Transcript edited *by participants*

WARNER NORTH: Hello. I'm Warner North, and I have the pleasure of interviewing Jim Matheson about the history of the Stanford Research Institute Decision Analysis Group. I worked with Jim as part of this organization. I am going to try to recollect some of the various events we had, and draw him out on the history of this group, what we accomplished, some of the challenges, some of the areas where perhaps we fell short of expectations.

But first I want to introduce Jim as one of the early pioneers in decision analysis working with Ron Howard. I was a student of Ron Howard's at Stanford, as a transfer out of physics. And I had the opportunity to join the SRI group in 1967. I first want to ask Jim to talk about his background, how he came to meet Ron Howard, and how the SRI group was formed prior to my involvement.

JAMES MATHESON: Thank you. Let me start with my undergraduate work, which was at Carnegie Institute of Technology, now Carnegie Mellon. I was in electrical engineering. And I worked a lot at Westinghouse Research Laboratories, which was nearby. As I was graduating, I met Bill Linvill, who was at Stanford at that time in the electrical engineering department. He was running a program called Engineering Economic Planning.

Bill recruited a couple of people from Carnegie to come out and do the PhD program at Stanford, sponsored by a Westinghouse Fellowship. I went into the program at Stanford and studied electrical engineering, which is the field of my degree. But I was really more interested in engineering economics. My thesis was on teaching better using mathematical models of human learning. There was a group at Stanford called the Institute for Mathematical Studies in the Social Sciences, building models of how people learn things. Some of these models were Markov models, so there were probabilities that you would learn things. Every time you saw a word, what's the chance you'll learn it or just guess it correctly?

People at this Institute were busy comparing models based on statistical fits. And I said, why don't you see which one teaches better? So my thesis was on, if you believe the model, how would you teach? And I found that there were different optimum teaching procedures for each model. This involved dynamic programming of decision making with Markov processes.

It was also an interesting twist with Markov processes having unobservable states. You

couldn't observe whether the student was in the state where he had learned the word or not. You had to infer this state from his answers, which might have been guesses. This made an interesting probabilistic inference problem. So I was pretty well versed in Markov dynamic programming.

In 1963 as I was writing up my thesis, Ron Howard visited the OR department at Stanford, gave a talk on semi-Markov decision processes. Bill Linvill, who was also good friends and a mentor with Ron and also my thesis adviser, said, "you guys need to meet." So we met over a picnic table after Ron's talk, which was nearly incomprehensible. It's a very sophisticated subject, and I was one of the few people in the audience that I think could follow it somewhat.

But Ron's own background, his own thesis, was establishing dynamic programming for discrete Markov processes. So we had that common background in probabilistic reasoning. We sat down to lunch and started talking about decision making and said to each other, "why don't people make decisions this way?" We both came had the same thought that business decisions, personal decisions, all decisions should be made with the same kind of logic we were using in dynamic programming. Not necessarily with dynamic programming, but the same kind of decision making view and engineering aspect of understanding the system, uncertainties and values.

We chatted for a little while, and lo and behold, he was the associate director of the MIT Operations Research Center. And he said, "how would you like a job there? And next year you can start. So when you graduate, come out to MIT." And I said, "will you be there?" You're here at Stanford now. And he said, actually not. I'm taking a sabbatical at Stanford.

So I said, "then I'll go back to Westinghouse. They paid for my fellowship, and I would like to give them some return on that," which I did. Ron came to Stanford for a year of sabbatical as a visitor. And I went to Westinghouse. The group at Westinghouse then hired him as a consultant.

In the ensuing year, which was 1964, he did some consulting at General Electric's Nuclear Power Division in San Jose which was his first major decision analysis. Meanwhile I was at Westinghouse doing my first decision analysis on a new kind of television.

So we're celebrating this year 50 years of decision analysis which dates from 1964, when actually we both did our first decision analysis. Later, Ron published a paper, "Decision Analysis: Applied Decision Theory" defining the field and describing his first application.

So probably the next step in this history is that Ron and Bill Linvill, both originally from MIT, were used to having halfway houses where they could send their students to get real world experiences, such as Arthur D. Little and Raytheon, places where they had good groups in their field. They could send their students for internships, where thye would get good experiences. They didn't have such a place at Stanford, at least in their field.

And so they looked next door, which was the Stanford Research Institute. The Board of Stanford Research Institute at that time was identical with the Board of Stanford. It was set up by West Coast Business to provide a place for research. So they talked to the Stanford Research Institute directors and developed a joint program in engineering economics with Stanford and Stanford Research Institute, now called SRI.

So they started this new SRI group. Then I got a phone call. "Would you like to come out and be part of this group?" And I think I interviewed on January 2, 1966. I remember spending January 1 playing a ukulele for one of the director's kids on New Year's Day. So I took the job. I came back to California in early '66.

