And not San Diego; San Diego is not part of SCAG. Imperial, Riverside, San Bernardino, Los Angeles, Orange, and Ventura counties.

Aspaturian: Not Santa Barbara?

Jones: No, it doesn't go up to Santa Barbara. We didn't go to every individual jurisdiction. That first year, I think I gave eight different talks in different areas. I would speak about imagining America without Los Angeles, without Southern California: Here's the risk we are really facing, what are the major issues; why water and the buildings are so important; and then how to identify your stakeholders to address these issues. We also did workshops where the cities or the counties—the counties could participate in this as well—sent out a building official, an elected official, and an emergency responder. We had all the building officials talk about their retrofitting issues, the emergency managers talk about their response, and the city managers about their governance. And then we had these officials get back together with the other people from their jurisdiction and help them think through, Who are your stakeholders? Who are your supporters? Who are the people you have to win over? The cities left with an outline of their main issues and the people they could turn to for solutions.

And then the second year we invited any cities that wanted to move forward on this to participate in smaller working groups to actually craft legislation. We had 40 jurisdictions work with us in the second half. The legislation didn't all get through. I think we got a dozen that have passed soft first-story ordinances. A couple cities have grappled with the concrete building issues—Santa Monica and, West Hollywood.

A couple of others tackled their water issues. Water gets really complicated. LA has its own municipal water district, but a lot of cities don't. And a lot of cities have multiple water districts. But Craig Davis with LADWP was willing to work with other districts; we brought him in, and he worked with Culver City. They're a small town, but they have three water districts. I remember meeting with the City of Fontana, which has four water districts, three of which came to the meeting. You know the problem is that you can go and start the process, but then you've sort of got to leave, and I'm not 100 percent sure what everybody has done in response after that.

Aspaturian: Is there a mechanism in much of this for others to continue carrying on the work?

Jones: Not enough.

Aspaturian: Not enough.

Jones: Because there isn't funding. There is momentum to the degree, you know, that if we inspire a city, the city keeps on doing it. Pasadena, for instance, has passed a soft first-story ordinance, mandating retrofits of many apartment and condo complexes. And they're trying to deal with their water issues too, because they have some single points of failure in their water system. But grappling with it and taking it on is the challenge. Pasadena's mayor—it was Terry Tornek at the time—came to one meeting that I had on this, and then he took off on it, and got the soft first story ordinance through. What I can do is inspire people; I can't do the maintenance. I'm not in a position to be doing that. And it's also not where my skill is. I don't do great on politics myself.

"Science activation is how do we take the science & use it to make better policy"

ASPATURIAN: I was going to ask—and this actually leads into what you're doing now with your center—What, from all this experience, are your chief takeaways on how scientists can interact constructively with policy makers?

JONES: There's several things. One big issue for me is around science communication. That's now a thing, right? It wasn't when I started at USGS. SciComm is now a big thing. The scientists don't do it.

ASPATURIAN: No, they don't. Not a lot.

JONES: And that is a really important piece of this. As a scientist, having gotten that PhD, I understand at a deeper level how you determine whether something is true or not, and when you believe it or not. And how to evaluate data. I don't know the details of the chemistry that goes into climate change issues, but I can understand what the data says, right? With psychology, with social science, it's the same thing: How do you evaluate the significance of what you're looking at? That's what that PhD is teaching you to do. And I think you need that level of skill to be effective at communicating the science. And it's not just communicating.

I've actually started saying that I do science activation. Science communication is a unilateral process: I have information I'm trying to get it to you in a more effective way. Science activation is how do we take the science we have and use it to make better policy, better actions. That's a bilateral process because it's not just what I have to tell you; it's also what are the constraints of the system that you have to give to me.

Aspaturian: Do you think most scientists appreciate this at all?

Jones: This is a really— well, when I first got here to Caltech, I mean I wasn't doing this at this level. I was a research scientist. But when I started with this, you know, except for my role on the Seismic Safety Commission, I had a feeling of "why are you wasting your time on this, you could be using this to write more papers." And definitely in the '80s and '90s. There was some earthquake in there, I think it was Whittier Narrows [1987] where we had just done this paper on foreshocks and we made a statement about the probability of having a bigger earthquake, and I got feedback from a faculty meeting. Some faculty member was furious that *this girl* was talking about predicting earthquakes.

Aspaturian: A Caltech faculty meeting?

Jones: Yeah. So definitely I had a feeling early on that there was a resistance to this. After I completed my federal service, Caltech gave my center [Dr. Lucy Jones Center for Science and Society] a contract to put on a science activation workshop for graduate students. We decided we could handle twenty students. We had 60 applications.

Aspaturian: Really.

Jones: There's a really big generational cultural shift going on in science.

Aspaturian: I would say so.

Jones: And I think that the whole problem around climate change has shown scientists what happens when you stick in your silo and leave the solutions up to other people. And the newest generation of scientists isn't tolerating that. They want the social and policy interaction really badly. I did the class twice at Caltech, and it was so much fun and such passionate students. Because we have all these legislative connections, we brought in legislative staff from Adam Schiff and Judy Chu's office, and also from the state legislators' offices, and basically we held this weeklong workshop for students to learn how to talk to a policy maker. Looking at science activation, looking at the communication issues, asking what are the fundamental drivers—that was sort of my part at the beginning.

Meeting with the California State Assembly's Legislative Women's Caucus in Sacramento. *Photo courtesy of Lucy Jones*

We then had John Bwarie coming in with, How do you talk with a staffer? How do you make a pitch? Traditionally, you're told: Make sure you have your ask; say what is it you're asking for. But maybe, as a scientist, you're not asking for anything, maybe you're trying to share information. What does that mean, and why should they take the time to listen to you?

So just all of those discussions, learning back and forth there. And they would go in teams of three to meet with these staffers. We actually had these staffers come here because, for one thing, they like coming to Caltech. It's really interesting for them. And we could be a lot more efficient doing it together. Several of them led to ongoing communications.

Aspaturian: And relationships?

Jones: Yes. So it's both been really fun, and there's a desire for this now that wasn't there twenty years ago.

Aspaturian: So this has been limited to Caltech. It's been held twice; are there plans to branch out?

Jones: Well, yeah,, but then—pandemic. So our first branch out was when AGU invited us to their Ocean Sciences Meeting in February 2020. It was a very different sort of format. We did three lunchtime workshops, but we couldn't bring in elected officials in the same way, and then the pandemic happened, and all of this got stopped, and AGU never wanted to pay for it. I don't have outside funding to keep on doing that. I mostly volunteer for my center, but my employees don't.

Aspaturian: Right.

Jones: It's sort of hanging out there now exactly where we take that part.

Aspaturian: I mean, I would think the UC system and possibly Stanford would be interested in something like this, too, if you're focusing on academia.

Jones: And maybe I should go to the Annenberg Foundation and ask for funding for training scientists in doing this. When the pandemic hit, we were also talking with the Los Angeles Natural History Museum about jointly putting on a summer workshop because they have facilities that we wouldn't have to pay for, and we'd give scholarships to students to come. It's finding the funding to do it. So for now it's been Caltech and the one event with AGU Ocean Sciences, and then the pandemic shut everything down, and we aren't quite sure where we're going to take it now. I've since given talks at some science communication conferences, so that's one place where I'm doing it.

Connected Communities Resilience Program: empowering communities to cope with disaster

There are other things that I'm doing going forward. One is something that we call the Connected Communities Resilience Program, which we ended up developing over a few years. It's a process for bringing together community-based organizations like churches and schools—places where people already come together—and helping them connect with each other and figure out how they're going to help each other in the next disaster. The social science research is really clear that it's social capital—the degree to which people are connected to other people—that determines the success after such an event. And that's something that Southern California does pretty poorly. We've done it with a few communities; we're doing two programs right now funded by Southern California Edison and the gas company. But— we'll have a few really interested people in it, and it ends up pulling teeth to get enough organizations together to do this. There isn't enough commitment because Southern California doesn't do it very well.

Basically this is our first time really rolling it out and doing it. I'm looking at modifying it, partly based on a model from Wellington, New Zealand, where they've set up resilience hubs that everybody can walk to within half an hour as a way to keep connected after a big earthquake. I've had the Episcopal Diocese of Los Angeles ask me for help, and we're looking at creating connections between the different churches dispersed so that when someone, for example, gets hit by a wildfire, people who aren't hit by it can come in and help. I think that might be a long-term project.