WARNER NORTH: I'd like to interrupt to ask you to expand on Bill Linvill and the role he played as an enthusiast for the wider application of systems engineering and associated disciplines, including the emerging decision analysis. And something about Roy Amara who was at SRI at that time, and later an important contributor to futures research.

JAMES MATHESON: I played that ukulele at Roy Amara's house. He was an influential director of SRI. Maybe that got me the job--I don't know. He was very forward thinking and a good friend of Bill's. I'm not quite sure how they met. But Bill Linvill had a history in systems engineering at MIT and then at Stanford. And he was a great believer in systems thinking.

WARNER NORTH: Wasn't it Air Defense Systems in part? I think there were a number of very broad applications of systems engineering. One of these, I believe, was about North American Air Defense. He also had enthusiasm for aiding developing countries such as Mexico in areas such as population control. Willis Harmon was involved at Stanford, and was a very visionary person in terms of education. I think we all learned a lot from the people that Bill Linvill attracted into the Engineering Economic Systems program.

JAMES MATHESON: Yes, Willis Harmon was part of the initial program. There were several projects done initially in this joint program. And getting back to Bill Linvill, he was very passionate that students needed

job experiences before they wrote their PhD. That there should be a place for them to go work under the supervision of somebody he respected, and then return and do their PhD. So he was very much looking for that. And Ron was also looking for that.

So we started this program, and I'm trying to think of what was first. There were several early applications. One was to banking for Morgan Guaranty Trust. Herb Ayres who was a denizen of INFORMS (which was TIMS, The Institute of Management Sciences) back then. Herb was the vice president in charge of management science at Morgan Guaranty Trust.

WARNER NORTH: Would you describe the problem he was working on?

JAMES MATHESON: There were many problems. One was the sources and uses of funds. Let's say, we've got a lot of money today that has just arrived. And we have to decide whether to invest it short term or long term. And the key assessment for this decision is having a probability distribution on movements of the interest rates, to determine whether you should go long or short. So there was a lot of work on problems like that at Morgan Guaranty.

So another early problem-- and I'm trying to sequence these. I'm not sure I can. Two big ones were the space program.

WARNER NORTH: The GE Project: GE wanted to be private contractor for Mars exploration.

JAMES MATHESON: Right.

WARNER NORTH: And asked for your help.

JAMES MATHESON: Yes, GE asked for our help. They wanted to demonstrate to NASA that they had high competence. So they asked us to develop a method for planning a space program. We recruited an intern-- Arnie Pollard was his name -- who worked heavily on that. And that was one of the big initial projects.

But another really big initial project was for the Government of Mexico for how to expand their electrical power system. And I think Warner--

WARNER NORTH: And that was where I came on board. Would you describe the origin of that project with Ron Howard's talk?

JAMES Yeah, I think Ron Howard had given a week-long seminar at the Commission Federal de

MATHESON: Electricidad (CFE) in Mexico. That's the Mexican electrical power system. I'm not sure if it was solely for them or for a group of Mexican businesses. But CFE got very interested in whether this could help them with their planning decisions, whether to call for bids on a nuclear power plant.

That was about a year long project. And they sent four people from Mexico to reside near Palo Alto, California, near Stanford, for a year as we did this work. And it was pioneering work in electrical systems planning besides applying the decision analysis within this context.

WARNER NORTH: The model we built for power system planning lived on for another decade in Mexico, and was the basis for a lot of subsequent work, some of which was after I and friends left SRI to form Decision Focus. So the project was important in terms of learning about the electric power system, how complex it is, how to apply ideas of dynamic programming, and how to take into account a great many issues beside economics that were of interest to the Mexican government, which owned and operated this power system.

JAMES MATHESON: Yes. We had a social value model, and looked at what social values might be important, as well as profitability. We had a lot of dynamic decision making, because you make your decision, and then the plant isn't installed for several years, and then the electrical demand might change. It's a very dynamic environment.

But shortly after that, a guy named Myron Tribus, who was another important figure-- he was head of Dartmouth's School of Engineering--went into the federal government as Assistant Secretary of Commerce for Science and Technology. And NOAA reported to him. The National Hurricane Research laboratory, part of NOAA, was, at that time, interested in seeding hurricanes with silver iodide particles you put into the clouds.

WARNER NORTH: The background for this was that Tribus had had experience in a previous time with General Electric on the potential for cloud seeding. Once he had this government job, the National Hurricane Research Center reported to him, and he became very interested in the potential for cloud seeding of hurricanes to reduce the damage from hurricanes impacting the United States.

JAMES MATHESON: That was a project that Warner and Ron and I-- mostly it was the three of us, I think, that worked on that exciting project together. And there is kind of interesting twist. We did some good work showing that you should at least test hurricane seeding. And just about the time we were going to give Myron a recommendation, Nixon resigned. Trouble cascaded down, and

Myron Tribus was replaced. The people that came in didn't want to do seeding. I mean, I got phone calls commanding, "don't talk to the press about this." So we compromised by publishing a paper--

WARNER NORTH: Yeah, I was project leader on this project, and spending much of my time over a period of many months. The sequence, as I remember it, is first we had the great good fortune of being able to get a presentation in the White House to the president's Scientific Advisory Council, which I gave, with an introduction from Jim and Ron.

And we had a lot of enthusiasm. And we were contacted by the members of the President's Science Advisory Council about similar applications of decision analysis to energy. This was from Herb Simon in particular. And the president's science advisor, Ed David, presented the analysis to President Nixon, who seemed very receptive to it, especially on the need to have dialogue with the public of whether you should use a relatively new and unproven technology as an emergency measure to deal with hurricanes.

This was very much counter to the thinking of the scientific establishment and the National Weather Service, which felt they needed a great many replications, trials offshore, before they would dare seed a hurricane. But we had, on the other side, Dr. Tribus as a very enthusiastic advocate of going ahead immediately, because it was believed very unlikely that the hurricane would become worse, and reasonably probable (about 50%), that the seeding would reduce the property damage.

Well, we turned that into a decision analysis, which we presented. The problem was right about that time cloud seeding was carried out in North Vietnam as part of the Vietnam War. And there was a breakdown between the military people doing cloud seeding and the civilian agencies and the Weather Service.

There was also a breakdown with respect to PSAC, the President's Science Advisory Committee, which was disbanded. So the thought about applying decision analysis on other major national problems disappeared with the disappearance of President's Science Advisory Council.

JAMES MATHESON: Yes. The enduring thing is we jointly published a paper that appeared in Science.

WARNER NORTH: Yes. And that was something that Myron Tribus arranged.

JAMES MATHESON: It was published, often read, and never used because of this breakdown. Around this time, Carl Spetzler joined us.

WARNER NORTH: Yes. I believe that was actually earlier, during the CFE project.

JAMES MATHESON: And Carl Spetzler was mostly motivated to work on business problems. He brought in a lot of business focus. And we did many, many business applications-- like whether to open a mine?

WARNER NORTH: Well, previous to coming to SRI, he had worked with Ralph Swalm at Syracuse. And he had been involved in the decision for Amoco on non-leaded gas, expanding non-leaded gas. So he came in with a very good case study that he had done before joining with us. And together, we tried to develop more large-scale commercial clients.

And as I remember, our first effort at giving a seminar for some of the corporate sponsors of SRI fell rather flat because we were too much into academic theory and not focused enough on what would help them. But we persevered. And I believe Carl contributed enormously to figuring out how we could sell into the corporate world and develop successful projects.

JAMES MATHESON: Maybe one other twist along the way here was that Ron Howard and I wrote, I think in 1968, a essay on decision analysis for the SRI Long Range Planning Service, which had many subscribers. They would issue pamphlets on different things, usually about 40 pages in length.

It turned out that our pamphlet outsold any that had ever been issued. And we used that as a starting point to build a set of seminars on decision analysis. I think the first one was in Switzerland-- for an executive seminar on decision analysis. But suddenly, we got a call from the Swiss military, and they wanted a seminar. And I think you were at that one.

WARNER NORTH: Yes.

JAMES MATHESON: I was planning to be at the seminar, but we had to squeeze in a military seminar before our first seminar.

WARNER NORTH: I remember we did a private seminar at Phillips Gloeilampenfabrieken. That was the first I remember of going to a European audience and selling over there. SRI had a large presence in Europe. And subsequently, we have a lot of activity doing decision analysis projects in Europe with Paul Skov as the person who was in residence over there.

JAMES We then brought the seminar program to the US. It was just kind of coincidence that it started

MATHESON: in Europe. There was an opening there to do that. We started giving executive seminars--sometimes one day.

WARNER NORTH: We had a one day version and a five day version.

JAMES MATHESON: These seminars helped our marketing. We were the people doing decision analysis ly, and the executive seminars were very popular. We had no trouble filling the audiences. We usually gave them in three cities such as San Francisco, Chicago, New York, or something like that. Maybe throw in Boston. And then we'd do such a series about four times a year.

These seminars were our marketing: people would just call in. I remember sitting in a meeting. Where our staff was worried about where business was going to come from. And I said, well, you know, sometimes we get a phone call out of the blue from people we've seen at our seminars. And just then the phone rang, and I picked it up. It was another job.

[LAUGH]

WARNER NORTH: So the seminar program went from being relatively unsuccessful on its first occasion to being the major draw for bringing in business.