It's a funny thing—you were asking about the scientific side and other scientists. It's the status as a good scientist that gets people to listen to me and want to become part of these projects, where they wouldn't otherwise. Even though it's not my science research skills that are being needed to do this.

Aspaturian: It's your brand almost.

Jones: It is a brand.

Aspaturian: Yes.

Jones: And so we're developing the program that's an integration of social science and physical science to say how do we actually help each other. It's as much working with John

Bwarie, it's that combination of putting the science together with community aspect.

Aspaturian: So that is the mission of the Lucy Jones Center, this integration.

Jones: The science and society integration where it's John and me. I take his advice on all those community things, and I do the science things and we figure out how they come together.

Aspaturian: Have you ever held or considered holding a workshop where policymakers can come listen to scientists who can communicate effectively about why it is important to consider the science.

Jones: Well, policymakers are hard ones to pull out. I haven't done that, but I don't know how I would get them together.

Aspaturian: Maybe their chief aides or something like that?

Jones: I think they understand the importance of the science. Well, the chief aides are the busiest of all.

Aspaturian: Okay, well.

Overcoming societal hurdles to science activation: bridging the gap between scientific research & public acceptance

Jones: They know they want the science, but they aren't necessarily going to have the energy to move toward it. This is what I wanted to say here: I think there are two societal issues that get in the way of science activation. One is the way in which we communicate science. You know, Caltech and every other university likes the publicity of having their newest research reported in the press. Every campus has offices to try and promote that. But the reality is that the scientific process is not that. What you get out of that breaking-edge paper and especially that newest talk at the next conference are results that haven't been through peer review at all.

Aspaturian: The objective of science is not to hook the public; but—

Jones: But the universities are driven to hook the public because that gives them funding.

But that piece of information that's at the most recent talk has a less than 50-50 chance of being the actual answer. And the actual scientific process that we all know as scientists is that somebody says one thing, and somebody else just goes, "I think that's bull, and I'm going to prove it wrong because I'm going to do this experiment." And five years later, you've figured out what is true. Meanwhile, what the public is hearing is, "Oh, scientists say X," and then next year it's, "Oh. No. X is wrong."

Aspaturian: "You can't trust the science."

Jones: Yes. The message that comes to the public because of this is that science is always changing. That scientists don't really have an answer. It is really awful for society that this subliminal message is what comes through with this approach to science communication. We should not be having any— at the minimum, we should have no press at science conferences because none of the information presented there is peer reviewed. But of course we aren't going to do that.

Aspaturian: I think that would be anathema to consider.

Jones: I think it would be very, very hard to get such a change through. But it's a major problem, because it's communicating that the scientists are always changing their minds instead of engaging in a process. By the time a scientific result is actually ready for policy, your researcher's bored. So here's our second problem: We fund people to do research. You get tenure here because you came up with a new idea, not because you saw that idea implemented anywhere. We do not fund anybody to do the translation. We fund the research and after five years— forget the publicity part of it; you finally got an answer. The researchers are now bored; they've moved on to other things. Who's supposed to pick that discovery up and get it to users? As research organizations we have said, "That's somebody else's problem." But the users, they don't know how to get it, and society hasn't funded those people in between.

And essentially the USGS funded me to be that liaison with the city of Los Angeles. I was not doing cutting-edge research; I was taking existing knowledge that we had gone through the consensus process to develop. But just handing over the report was not enough. When we had finished ShakeOut, we handed in the report, and it made for better emergency response, but it made for no policy changes. Part of why I went to work with the mayor was that I wanted to see if we could go farther. Instead of just saying, "All right, it's going to cause all those fires, let's figure out how to fight them," how about we figure out how to stop the fires from happening in the first place.

And it was a great experiment, and it worked, and we got the best seismic safety policy changes we've ever had. But the USGS is not funded to do that. They can't go to Congress and say, "This is what we're using our money for." Now I think if USGS went to Congress and said, "We want to use money for this; can you give us a separate pot of money to fund a cadre of people who will do this work?" I bet you we could get it funded. But research organizations don't see it as in their mission. And actually the USGS can't do policy. That whole thing where I couldn't be the author of the *Resilience* report, right?

So we have this gap in society between the researchers who are creating incredible information and the people who really need that information; there's no process for communicating it, and the void is being filled with disinformation. With the whole social media environment—all the things that are changing society—that void has been filled up with really bad information. Every year it becomes a more critical problem that we—scientists and the larger society—have not done our part to bridge this gap.

That's the thing I'd like to see changed. It's probably not going to happen at Caltech. Caltech is what it is because it's an amazing place to do research. And what we're talking about here, really, is a different set of functions. It's a different standard for promotion. That's one thing I've learned after four decades in the government and at Caltech: Smart people respond to the reward system, and they are going to do what they are rewarded to do. And at Caltech, obviously, you get your tenure because of critical, important research that you've done. We understand how that works, and you get amazing research done. But I also think, Thank heavens we're starting to see promotions also connected to treating other people like decent human beings. That's finally becoming part of the process, too, and I think that's really important.

Caltech is not going to be funding people who don't write a new paper. But who should? We've got to find those sources. NOAA and the USGS could be that if we dealt with this issue of policy and where that function lies.

Aspaturian: There's private philanthropy, but that comes with its own whole can of worms potentially.

Jones: Right, and that's how I got started. My center has got a few different grants from private funders.

Aspaturian: Who are they?

Jones: The Weingart Foundation and the Annenberg Foundation gave me starting grants and of course the SCAG projects kept us going. At this point we're mostly working with—they aren't general grants; they're purpose grants. Southern California Edison foundation has actually funded us quite a bit over time. They developed much of this community resilience program, and Annenberg also contributed to that. And then the California Earthquake Authority gave us a grant that supported a process where I went with them to give presentations to various communities, and they gave us a grant for the center. Right now I've also got a grant from Caltech to create the Earthquake Fellows Program to use seismology to bring under-represented groups into science.

Aspaturian: Has that started?

Jones: We had our first meeting two days ago with the students, who seem really wonderful and excited.

Aspaturian: How are under-represented groups being defined in this context?

Jones: We put out applications; this is of course the pilot year.

Aspaturian: Yes.

Jones: We just reached out to Pasadena and Alhambra; we didn't want to have to deal with transportation issues and all the other things. We had spots for eleven students and got 47 applications. Everyone selected is either from an under-represented minority group, which is African American, Latino, or Native American, or on free lunch or both, and most of them are both.

Aspaturian: That's exciting, very exciting.

Jones: It's very exciting. We'll see how it works. The Seismo Lab hired me to talk with its professors and figure out how we want to do outreach, and this is something that I think NSF is pushing organizations to do. The other part that's cool is that the teachers will all be graduate students. This is part of the grant—them learning how to teach.

Aspaturian: Yes, of course, how to communicate.

Jones: How to communicate. And we've hired a high school science teacher to be the coordinator and do the student interaction. We had our first organizing meeting and then the first lecture day will be June 4th [2022], and it'll continue through the summer. So that's a really exciting project.

Lucy Jones Science Center: from seismic safety to climate action; using music to inspire change

And I've got funding from the United States–Japan foundation for the project I call ["Tempo: Music for Climate Action"](https://drlucyjonescenter.org/tempo), which is a collaboration between climate scientists who know what we need to do, social scientists who understand the psychological factors that are keeping us from taking action on climate change, and musicians who know how to evoke emotion. The idea being to use music to change the emotional climate around climate change. So we'll see how much we can get done. We're bringing together the scientists, the social scientists, and the musicians with the goal of supporting musicians in composing music that will actually inspire action. And this also involves working with Neil Fromer, who's executive director of the Resnick Center [Resnick Sustainability Institute] here on campus.

This also goes back to 2019 when I was a visiting professor up at the University of Oregon School of Law. The policy center there wanted me to talk about resilience, so they had me come in, and it was really fascinating. And that's where I got to meet Paul Slovic, who's the psychologist who studies risk.

Aspaturian: Yes, you talked about him last time. [*Session Eight*]

Jones: He's at Oregon, studying how we perceive and react to risk, and particularly the psychology of why we don't act on it.