JAMES MATHESON: Yes, especially commercial business.

WARNER NORTH: But we brought in some business from the public sector as well.

JAMES MATHESON: Yeah. And I think Warner's more of a specialist in the public sector than I. Maybe you could elaborate.

WARNER NORTH: Well, there was one seminar specializing in probability assessment for EPA that led EPA to try to use decision analysis in setting air quality standards. This was back around 1975 or '76. By '78, there was a science advisory board subcommittee set up to review EPA's proposed application. I was involved in that. And this issue still continues. I'll have a presentation this afternoon [at the 2015 INFORMS session] by Anne Smith on the latest in terms of proper characterization of uncertainties on the health effects of air pollution in the context of the decision to set these standards.

JAMES MATHESON: Yeah, and another thing we did somewhere in that time frame was look at nuclear reactor safety. And we wrote up sort of a pilot view of how one would address the risk analysis for

nuclear plants, which involved looking at all the sources of damage to these plants, all of the causes, all the way to radiation being released from the plants. And then evaluated how the radiation could reach people or property, and all of the damage radiation does.

WARNER NORTH: Would you characterize this as a pilot study for the study led by Norman Rasmussen, which became WASH-1400?

JAMES MATHESON: We did work on WASH-1400. I'm not sure whether we did that during Rasmussen's leadership or just before. But we had a pilot study, and then what's called WASH-1400 was sort of the biggest credible risk analysis of nuclear plants. And we were advisers to Norm Rasmussen, and a key person that had experience back from GE's nuclear power division, Howard Cook, who actually did the analysis of boiling water reactors for that study. Norm Rasmussen did pressurized water reactors. So our group actually did the major study of one type of reactor.

But what's interesting is that then the nation regressed. And so the nuclear regulatory people in the industry stopped looking at all the consequences of nuclear actions. They just looked at, well, is there damage to the plant? That was a type one analysis. And then they looked at, is radiation released? Is there a core melt? They're more interested in the core melt but not the consequences.

And it's only recently they're calling for end to end analysis again. I just served on a committee of the National Academy of Science to investigate the Fukushima nuclear reactor accident. And that-- we certainly advocated in the report the National Academy just issued that they go back to what our original pilot analysis had recommended. End to end analysis, and even add in a few twists.

At Fukushima, they had several plants right next to each other. And if common resources-- like fire engine--are being used at one plant, they are not available for the second plant. Hydrogen gas from one plant blew up another plant. All these interactions between plants are not usually accounted for.

The other thing was the cause was a giant tsunami that disabled the infrastructure. So when the nuclear plant had its problems, there was no infrastructure. You couldn't even drive on the roads. You couldn't send them help.

WARNER NORTH: And you couldn't bring in electricity from the outside.

JAMES So I think those pilot applications were very important. And to some extent, we were way

MATHESON: ahead of our time. I guess that's one of my curses. We get a lot of the early applications. Warner and I were called in to work on the possible contamination of Mars, as we had an agreement with Russia to limit the probability of contaminating Mars by Earth organisms.

WARNER NORTH: Carl Sagan became well known for his advocacy of, shall we say, good practice and innovative ideas in exploring space. Once well known, he worked with his Soviet colleagues in the 1970s to negotiate an agreement that had essentially the force of an international treaty, that the probability of contaminating Mars with terrestrial life should be held by both space-faring nations to be less than one in a thousand.

When photos indicated the possible presence of liquid water on Mars, all of a sudden there was great concern: Were we violating this constraint with the US program? And I got a call one day from Joshua Lederberg, a Nobel laureate at Stanford, asking if I'd be willing to talk to some people from NASA. So they came over to SRI, and they asked us first to do an audit of the probability calculation, and we found some problems in it. Mike Harrison from the Stanford Business School was involved in this first phase.

And then we were asked to do it right. And that was fascinating. We came to the conclusion that the probability of contaminating Mars from the Viking lander was six chances in a million, a factor of 16 below the mission constraint, that is, one order of magnitude below one in 1,000, which is one in 10,000. And the sensitivity analysis showed you would have to change several variables simultaneously before the constraint would be violated.

The reason for this could be very simply explained. The Martian atmosphere was too thin to support a particle which was large enough to shield a microbe from ultraviolet radiation. Now nobody thought of this in the NASA hierarchy when they were facing the problem. But when we presented it in the form of a numerical analysis, everybody bought in. So much so that the Space Sciences Board of the National Research Council decided that there is no issue here. And there has been little if any subsequent analysis.

This story is similar to Jim's story about Fukushima, I've had discussions with the head of JPL to the effect that where you are landing a spacecraft right near an area that might have water in near liquid form and might also have carbonaceous material , then the conditions for terrestrial life reproducing on Mars could be met. This problem deserves serious further attention as we explore areas of Mars quite different from where Viking landed.

JAMES MATHESON: Yes. One thing you might not know Warner, is I went to a talk that happened to be given at Stanford about five years ago-- on the latest in planetary quarantine. I think they were NASA people. These people were explaining planetary quarantine, and it all looked very familiar. And I went up afterwards to this young woman and said that we worked at SRI, and I explained what we had done. She said, oh, that's our Bible.

WARNER NORTH: Well, I hope Charles Elachi has found that Bible, because he was worried about it. And I did send him our reports. Not just the summary publication, but the detailed reports we wrote as SRI reports. They are available on the internet.

JAMES MATHESON: So we did a variety of work. Most people think of us as doing mostly commercial work, but you can say we did a lot of interesting applications, including outer space. Not so much on governmental decision making. Oh, I guess one thing we have to mention is the work again in the intelligence community.

WARNER NORTH: Yes. We want to get the story of influence diagrams and how it related to your earlier work for GE with coalesced decision trees.

JAMES MATHESON:

We should explore what led up to the invention of influence diagrams. We had struggled with complicated probabilistic modeling on previous work. For example, in the work we did for General Electric on designing the space program, we had decision trees with millions of nodes, and millions of policies. We were writing computer software that could only run on weekends on General Electric's computers. We'd ship decks of cards to Philadelphia, and they would run them, and they'd ship the printouts back. On Monday, we'd get the printouts and look at it, and then send them another deck of cards.

And what we learned was a little bit like what I had learned about Markov processes, that a lot of the decision tree structure is repetitive. So once you have arrived at a certain place, the past doesn't matter so much. You're in a certain state where you've finally landed on Mars, but you haven't done life detection. And that node repeats a million times in this tree structure. So if you can only calculate it once, that's a big advantage. And so we developed software-- we call it coalesced decision trees-- that could handle this. But it was a bit of a kludge. It was early days of computation.

So stepping forward, we were working-- and I think Warner was highly involved with the intelligence community on how to assess the probabilities of various things happening in the Mideast.

WARNER NORTH: It was the value of information in the context of a threat of regional conflict. And we structured it as a large decision tree where we would look at the value of information in the context of a series of the order of 20 events that might happen in the future.

JAMES MATHESON: And then those 20 events are all related to each other. One-- you know, there's an assassination somewhere, and then there's revolution somewhere else, and then somebody attacked somebody. And then we have to decide how the US is going to intervene. These were the sorts of events.

WARNER NORTH: I, as project leader, set the goal that we wanted to have a structure such that we could interview an area expert in the intelligence community, and in a half day interview, we could assess the probabilities needed to fill in the tree structure. Allan Miller on this project had the idea of going from the notion of coalesced decision trees to using influence diagrams as a visual aid to what depended on what.

JAMES MATHESON: Yes. Al tried 18 different kinds of representations. And we kept the whole group working as kind of a team. We kept looking at various representations. This doesn't work for that reason. This doesn't work for another reason-- should arrows go two ways? Should arrows go one way? And we finally came upon the fact that the diagram is a set of bubbles representing variables in a probability distribution, and if you want to put a joint distribution on it, you would basically have to have a directed graph connecting all the nodes.

But if you do it right and you have conditional independence, you can erase a lot of these arrows. So you end up with a simple diagram with a few arrows, where some events like the assassination may be a triggering event. And when that happens, it triggers other events.

And it became-- as I'm an electrical engineer, we use signal flow diagrams all the time with bubbles and arrows--And it really became a signal flow graph for thinking about probabilistic systems, and a much more attractive way to encode probability and to design computer software. The software uses special methods of computing influence diagrams directly instead of making decision trees out of them.

But you could make a decision tree and calculate that. The influence diagram would tell you all

the information you needed for coalescence, because it shows what depends on what. So it's a huge breakthrough. And DARPA also funded some work on automated decision aids, which the earlier work fed into. And we eventually wrote up papers on influence diagrams, which are among the most referenced papers in our profession. Did you want to add anything about influence diagrams?

WARNER NORTH: I will add from my perspective as project leader, we went from a situation where the tree seemed impossibly complex to a situation where we accomplished three interviews in a half day or less, filling out this tree turned into an influence diagram where we went all the way through with the numbers. We came up with a map of potential events in the Middle East circa 1973. And it was remarkably accurate in terms of what happened subsequently. We felt we really nailed it. But this work was classified, top secret.

And I will add for the record, I went to SRI Management after a meeting with a manager in the intelligence community who said, basically, I will continue your research, but I don't expect any young whippersnappers like you with computers and fancy diagrams to help me make decisions. And that was the point where I decided, if others at SRI wanted to go on in this area, I would help them learn what we had learned. But I did not want something that I saw as valuable as a decision aid getting stuck in top secret classification and not being available for other uses. Fortunately, we're able to negotiate with DARPA and others so that the essence of our work became widely available.