There's a whole bunch of factors. Take fear, right? We've evolved to respond to fear. We don't like being afraid at all; it's a very negative emotion, and we'll do a lot to change that. On a short-term risk, we've evolved to take action: We need to be running away from the predator or turning and fighting, depending on what the situation demands. But on a long-term risk—

Aspaturian: Like climate change, for example.

Jones: We have the option of not thinking about it, and *we don't feel afraid if we don't think about it*. And we can get away with it because it's too long-term a risk to be directly affecting us. Sadness comes into this too. Also despair. When we grieve over something, it's a disempowering emotion. It makes us want to go hide. If we feel like there's nothing we can do about something, we are never going to take action to prevent it. Look at a lot of the music that's meant to raise awareness about climate change; it's making people feel really afraid and really hopeless. It's a horrible message: We're destroying the world. So we need to have music that's not focused on fear and despair but focused on hope and empowerment.

Aspaturian: Motivational in some sense.

Jones: It's also really tempting to go to anger. Anger is a very motivating emotion.

Aspaturian: It's not a constructive one, necessarily.

Jones: It destroys community: You have to be angry *at someone*. And our only solutions for climate change are ones that we can't do alone: We have to be forming community. So there are all of these psychological issues. There's also the scientific story —that there are solutions. Helping the scientists share that positive story is as important as anything else because we have to believe we'll make a difference. So bringing together the scientists, the social scientists, and the musicians—it's really exciting. The first year of Tempo was a process, by which we sort of formed these ideas and had some meetings within the core group. We got a film composer and a choral composer, funded by the US–Japan Foundation. They had come to me and said, "We'd be interested in funding you, what would you be interested in?" They thought I was going to be giving them something about earthquakes. I was like, "Well, you know, I can't justify working on earthquakes when you look at what's going to happen with climate change. And this is what I'm interested in right now."

Aspaturian: When did this shift take place for you?

Jones: Well, it was also part of this decision I made to leave the government. I can't as a government scientist talk about atmospheric science because I'm not an atmospheric scientist.

And yet, I don't see the point of pushing earthquake resilience if we aren't dealing with the climate.

Aspaturian: Which is an even bigger issue.

Jones: Yes, because it's a bigger issue and more is going to be happening. So I was already there. Another piece of this is our family tradition of retiring from technical jobs to go and become musicians, because none of us are good enough to play professionally—well, one cousin made it as a professional musician. Most of the rest of us have found some other job along the way, retired early, and gone back to music. That was a factor for me. Of course my music with viola da gamba is not exactly popular. I can't remember if I said this to you earlier—that my brother left a software engineering job to become a jazz pianist.

Aspaturian: I think you said he was a gifted pianist [*Session One*], but this is the first time I've heard about the new vocation.

Jones: He gets three gigs a week, and I get three gigs a year. Yeah. Actually my first professional gig was during the time I was at City Hall. That summer I ended up being in a four-viol consort to accompany a ballet down at the Orange County Center for the Performing Arts.

Aspaturian: How very cool.

Jones: It was like "I don't have enough to do? I need to be running down to rehearsals in Orange County?" But it was so much fun.


DEPARTURES
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Performing a gamba duet at Loyola Marymount University with Shanon Zusman in 2015 and taking part in “Bach in the Subways” at LA’s Union Station with the community musical group Los Angeles Baroque in 2018. *Photos courtesy of Lucy Jones*

And so I wrote this music about climate change. I’m sorry, did we talk about this?

Aspaturian: I don’t think so.

Jones: Well, it involved taking the global climate data and turning that into pitch. So you’ve got this chromatic non-musical line of notes, and I’m trying to make music by composing around that. Actually that was part of how I met Paul Slovic. We did a performance of the music at an event up at the University of Oregon, and we could sort of see how it’s a great awareness tool, but by itself could be de-motivating because it focuses on how much the climate is changing which can makes us feel powerless.

So there was this recognition that the music has all these different functions. It’s really been this process of serendipity and grabbing opportunities. There is an Argentinian named Emiliano Rodriguez Nuesch, who has a company that does creative risk communication. He searched me out about ShakeOut back when I was still with the USGS and—do you remember Pablo? (Jean-Paul Ampuero). He was a professor at the Seismo Lab—

Aspaturian: I do not know that name at all.

Jones: He ended up leaving. He's in Nice now [Géoazur Laboratory]. But Pablo and Emiliano and I ended up doing these earthquake resilience workshops in East LA, and then I connected Emiliano with women that I'd been working with in Japan on tsunamis, and he created this amazing video. He had started out in advertising and ended up getting into environmental issues; he's been funded by organizations like the World Bank and the UN. He was part of the International Decade for Disaster Reduction [1990–1999].

So as I was starting with this music, he came and approached me about a video he was doing for the World Bank on using music for action. Half of it's on me and my work in LA, and half is on a woman in Haiti that he was funded to work with—this young composer creating music about hurricane preparedness. So when US–Japan Foundation approached me, I ended up creating a proposal with Emiliano to create Tempo, and they funded us. It's this crazy interdisciplinary project—how do you even categorize it? It needs to be bicultural, and so we're doing this with Japanese and Americans musicians, social scientists, and physical scientists, and we've struggled to find physical scientists in climate change in Japan to connect with it all. The cultural issues around climate change are really different and tied into bigger cultural issues like how you see your relationship with nature—

Aspaturian: Yes, of course.

Jones: There's also nuclear power and all of those issues in Japan because of the Fukushima earthquake. There's all sorts of interesting things culturally—the role of rhythm within your music. This year we are focusing on what we're calling Tempo dialogues. We have every month a meeting with a musician and a scientist on some shared topic. The next one will be a presentation from a British psychologist. Her PhD is actually in ecology, but she ended up getting into the psychology of climate change.

Aspaturian: Sounds ideal for your purposes.

Jones: Oh, she's fantastic. Talking about hope, and how disempowerment will keep us from acting, and how we need to have hope that our actions can make a difference. And we'll also have a Canadian choral composer who's written a sequence called the Quintessence Project on a journey to hope as things come apart. They will have a discussion on how the music and the science are related and then move into a discussion with the participants. We're doing one of these every month. We have a YouTube channel with all these videos.

Personal & professional plans going forward

Aspaturian: What do you see the center doing over the next few years? Particularly as we enter what we hope is a quasi-post-pandemic era?

Jones: I'm trying to figure that out right now. My husband is also on a path to his retirement; a couple-years' phase-out. He's basically asking, "Do I want to take on a consulting job?" I also do consulting. An 18-month-old grandson—

Aspaturian: Another one on the way.

Jones: Another one on the way—and how much time do I want to be grandmother versus how much time do I want to do this? How much do we want to spend trying to find the funding? I think Tempo is the one project that I really want to keep going.

Aspaturian: That is your priority at the moment.

Jones: I'm passionate about it. My goal is to be able to create the Tempo Festival and maybe be part of some other festival, but with prizes for music that fit the psychology aims. US-Japan Foundation has been able to fund us to keep this going, and we still need to find the funding to do the prizes and the festival; that's sort of my biggest priority right now.

Aspaturian: I see the physical sciences, social sciences, and now the arts are coming into play as well, in terms of your overall agenda.

Jones: I suppose you can evolve through life creating a taller, narrower silo as you go through, and you specialize—

Aspaturian: Or you can go in the opposite direction, which is what you seem to be doing.

Jones: I seem to be— the science is important. I'm a scientist. You can't do this without the science. You can't deal with climate change without the science. But we also can't do it with just the science. We've shown that horribly, dramatically. But also my music—I'm very much a classical musician. We joked that once I discovered the 17th century, I never saw a point of coming back to the 20th. I love that music, and I also realize it's niche. I have to spend a lot of time explaining the instruments and the style for people to listen to my music and hear it as science. I'm actually going to be giving a talk in a little over a week where I'm going to try to do this for the first time—combining the climate science with a performance and seeing how it goes. If we really want to change opinions, I think we may need country musicians. We need music that speaks in the genre that people are listening to. And actually, I like country music quite a bit, but my connections tend to be more classically oriented.

The other thing is that choral music is a participatory thing. And since we're trying to create community, I think in the first round we're talking about really focusing on choirs. But again, I've got to find some more funding to keep it going. I think we can do this. I've got to figure out how to get out there and "sell" it. And—do I have the energy to go out there and sell it? There's still a bit of a debate about that with myself. But I also think that if I could again get support for really doing those policy workshops for scientists, that is something where I felt like I was really making a difference, and where I have a unique perspective that's useful to share. And while I really like what we're doing here with Caltech with the earthquake fellows, that's not something that's going to keep me involved. We'll set it up; we're going to write an evaluation at the end of the year, and then the lab will continue with it. You know, I took a government job in the first place because I really hated writing proposals.

Aspaturian: And the irony is—

Jones: Right. Here I am, right back there, needing that part that I don't do really well. And yet really trying, because I've found myself in a place that doesn't fit any of the traditional boxes.

Reflections on legacy: science & public service

Aspaturian: Taking all of this into account, how do you conceive of your legacy, or how would you like it to be thought of? It's a little early, I know, to ask that question in terms of your career, but—

Jones: Not necessarily. I am 67 years old. [Laughter] I could stop right now and have had a successful career—and what would be the legacy? I guess that's why I haven't stopped yet. My legacy that I would like to see is some place where the science activation, the science translation, not just science communication, is actively supported. I can at least leave the legacy that it's needed. It would be great if we could find some way to get it established and going. That's probably the most important thing. Which maybe should tell me something about what I am going to do next and what my center's going to do. If that's the legacy I want, I should be trying to find funding for getting the science activation going. We really dropped it because of the pandemic.

Aspaturian: Of course.

Jones: You can do some of this online but there's that whole thing of how do you have a real bilateral exchange; it's almost impossible to do that online because it's not imparting information.

Aspaturian: Not everything works over Zoom.

Jones: Yeah. But I'm really not sure where the funding would come from. Society's changed a lot over these last two years. I'm still trying to figure out where science activation can fit in the larger scheme of things. And if I could get the climate anthem written by somebody, that would be a great legacy, too. I think we need music that pulls us into community, demands action, and inspires people. We even know—that's quite an interesting part of this—what would be the characteristics of music that would work that way. It needs to be pretty.

Aspaturian: Yes, melodic.

Jones: Melodic because melody gives us joy, and we need to want to be part of this. We're not going to feel happy about climate change, but it needs a sense of joy, which means a beautiful melody. It needs strong rhythm because rhythm is what moves you forward. It gives you the oxygen. You want it to be something you can chant, that you can be singing as you march down the street. I think the words need to be simple so that it can be sung in multiple languages. And then it needs a message of hope—but. The problem with hope is that it makes you feel like somebody else is going to take care of the problem." It can't be that message. It has to be one of demanding action without being too angry. So I can say I know what the characteristics are. Now to find the musician who can write it.

Aspaturian: Listening to you—melody, rhythm, hope, voices —I'm thinking gospel.

Jones: Oh, yeah, there's a lot of that. That's probably going to be our next dialogue—looking at the role of music in the civil rights movement as an inspiration of what this could be.

Aspaturian: Yeah, I think you're on to something there.

Jones: Yes. It needs to be something that's not just for us classical people. I can do a really good job of writing 17th century style imitative polyphony. But that's—

Aspaturian: Probably not what's indicated. How about the role you've played in making the Southland and the city of Los Angeles a safer place for people to live and work?

Jones: That's done, if you will.

Aspaturian: But it's a *legacy*.

Jones: It is a legacy. It's a legacy. I know Mayor Garcetti talked about this and said people will be alive after the [San Andreas] earthquake because of what I've done.

Aspaturian: You won an award, I think, in connection to this in 2015.

Jones: A whole bunch of awards, actually. I got the Ambassador Award from the American Geophysical Union. It's funny—I think I said this earlier [*Session Seven*]—I made the decision to walk away from science competition, but then because of this I got the Ambassador Award.

Aspaturian: You got the accolades anyway.

Jones: And that included being a fellow with AGU. And I got the Service to America medal, which is for federal employees; there are seven categories and then one overall best, which is Federal Employee of the Year. One other US Geological Survey person [Paul Hsieh] won it, and in 2011 he got the Employee of the Year, too, for developing the algorithms to stop Deepwater Horizon—you know, when they capped it. He did the science to convince them to keep it capped, that they were actually doing it correctly. [Deepwater Horizon was an explosion and massive oil spill that occurred off the Gulf of Mexico in 2010. *–Ed.*] Stopping Deepwater Horizon, that's a big one. I didn't get the overall award, but I got the service award, the same year that the team that had fought Ebola got the overall award. Last year, Dr. [Anthony] Fauci got it. So it's like, yeah, it's sort of the ultimate accolade within the government.

Aspaturian: Yes. And some outcomes like yours are not immediately obvious, either.

Jones: Actually, I think the retrofitting ordinance was passed the week they gave me the Service to America medal. So it all fit together. And I got the Distinguished Alumni Award from Brown.

Aspaturian: Understandably.

Jones: I love it because it was actually the William Rogers Award, which is given for fulfilling the Brown charter—you know Brown was chartered back in the early 18th century—of living a life of "usefulness and reputation." It's a very 18th-century framing of it.

Aspaturian: And yet it resonates today.

Jones: It does. So I got these awards, and it feels like I accomplished something there. And it's an ongoing thing; we still have communities taking this stuff on. And I could put all my time into getting the rest of them there; I just feel like I can't when I face climate change. Climate change is so much bigger and so much more damaging. And you know, the solutions are there.

Aspaturian: They are indeed.

Jones: We know how to do it.

Aspaturian: That is what is so frustrating and maddening. Probably requiring an Apollo or Manhattan project in scale. Yes, that's right.

Jones: The thing is, it's also global.

Aspaturian: I was going to say that too.

Jones: And that's the difference between it and, say, the Manhattan Project. Americans don't cooperate really well. A project that we do alone to prove that we're the best; we like those things. A project with everybody else, to do together for a solution for everybody? That's a lot harder to motivate Americans to do.

And maybe music— I'm not going to contribute to the science for climate change. My son is, and I'm really proud of that. Maybe I can contribute some to the policies: I think that training climate scientists about policy interaction—that's a really useful legacy. And then the music. If we can get some music that helps motivate people and moves them in the needed directions and conveys that we can't do it alone. Social science is incredibly important here, and I think that's one of the things the physical scientists need to learn. We don't make decisions about risk for logical reasons. Alright—we can support the decision-making process with the logical reasons.

And if you're really a scientist that lives with the numbers, you can get to an emotional commitment from the logical reasons—

Aspaturian: From the data.

Jones: But it's that emotional commitment that is necessary to make us act about risk. And that's the fundamental thing I'm taking away from the social science research. That's why I turned to music because that's where people do generate emotion.

Aspaturian: True. Shall we stop there?

Jones: I think that's enough. [Laughter].

With Egill in Washington, DC, just after receiving the Samuel J. Heyman Service to America Medal in October 2015. *Photo courtesy of Lucy Jones*

Session 10, July 21, 2023

"I wanted to write a book that shared insights I've gotten from talking to so many people about disasters"

Aspaturian: Recorder's running; here we go. Today we are going to talk about the book you published in 2018: *The Big Ones* [*The Big Ones: How Natural Disasters Have Shaped Us (and What We Can Do About Them)*], about how natural catastrophes have shaped human destiny and how humanity has reacted to them in a variety of ways. Would you like to describe the book a little, beyond what I've just said?

Jones: Okay, sure. I organized the book somewhat historically, where I looked at a variety of catastrophes in human history and used the stories of those catastrophes to explain how we as human beings think about disasters. With each one, I could pick up one particular aspect and develop it in more detail. I knew I wanted to write a book that shared some of the insights that I've gotten from the process of talking to so many people about disasters and from the various things I've tried to help people with. It actually began because of an article about me in the *Smithsonian* magazine.

Aspaturian: What year was this?