JAMES MATHESON: Yes. We could go on and on talking about different projects in energy.

WARNER NORTH: I think we might now ask "how good our idea was of the "teaching hospital" at SRI? How well did it fit? And why was it that after 10 years, we started to break apart? I'd like to get your ideas. At some point, I want to describe the acquisition of the word processing system. This event, in my judgment, was a significant cause of at least the first wave of exits, and maybe indicative of the problem of trying to do what we were trying to do in a non-profit think tank setting, as opposed to being out in the harsh world of for-profit consulting.

JAMES MATHESON: Yes. Well, first of all, I should say SRI was a wonderful home to get started. It's kind of a technology bazaar. If you do credible work and you can get it funded, you can develop a department and a program. And we did credible work, and we had a close tie to Stanford. We got lots of funding. And we were pretty much left alone. I think they valued us pretty highly and

supported us.

But we never asked for big investments. And we were living within this non-profit mentality where you should be thankful you're able to do your research. And it was always a battle to try to get commensurate salaries for people with what they could get elsewhere to keep them in the group.

And then they supported it pretty well, actually. I remember one year I asked everybody to write down, given you're here and all that and you're in our group, what salary do you think you should have to be competitive? And they wrote it down. Everybody handed me a piece of paper. And I went and got it. So, they supported that.

But this was not consulting level salaries. And so towards the end of your tenure there, there was actually a study done by the president and others of whether they should have a profit making subsidiary. And they looked at that, and we were hopeful that we could be part of a profit making subsidiary and have different rules for salary.

But it became clear that that wasn't going to happen, at least not to us. And I think people started to say, we were so successful, for this group to continue, they have to be in a profit making environment. And at that point, people started to leave. Warner-- I guess you were in the second wave.

WARNER NORTH: I was in the second wave.

JAMES MATHESON: So the first wave was a group of people that started a company called Decision Focus.

WARNER NORTH: No, that was Applied Decision Analysis.

JAMES MATHESON: Applied Decision Analysis. Excuse me.

WARNER NORTH: And the story I want to tell is my memory of how that happened. And that was that these three people, all of whom were at the INFORMS meeting here yesterday and probably today, were all finishing PhD dissertations at Stanford.

And one of them found out about a word processing machine called Vydec. Rather the usual process of having secretaries retype things, this would allow writing on a computer and being

able to correct the text as you went. I had just finished my PhD and dissertation a few years earlier, and was very aware of the problem of having to use a "Correctype" to correct errors on typewriter, going back over and over again correcting drafts to get the text right. So I was very supportive, as Jim's assistant, of buying this word processing machine so these three guys can use it at night for their PhD dissertations. And they had offered to train the operator by day so that we have a skilled secretary that knows how to be much more productive getting text written and corrected.

The problem was that SRI had a central report facility. And after the operator had been trained and these guys were far along in typing their dissertations, the management came and said, you have to give that machine up, because this type of document production has to be done by Central Report Services.

I went and visited the president of SRI at his home in an evening to protest and was told, "no, I'm reaffirming the decision." And you must understand that SRI is a seedbed where talented people work for a few years, and then they may go out and have their careers elsewhere.

And I thought to myself, Mr. President, you just gave me a good reason for why I should leave in the near future. I was part of a group of four people that has been asked by senior SRI management, "what can we do to retain excellent staff at SRI?" Our proposal was, "Set up a sabbatical system so that if you really did excellent work, you could be awarded six months of free time to do what you thought was interesting and important, rather than being sold sufficiently on projects." That idea was not implemented. All four members of the committee had left SRI within a few years following our group's proposal in response to the question we had been asked to answer.

JAMES MATHESON: Yes, that was too bad We had these groups leave. And then Carl Spetzler actually left to another small company, Research Planning Associates. And he turned around and hired a lot of the remaining people, including taking me and Ron Howard.

And we haven't said enough about Ron's role. Ron was always an advisor to this group. And practically all of his consulting side of one to two days a week was spent within the SRI Decision Analysis Group, in a very teamwork environment. He was on the team.

WARNER NORTH: And I think we would have to say that many of the early, big jobs we had-- Mexico and the connection with Myron Tribus--

JAMES MATHESON: And the Morgan Guaranty.

WARNER NORTH: Morgan Guaranty. These were all things that Ron brought to us. We didn't realize nearly as much how we would use the corporate sponsors of SRI as a way to get business. We wound up doing that ourselves primarily through our seminar program.

JAMES MATHESON: Yes. Our seminar program took off, and that's what really funded us. So I guess the other thing that happened just before everybody left-- and I'm not sure of the exact timing-- is we started to do analysis of portfolios of projects, which has become very important. Because we had done "one-off" projects - should you do this R&D project, should you do that R&D project? - it was natural to move to portfolios of such projects.