Jones: That was 2012. It was a really lovely profile done of me. Also, some good pictures. After it appeared, I got a call out of the blue from Farley Chase, a literary agent, asking me if I ever thought about writing a book. And at some point actually he said he always was looking for authors that could stand up to Stephen Colbert [talk-show host: *The Late Show with Stephen Colbert*] [Laughter]

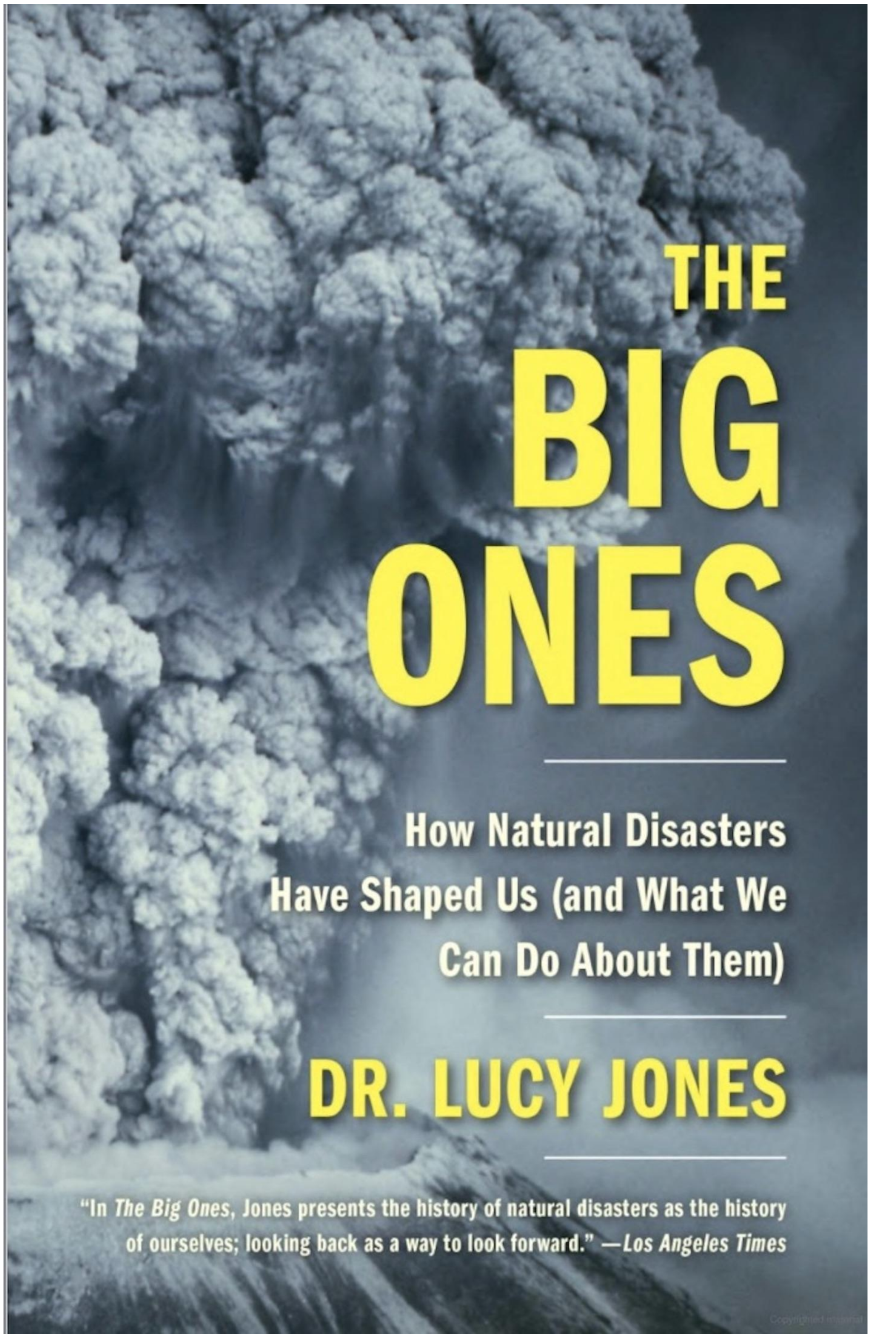
Aspaturian: That was his criterion.

Jones: That was a factor that he looked for, and that's what struck him about the *Smithsonian* article. And we talked for a while, and there was also the idea potentially of doing a book focused on "disasters I've known"— you know, the personal side of the disaster stories. I talked with him for quite a while, but then I realized that if I wrote it as a government employee, the government would own the royalties, and that didn't seem worth it. So I put it off.

But once I retired from USGS, it seemed like a good time to think about trying to do this. I called Farley back, and he was still interested. And so, a fascinating process—looking back at where I was before I started to where I ended up with the book. I think our book proposal was called *Acts of God*. Or *Acts of God and Acts of Man* or something. I was thinking of some sort of—

Aspaturian: A bit biblical.

Jones: Well, more the idea that *Who can know what will happen?* "Acts of God" are not covered by your insurance policy. I didn't actually come up with the final title until after the book was completely done. I had reservations about calling it "The Big Ones" because sexual jokes get made out of the title. And I've always been so irritated when talking about "the Big One," as if there was only one big one. But we ended up being able to pick that up and turn it around to *A big one's one that changes society.*


THE
BIG
ONES
How Natural Disasters Have Shaped Us (and What We Can Do About Them)
DR. LUCY JONES
"In The Big Ones, Jones presents the history of natural disasters as the history of ourselves; looking back as a way to look forward." —Los Angeles Times

"The challenge for me coming out of science was, How am I going to write 70,000 words?"

Aspaturian: It's certainly a phrase people relate to easily.

Jones: Right, right.

Weaving scientific, sociological & psychological perspectives into a historical narrative

Aspaturian: How did you decide what to focus on with your various chapters? There must have been dozens of possibilities.

Jones: There were dozens of them. The Maule earthquake in Chile [2010] and the Christchurch earthquakes in New Zealand [2011] were both ones that almost made the cut and didn't quite. I really wanted to have a variety of disasters. What I ended up doing, starting with Vesuvius and Pompeii, is putting them in chronological order, which also then let me develop the theme of how the human response and the societal response has evolved through time.

So at Pompeii, it's an erupting volcano [79 CE]. Which means I get to explain a bit about how volcanoes work. I put in some of the basic earth science—not the complicated stuff, but the type of material you'd be getting in a college geology class and doing it in a way that should be understandable by people.

Aspaturian: And you also are able to bring in the scientific perspective, as it was handled in antiquity.

Jones: Right. So it's a combination. Here's the physics— how it happened scientifically. I look at some of the history of what happened with each of these events, and also at the social history—how people at the time saw this. So, for example with Pompeii, I ended up really researching the old myths about Vulcan. One of the things that happened in that era was a Vulcanalia—a summer festival held in honor of Vulcan, Roman god of the fire. The timing of these was basically tied to the really hot times of the year when fires would occur; and so that was a way to bring up how that society thought about these fundamental elements of the natural world and work that into the overall history of the disaster. And I usually tried to have some person that I could focus these events around, which made it easier for me to tell a story

Aspaturian: Which in that case, I think, is Pliny the Elder?

Jones: The Elder. And his nephew, Pliny the Younger, and a little of their history. Who is Pliny the Elder? Well, he actually wrote what's considered the first encyclopedia. And, you know, this is one of the most amazing things in the world: I sat at my desk, at home, because I had just retired from the government. And I had Google, and I could find all of Pliny the Elder and Pliny the Younger's writings already translated and just sit there and read them.

So I found doing the research incredibly interesting, because I got to indulge the history part, which had been an interest of mine from college—that whole side of things. And also develop the psychological side of it. There's been a lot of psychology research on why people think and react the way they do about risk—natural disasters and the uncertainty about them. And then tying all that in, to some degree, with religious history, because since we're so afraid of random events and don't want to believe they're random, most human societies have traditionally put the blame on God or, you know, on some sort of spiritual thing that can't be tested.

Aspaturian: Right. Religion is an effort to grapple with these events.

Jones: To grapple with it, yes. I actually sat and read all of Saint Augustine. And I wrote about how he thought about what were called "natural evils."

And so for me it was a process where I got to take all these things that I used to really enjoy doing—before I ended up just focusing on science—and to try and weave them all back together into a story for each of these events. But, to get back to the question of how I chose my events: There were some I knew I wanted in there. I knew I wanted Pompeii because I needed to spend some time looking at the evolution of the original Judeo-Christian traditions about disasters. I knew I wanted the Lisbon earthquake [Great Lisbon Earthquake, 1755] because it was so fundamental in happening at the boundary between the Inquisition and the Enlightenment.

Aspaturian: Yes, it's on the cusp there in a way.

Jones: Well, it helped push things over. I mean, outside of Portugal the Enlightenment was well under way. In Portugal it was really held back by the Inquisition. And the way in which the Marquês de Pombal, Portugal's prime minister, used the earthquake to break the power of the Inquisition—that was something I learned as I did my research. I also wanted to write about the quake because of the very famous Voltaire poem on the *Disaster of Lisbon*. [*See also Session Five*] I guess it was the early ones where it was easy: I knew I wanted to put the Laki volcanic eruption [1783] in there, because it is the eruption that killed a large percentage of the Icelandic population and really changed Iceland.

I had read a bunch of books about it, and of course I had the family connection too. It was the greatest natural disaster in the history of humankind; it had a global impact that probably killed at least six million people worldwide. And I could also talk a bit about climate change with that one. And then with the later times—well, I skipped the 1906 San Francisco Earthquake. Partly because the biggest disaster in California history is not an earthquake. It's the Great Flood of 1861–62.

Aspaturian: Which nobody remembers, as you point out.

Jones: Right. And so that allowed me to draw that contrast. I had this idea, though, that for each disaster I needed to talk about the science along with it. Having a dozen earthquakes wouldn't really work, and so I look at different parts of the earthquake problem—for instance, what it means to be directly on top of the earthquake. I really focused on that aspect in the Japanese earthquake in 1923 [Great Kantō Earthquake], which allowed me to talk a lot about the beginning of seismology, because it really began in Japan in the late 19th century. But 1906 didn't give me so much to add to the history. It's already been really well done, and I didn't have a good story out of it.

I thought about doing it around [Grove Karl "GK"] Gilbert. He was one of the geologists who organized research teams to study the San Andreas fault after the earthquake, and that work was funded by the Carnegie Institution [for Science]. There's a report on the 1906 earthquake, published in 1910, where they first map the San Andreas; there's a lot of interesting material in there, but I just didn't feel like it was enough to stand alone. And it didn't add to the psychological development in the way some of the others did.

Aspaturian: Well, I mean, obviously you wanted to have an international spectrum, and I think the flood story in California is probably in many ways more evocative and revelatory than revisiting the San Francisco earthquake would have been.

Jones: That was really what I felt, yes. And Farley was, without question, really pushing me to include more international cases.

Aspaturian: That was a good insight on his part, yes.

Jones: And in fact, the first translation of my book was into Portuguese. Which I'm sure is because of the Lisbon earthquake. [*LJ subsequently added:* "I understand why some of the translations happened. The Korean translation was second because I told the story of the Korean massacre after the 1923 Kanto earthquake. Japanese and Spanish aren't a surprise either. The Chinese translation took out the chapter on the Tangshan earthquake because what I said was too different from the official version of history, but I ended up agreeing because otherwise there wouldn't be a Chinese version. But I still am trying to figure out why there is an Estonian version."]

Jones: There was one point in my first draft where I was working on the Hurricane Katrina [2005] chapter as just this reflection of what happened in the 1927 Mississippi floods. I did this intentionally to make the point that, 80 years later, all the same things still happen—the same abuse directed at the most vulnerable members of society; the same racism. Still African Americans being confronted with guns. By then I was already working with the editor at Doubleday, and he's like, "There's not enough new here. Maybe we should just fold Katrina into 1927." And I said, "Well, I can't. It's my only hurricane." With all the science about how hurricanes happened. But it ended up as the only chapter I completely rewrote. I shifted the chapter to look at the issue of blame and at the need to blame somebody for what goes wrong and the harm done in disasters. I focused more much on that and less on the lack of social progress.

Aspaturian: Yes, that does add a new dimension. You talk about blaming those in authority and—

Jones: Blaming the victim.

Aspaturian: And blaming the victim. Both.

Jones: And that allowed me to develop these chapters in a different way. I almost did the Christchurch quake, but I was so tired by that point. [Laughter] The other earthquake I thought about was Maule. I had actually gone down to Chile with the Red Cross delegation after it happened, and there were some really fascinating social things in there about how people thought. One of them was, they have no professional firefighters.

Aspaturian: Anywhere in Chile?

Jones: It's all volunteer, and they're emphatic about it: How could you possibly entrust your community to somebody for whom it was just a job? That instead, it's the members of the community who are passionate about doing this. And we're like, *huh*? [Laughter] But that was a really strong belief. They just thought professional fire fighters were an appalling idea.

Aspaturian: That would have been a very interesting cultural thing to write about.

Jones: So that was one I really thought about doing, but I had so many earthquakes at that point.

Aspaturian: Did you work with a researcher at any point, or did you do all the research yourself?

Jones: I did it all.

"I had to change the way in which I talk about my subject"

Aspaturian: Were you working on anything else while you were doing this? How much time did this take for you?

Jones: I wrote it in a year, and it was the majority of my time, but it's also when we started the Lucy Jones Center. [*Session Nine*]

Aspaturian: When you say "wrote," are you including all the research in that time frame, or was that done prior to some degree?

Jones: No because I would pick an event, and I'm doing the research as I'm writing about it. When you're doing the book proposal, you have to do an outline with about one page on each chapter that you're going to do. So I had written that out, and as part of that process my agent and I would be talking through which events to be doing. The challenge for me, coming out of science, was that—you know—you write two or three papers a year. You do your research and then you sit down, and you write it right then, and it's not very long. It's ten pages. How am I going to write 70,000 words?

Aspaturian: And you had to turn yourself into a storyteller as well. Science—

Jones: Right. I had to change the way in which I talk about my subject. On the other hand, in my talking, my public speaking, I sort of naturally, instinctively go to more stories. So that part was not as hard as facing the 70,000 words. It just was overwhelming. So doing this chapter by chapter where I'm only writing, you know, 30 pages at a time, I could focus on producing those 5,000 words. Not 70,000 words, right?

Aspaturian: Also, each is a self-contained case study, which must have been somewhat familiar to you. coming out of a scientific milieu.

Jones: Right. So to start, I had this sort of case-study idea—and obviously, looking at way more than the science. I knew I wanted the science, and I knew I needed a person within each event to focus on. Partly because the role of the individual gives that feeling of empowerment—that you *can* make a difference. All of these stories of people who made a difference—for instance, the Marquês de Pombal or Pastor Jon.

Aspaturian: He's the one who led his congregation to the higher ground?

Jones: No, that's in the California flood. This is the guy in Iceland, the one who wrote the description of much of the Laki eruption and really saved his community. So you've got these different stories where an individual really made a difference. In some of them, I don't emphasize that so much. But I did in some of the chapters.

ASPATURIAN: In others it seems that you're studying—looking more—at the general sociology of the event.

JONES: Well, even in the Japanese one, I did spend time with a seismologist that we knew about: The story of the old seismology professor [Fusakichi] Omori being on a ship when the earthquake that he basically said couldn't happen—did happen. And it killed 140,000 people in Tokyo. And I had heard a story that he had committed *seppuku* because of this disgrace. It's the sort of thing that gets passed around—and so I wanted to capture that aspect. But then it turned out he didn't commit *seppuku*. So I described the fight between Omori and Imamura over whether the earthquake was possible but didn't rub in the defeat of Omori at the end of the chapter. My editor convinced me to end with the Korean massacre. So dividing the book up into those chapters helped me manage that aspect.

I think the hardest part for me was focusing on how to create the long book. I've thought about writing another book, but I'm back to facing the same thing: How do you do it without your discrete chapters to put the pieces together? That to me was the hardest part with this one and having the stories and the individual events to work with gave me a way of doing that.

“The need to protect ourselves in the face of disasters helps let cruelty out”

Aspaturian: That created the narrative flow-through for you. I will say that from my standpoint, I found the most interesting disasters to be the Yokohama [Kantō] earthquake, the collapse of the Mississippi levees, and the Sumatra quake and tsunami [2004]. Yokohama and Mississippi because the targeting of scapegoats was so vicious and so violent in both cases. Very different scapegoats, but both reflections of how the cultures behaved; and with Japan in particular, the counterpoint between the sophisticated advances in earth science, on the one hand, and this outburst of primitive barbarism against the local Korean population, on the other, was just astonishing. And it’s all there.

Jones: The Korean massacre after the earthquake is horrific, right? Six thousand Koreans just slaughtered by their neighbors. But we did emotionally the same thing with Hurricane Katrina, here in the United States.

Aspaturian: It happened in the Mississippi flooding too. No mass slaughter exactly, but there was a lot of violence.

Jones: Well, African Americans were forced at gunpoint out to work on the collapsing levee.

Aspaturian: That’s right. Indirect murder.