Then Exxon-- actually their central research lab-- wanted to look at their whole portfolio. We went through the whole Exxon portfolio once I think around '79. And they did it again two years later, just at the time we were leaving. And they were looking at the whole portfolio on a regular basis as a matter of stewardship and decision making. they also were able to see the influence of changing oil prices on the value of their entire portfolio.

So that has become much more prevalent since, but that was initiated there. A guy named Mike Menke was very instrumental in doing R&D work. And we started giving seminars, actually, on R&D decision making before this diaspora.

But then as happened-- I was mentioning that Carl left to join Resource Planning Associates. He then turned around and hired a bunch of us to join Resource Planning Associates, which we did. I went there. Several people-- Ron moved with the team to make his center of attention Resource Planning Associates.

Unfortunately, the style of that company and our style were somewhat in conflict. We had an office in San Francisco, which we then moved to Menlo Park, California. And we just got at loggerheads with the owner of Resource Planning Associates.

So one day he got frustrated. And Carl, who was the managing director, was relieved of his job. The head of resource planning asked me if I wanted it. I said no. Another guy said no. We got together and said, we need to start our own company. And we made a plan. I negotiated with Resource Planning Associates, actually a very friendly split.

And other members of the team got us a line of credit, and we started Strategic Decisions

Group. So a little pirouette there happened to create Strategic Decisions Group, which focused more commercially. At Strategic Decisions Group, we rarely did the kind of research-- space work and stuff like that-- that we had done in the past. But it turned out to be a very successful consulting firm.

WARNER NORTH: I want to back up to one project that I think we should mention in the mid '70s before we had this parting of the ways. We had a project led by Ed Cazalet for Gulf Chemical, which led to a project on Gulf's position with respect to synthetic fuels.

This was a situation where the president of the chemical company went up to corporate and saw this major decision problem of, "What should Gulf be doing and thinking about developing new sources for gasoline, diesel fuel, from resources like oil shale and coal?" The technologies were available from experience in World War II, but they were very expensive. And the perception was that the price of crude oil was going up, and there wasn't going to be enough supply to meet demand. And therefore, should these synthetic fuels be used to augment traditional sources of supply? So Ed led a team that developed a very complex energy model. I think you and I had near heart failure on whether they would ever get this model to work. But it finally did. And it did a very comprehensive balancing of supply and demand across the energy network from supplies, resources in the ground, to end uses. The model was on the scale of 10,000 equations and 10,000 unknowns.

We brought in Ken Arrow to ask, was this good operations research? Should this model work? And I remember him spending a day or so looking at it all, and then explaining that it was an application of Brouwer's fixed-point theorem, and yes, it ought to work. And finally it did.

[LAUGHTER]

Then this went to the government side when a decision analyst out of Harvard, Jim Walker, who was in the Office of Management and Budget and part of the staffing, was asked to evaluate an insert in President Ford's state of the union message of developing a million barrel a day synthetic fuel program within a relatively short period of time, like 10 years. Jim Walker thought this should be analyzed in detail to see if the proposed synthetic fuels program made sense.

So we became the support contractor to a large federal task force carrying out the analysis of the Synthetic Fuels Program. We concluded that the cost exceeded the benefits, and that it

would be a good idea to scale this program from a million barrels a day to one plant of each major type, and informational program of about a third of a million barrels per day.

We got phone calls from Congress asking, what did it mean that the cost even of this informational program exceeded the benefits? The net benefit was negative. And by a few votes, the program lost in Congress. But the next administration, the Carter administration, put this program into effect, and it was a disaster for reasons that were included in our analysis.

JAMES MATHESON: Right. Yes, exactly.

WARNER NORTH: I feel in terms of the SRI group's work in public policy applications, that was the high watermark in terms of doing a really big problem with 10 federal agencies involved and a terrific set of human resources in terms of their knowledge of the energy industry, and going from a private sector application to the public sector application very successfully. I can describe some aspects I think we didn't do very well, because we didn't reach out to the stakeholders and really get a dialogue going. Rather, we built a four volume report off of a big computer model that only a few people understood. But still, I think we did an excellent analysis that stands the test of time.

JAMES MATHESON: Yeah, I think basically, that model still exists out there.

WARNER NORTH: In fact, it exists in many forms, and it is being used, to my knowledge, for energy planning in at least six countries. And that may be as largest as 20. One version of it now lives at the Argonne National Lab.

JAMES MATHESON: Maybe I should turn to what happened a bit at SDG and beyond. So many of us were at SDG, Strategic Decisions Group. Warner had gone to another firm, so he was no longer part of the team, although we were certainly good friends and colleagues. But as the name implies, Strategic, we focused a lot on strategic decision making, and developed more and more tools for grappling with strategy. Things like strategy tables and decision hierarchies.