Jones: Digging into what happened in Mississippi, I found that there's a great book specifically about that, *Rising Tide*. And I quote from it a lot in that chapter. I didn't want to plagiarize though, but I think that was my main source for the history I wrote about. It gave me the through narrative, and then I really dug into Herbert Hoover and his role.

But the challenge always was, How do you organize it? Do I organize it by ideas I want to develop? But then I realized that there was a historical aspect to the evolution of social thought that allowed that narrative to develop in a natural way chronologically. It also made sense to think of it that way. Once I did that, you've got the 1923 Kantō earthquake and the 1927 Mississippi flood right next to each other, and that juxtaposition really felt, emotionally, like the low point of the book, because those were the events where you really look at the evil.

Aspaturian: You look at an evil, first with a natural catastrophe and then with a related human catastrophe.

Jones: To the degree I did history in college, it was Chinese history, right? And now I was really looking at American history and seeing that we might want to pretend we don't behave like this, but we do.

Aspaturian: We do, and every culture has its scapegoats.

Jones: Every one. We're not alone. And I think it was actually really important to emphasize that this is a cultural issue. This isn't saying that America is uniquely awful in treating the African Americans so dreadfully after a natural disaster. The Japanese did it to the Koreans. We all do this to "the other."

ASPATURIAN: Yes. To take another example, 1000 years or more of anti-Semitism in Christian Europe. I mean, same dynamic.

JONES: Right, right, right. We do that, and it isn't unique to any one culture. The need to protect ourselves in the face of disasters helps let the cruelty out. And so it was with this sort of narrative arc that I felt like I went down into the pit, and then started coming back up from that. And then, because I was doing it chronologically, what comes next is the Tangshan earthquake [1976], where I was able to turn more to my own experience. That's when I started getting personally involved—when I was alive and had some direct experience with the disaster.

ASPATURIAN: Yes, and that chapter—a lot of it—dovetails very closely with what you talk about in your oral history as well. [*Sessions Two and Three*]

JONES: Of course, by the time we're having my oral history interviews, I'd already written the book, which had been a process for myself of understanding the experiences I had had. It was research into my own life, if you will. Writing those personal experiences down got me to clarify how my work had affected me and what I thought about it. So I found it a time of personal growth to write the book, as well.

“Natural disasters & their impact happen in geologic time, which is very different from human time”

Aspaturian: I also thought the chapter on Sumatra was particularly interesting because it involved several things you talk about also in the oral history: The globalization of disasters and how that finally enables us to relate to these events in real time, even if they’re not happening locally, and how important it is to have that ability.

Jones: Right. You can see how one of the basic themes throughout the book is that we have trouble remembering. I guess I really bring up that idea first with the big California flood—that if it’s not within our grandparents’ lifetime, we don’t remember it; it is no longer real. And that natural disasters and their impact happen in geologic time, which is very different from human time. And so we have real trouble understanding that the worst can happen *to us*. And then the idea in the Sumatra chapter was, now, when we’re experiencing it globally, *space can take the place of time* to let us see what a really bad disaster is like.

You’ve got to be able to transfer these events from there to here, but when you have the pictures, when you can see and hear how people are affected—it’s not just globalization, but communication—we can more viscerally experience these things. And now of course, we’re seeing how these disasters are becoming more common because of climate change. Not the geologic ones, obviously, but the meteorological disasters. Watching what's been happening just this summer—well, we've always said that it's the natural disasters that are really going to be the first sign of climate change.

And this is one of the things that's really bothered me about how we often talk about climate change: We talk about, you know, the polar bears, and the coral reefs and rising sea levels. And if you don't live on the coast and you don't live in the Arctic, it becomes somebody else's problem. It's not imminent. And all of that social science research on risk perception tells you that this lack of personal engagement is going to make it seem not that bad. And yet, the first time I really heard about climate change 30 years ago was in a talk about how it's making disasters more common. It was in a presentation at the Board on Natural Disasters, really looking at what global climate change is going to do to disasters.

That reality is much more in our space today, and you can see it now—how the wildfires in the last few years in California have changed how people are thinking about climate change. And now we've got Canada burning down, and all of the East Coast dealing with wood smoke from those wildfires. And it's making the disasters more personal.

Aspaturian: It's bringing them home.

Jones: It's bringing it home. It's changing how people are talking about climate change.

Aspaturian: Finally, and hopefully, not too late.

Jones: Well, it's never too late. It's already too late to prevent it. Never too late to reduce it. And the more we do it, and the faster, we do it, the more difference it will make. Which is why, that's what I'm doing now.

"Telling the stories instead of giving a scientific talk"

Aspaturian: How do you feel writing *The Big Ones* affected your role as a communicator? You had to move yourself into a new mode of expression, I think, in producing this book.

Jones: Yes, and also when I did the book tour. Going and giving talks about the book required me to tell the stories, instead of doing a scientific talk.

Aspaturian: Non-scientific audience for the most part.

Jones: Well, I've already been talking to nonscientific audiences for a long time. And when I started down that path, it actually felt really radical at the time; now [laughter] it looks like a baby step. When I was working with the mayor's office in LA, I put together this talk about what the San Andreas earthquake would really do to Los Angeles, using ShakeOut. So it's all that science, all properly done science.

And I titled it "Imagine America without Los Angeles," [*See also Session Nine*] and that just seemed like this radical step. It was such a provocative title and, you know, it wasn't the straight science. And as I did those talks, and of course I had to do a lot of them, I started picking up—getting that feedback—on what people were responding to, and I started to develop more of that story aspect of it. I discovered when I was working with businesspeople in LA, at a majority of these talks, that ending with a graph showing the GDP of New Orleans compared to the GDP of Nashville before and after Katrina was very effective. These are comparable communities, about the same size and just a few hundred miles apart. New Orleans' GDP is a bit larger than Nashville's until Katrina happens. It drops down; Nashville's continues to grow. After Katrina, in seven years, the city loses over 100 billion dollars and that—

Aspaturian: That they could relate to.

Jones: I'm putting those concrete statistics up there, and my scientific colleagues are saying, "Oh, that's anecdote. There's this reason and that reason." Right. But the GDP comparison tells a story that my audience can *get*. And I could see how ending with that piece and *leaving that slide up for the question-and-answer period—woof!*—was effective. So I already was going through this process before writing the book—but. It's a really frustrating thing that as scientists, we all know the famous line, "The plural of anecdote is not data." And that anecdotes tell you what happened in one place; they don't tell you what's really true. They don't let you ask "why?" which is the big scientific question, right? And we are taught to really distrust anecdotes, as we should. But then having to face up communicating that science, you go back to the anecdote. And so to maintain my own personal integrity, I need to make sure there's also the science that is not the anecdotes. I want to be sure to provide the proof that this is the case, but then I give in and use the anecdote to help explain to people.

And so I'd already sort of grappled with that issue, because let me tell you, it bothers me. I'm enough of a scientist that I listen to and agree with that argument that one case doesn't tell you whether it is causal or coincidence, correlation or just random. And yet that's how you have to communicate it to the larger public. And so I'd already been there. And then the process of doing the book was not only facing up to that disconnect—almost cognitive dissonance for a scientist—and grappling with it, but really using my experience with it to create these stories. And I always made sure I put in lots of footnotes showing the data that backed these stories up. Not that almost anybody ever looks at footnotes, but they're there for my own personal integrity.

Aspaturian: What kind of reception did the book receive?

Jones: It's all been very positive. It was an Amazon Science Book of the Year and a couple of other things.

Aspaturian: I saw that. A lot of coverage in media.

Jones: Lots of media coverage. I still haven't gotten back royalties exceeding my advances.

Aspaturian: I don't think that's uncommon, unfortunately.

Jones: It's partly evidence that I had a really good agent who got me some pretty big advances. So it's hard for me to say exactly where things stand with it now. I won't hear about it for a while, and I'll be thinking that it doesn't have long-term longevity, and then—I've just been asked to give a talk to an insurance conference in Bermuda—tough life, right?— and they're buying everybody a copy of my book. So yes, it feels like it's making a difference. It had a good reception. It was *a lot of work*. I keep thinking I want to write another one, and I'll have various ideas.

I did start my first project with Southern California Association of Governments at the same time I was writing the book. That was going out and basically taking "Imagine America without Los Angeles" and *Resilience by Design* to various cities. But I didn't have to do a new presentation for each of those. So I was doing that in between working on the book.

Aspaturian: It was a module that you had already developed and had used.

Jones: Right. That thing was already done. And they wanted me to take it around, so it didn't take up that much of my time. I would say at least three-quarters of my time went into the book, basically, for a year.

Aspaturian: Have you always been a person who gets along on less sleep than perhaps most of us?

Jones: It's funny; I used to be.

Aspaturian: I have discovered this is true of a lot of Caltech people.

Jones: I bet you it's connected a little bit to our spectrum issues. Actually for me, I do have sensory disruption. Not to the extent that puts me on the spectrum, but I have it, and most of the people in my family do. And, therefore, getting to sleep is always really a challenge, and I would often lie in bed thinking things through. And so part of my mental processing happens in that way. When I was young, my parents discovered that the only way to get me to sleep—and I mean, this is, like, as a one- or two-year-old—was playing a record of Gregorian chants. Maybe that's why I like my early music. [Laughter]

Aspaturian: There are worse things than those chants.

Jones: So when I hear Gregorian chants, I want to go to sleep. But I think that's reflective of the sensory problems that I have. Most of my life I never slept much. I still slept six hours a night—I'm not one of these three-hour people, which I think is just bizarre. I am finding in my elder years that I'm sleeping more now than I did as a teenager. But when I was writing the book, there were times that I would wake up at three in the morning and need to go to my computer to get something down because it would finally become clear.

Aspaturian: Yes, that's very common while doing writing.

Jones: You know, it's sort of funny. That it's only now in my 60s that I'm sleeping eight hours a night.

Writing the book "changed a lot about how I talk about disasters"

Aspaturian: Did you feel, writing this book, doing the research, and delving into these events that you probably had not known a great deal about before, changed you or changed your outlook or changed the way you think in any way?

Jones: Not a transformative change.

Aspaturian: No, I would not imagine.

Jones: But the processing of that information, clarifying it, making sense of things, and being able to put the patterns together changed a lot about how I talk about disasters. And that, I think, really led to Tempo. [*Session Nine*] Because it made the whole risk perception aspect of it and understanding the research on risk perception so much more relevant. Writing this involved really delving into what risk-perception science was and creating a bit of an integration of these psychological insights.

I'm actually trying to write a paper right now that applies risk perception based on all these psychological studies to work out exactly how these psychological patterns affect how we think about climate change. So there's this fundamental principle of how we respond to randomness or to our need for self-efficacy: You only do things that you think will make a difference. You've got to believe that your actions will work before you put any effort into doing them, which is a fundamental human thing. That's why, you know, representation matters. Because you need to be able to imagine that your actions will affect the outcome. What does that mean for climate change? How do you believe your individual actions could possibly make a difference on this global issue? And I think it's a big part of keeping people from acting.

ASPATURIAN: It just seems too immense.

JONES: Right. And that's why the Tempo project and our communication with musicians is focusing on the personal commitment of electrification; the idea that if we all switched over to electrical appliances—not even getting rid of our current gas ones but buying electric as we need to replace them—we could meet our emissions goals. Just doing that one thing. Maybe that doesn't solve *the problem*, but if we meet our emissions goals, it would be huge, right? And that's, *oh, I could do that. And I could talk to my family about them doing it, too. And I could make a differ-*

ence. And so that whole thing, of how to integrate all of these research studies to apply them to this process of thinking about climate change and encouraging action—a lot of that happened through writing the book.

"It's integrating the science & policy-making that we've got to figure out in society"

Aspaturian: I think you must have become more aware of how important the human element is in all this.

Jones: Oh yes. We've had plenty of discussions of silos and disciplines and interdisciplinary work and all of that. It's back to that anecdote/data thing. You need the science; science is the external fact checker, right? It's fundamentally human to make patterns, but we don't have the internal fact checker. So we can make these patterns up, and if a pattern makes us feel safe, we grab onto it. We don't know if it makes us *be* safe: Science is the thing that goes and determines whether or not the pattern's real. We were talking earlier today about the paper that just came out on the Salton Sea thing, right? The geologists see four data points and want to make a pattern out of them.

Aspaturian: Just for the record, this is a recent study suggesting that the aridity of the Salton Sea is somehow holding back the southern San Andreas from rupturing.

Jones: And so you know, you have three data points showing that big earthquakes happen when Lake Cahuilla [prehistoric precursor lake to the Salton Sea] was at high stand. And then, does not having a high stand mean you don't have an earthquake? If you do *any* statistics, of course, it doesn't. This new theory is based on a small number of data points. But we want something like that to be true, and as geologists, we all want it to be true. We all try to find those patterns—we have to have that—but the science—the objective analysis—is incredibly important to discovering whether something—an apparent pattern or a model from that pattern—actually works. But the people making decisions; they aren't doing the research themselves. And the scientists then say, *Well, you should go read the read the research*, right?

Aspaturian: Well, as you said earlier in our interviews [*Session Four*], the scientists make these discoveries, and it is then left to others to determine what is to be done with them.

Jones: Because advising on policy doesn't feel like our expertise, and it's not. I'm not a policymaker; I don't understand all of the other factors that policymakers need to consider. But it's integrating the science and the policy-making that we've got to figure out in in society. Because otherwise, people without the time, or perhaps the inclination, to dig into the science themselves decide what to believe by whom they trust. And since trust is rather famously breaking down in society at this point, we are losing a common definition of what and who to trust. I would like people to be empowered to use their own ability to reason to make these decisions by something more than just being completely emotional. This is something where we could be training children in school. But trying that idea out must feel rather threatening to some teachers. *You're supposed to trust me because I'm your teacher!* It's a bigger problem overall than we're going to solve right now.

Aspaturian: Solutions don't scale up very easily from what you're talking about.

Jones: But we have to find those integrated approaches. So that's why I'm doing Tempo, but there's also the project I'm working right now in Canada where we're bringing together economists, engineers, social scientists, and emergency managers. We're trying to work out what would help people. We know it's cost effective to mitigate against the coming disasters, but we're not doing it.

Aspaturian: This is in Canada that you're doing this? With the government or with a specific province?

Jones: It's the Institute for Catastrophic Loss Reduction [ICLR] at the University of Western Ontario. It's actually jointly funded by ICLR and the university. And they're bringing a team—seven of us—together to come up with a series of papers about what approaches we could take to encourage disaster mitigation that we know is cost-effective. We always have the economists talking about how we just need incentives. Obviously, we need more than incentives. You have to believe that the incentive will make a difference. How do you talk about that? I'm actually doing a lot of the risk perception part of it, using the *Resilience by Design* project as a case study. We've talked about how this was a case where we brought the science in and got policy makers and general society to agree that it was worthwhile to make this investment up front—what made that work? So I'm doing an analysis of what happened back then.

ASPATURIAN: You talk about this as well in our earlier interviews, but I'm sure part of what made it work was that the events you were dealing with were on our doorstep. We were all familiar with earthquakes; this wasn't something that was, you know, 30 years down the road.

JONES: The big issue was, again, that self-efficacy: You know, the earthquake's there, and it's so terrifying you don't want to think about it. Instead of thinking, *Oh, doing this would actually work*.

So part of it is convincing people that taking these actions will be effective. And that's where science comes in. Instead of saying, "it's going to be really bad," we're saying *We're going to use the science*. The science says, "*See? Earthquakes are real. They're going to happen.* What we need is *this investment* that will make *this difference* in the outcome." And that requires much more complex interdisciplinary science, because it's all of the factors coming together.

ASPATURIAN: Not only physical science, but social science, as you outline.

Jones: Yes. Right, right. And engineering.

Aspaturian: Well, we've spent, taking into account the interviews and the back and forth of editing and reviewing the transcripts, the better part of nearly a year, maybe more, on this. I just want to express what a pleasure and a privilege it really has been to talk with you.

Jones: Thanks, Heidi, and thank you! I found myself—when I read the transcripts I was like, "I guess I have had a pretty interesting life, haven't I?"

Aspaturian: I would say so.

Jones: It's really sort of nice to be able to reflect back on that.

Aspaturian: And as you say, if you ever want to write your own memoir—

Jones: I've got an outline for it now.

Aspaturian: You are going to have a rich resource to consult. Thank you very much, Lucy.

Jones: Thank you.