And the basic question there is in strategy, you can do anything, right? So how do you put bounds on that? And tools like strategy tables give you a table with many dimensions of things you can do. And then you do reasonable-- not all combinations of those dimensions, but reasonable combinations that maybe go slow, go fast, expand in foreign countries, whatever

you label some things, and you analyze those.

And that really gives you the lay of the land-- it's almost a sensitivity analysis. The best decision that you come up with from that analysis usually can be improved upon. But it gets you from almost infinite dimensions into something tractable.

So we did a lot with strategy. We did a lot more with portfolio decision making. We did a lot with R&D. I started the R&D Decision Quality Association, which was a group of about 10 companies, who actually funded work on software for doing R&D decision making, and also a lot of training. Every year, we'd bring this group together. We'd have an executive group, and we'd have professional group meetings of this quality movement.

And every year, when we would have the big executive group meeting, we had to think of how we were going to entertain them. And you do this six months in advance. So before one of these meetings, my son David and I said, let's do something about the principles of a smart organization.

So what are the underlying principles that make an organization smart? And then as the time approached for the meeting, we had to come up with it. So we actually rented a hotel room, something like this room we're in, and papered the walls with ideas and thoughts, and came up with nine principles of a smart organization, and published a book with these principles.

They're things like a value creation culture. Everybody-- our purpose is to create value. And it doesn't matter who suggests it, we're going to judge ideas on the merit of, do they create more value? Things like that. Outside in perspective. Don't get stuck with your internal perspectives. Bring in outside perspectives. Then discipline decision making. Have decision-making processes in place so that when you want to decide something, you can say, use a dialogue decision process. And you know what that means. You're going to assemble a couple teams and you're going to have executive guidance. So we published this book, The Smart Organization.

And around that time, Strategic Decisions Group actually was sold and then bought back. And in the buy-back in 2000, they decided to spin out the software. So we had been doing software in this Decision Quality Association. And it was suggested that some of us take the software assets and form another company.

So my son David and I, who had written the book, and a few others formed SmartOrg

Incorporated, which is where I am now. I'm the chairman and CFO. He's the CEO. And we shifted from the consulting model-- the consulting model is, we're really smart guys and you need us to come help you make decisions. Sort of like a doctor or a lawyer, right?

We shifted from that model, to regarding our customers as very smart guys, who need good software to help them do this complicated decision analysis. And we will build you software. We will give you training. We will put in good processes. But ultimately, we want to make your organization smarter. And it's not that there's no place for consultants. When big decisions come up, you might want the big time consultant. But we want the less than earth shaking decisions, but important decisions, handled well within companies. And so that's what our firm is dedicated to now.

WARNER NORTH: As we wrap up, I'd like to throw out the question of, how have we evolved from those SRI days? And propose that one of the ways we have evolved is a lot more emphasis on process for understanding why normative or rational decision making is not enough. You have to figure out how to interact with people, how to get the ideas implemented.

I recall meeting Amos Tversky and Dan Kahneman in the early '70s as part of the SRI Group's outreach. And we all went through their questionnaires and made the classic mistakes of thinking fast rather than slow, and learned a lot, and build it into our process for probability encoding.

But it seemed to me the larger lesson, which you talked about in your INFORMS presentation today, and I think I did in somewhat different language, is the importance of being able to facilitate dialogue between decision makers, experts, stakeholders, and develop a consensus for moving forward on-- I will call it carefully engineered decisions with a structure of models and data that can be reviewed by other people. And substituting this for intuitive decision making by people who have done it for years and think are very good at it, but whom professors such as Chris Argyris have described as making classic mistakes because they're caught in old traditions and old ways of thinking, and they don't listen well.

JAMES MATHESON: Yes. I think where we're headed is engineering the whole organization to be smarter in many ways, and have the right people, have the right kind of open dialogue. A lot of these-- I'm sure in the governmental area they talk about transparency but rarely get effective dialogue.

When you do that, you're going to hit controversy. Now, is controversy OK? I think engineers don't have much trouble with controversy, because they're used to engineering reviews. So

you do a review and people try to shoot you down, and you have to have a little bit of thick skin. But it's hard to do that with the media today, for example. In public decisions, it's very hard to have that kind of review. I think companies that have adopted decision analysis processes get more open about it.

Even communicating about uncertainty, you've just decided to invest in a plan, and some guy gets some new information about where the competitor's going to locate his plant. Is he supposed to shut up about it and be supportive of the decision? Or is he supposed to bring it up to the CEO and say, hey, wait a minute. We've got a problem. It's more important to vet the news, bad or good. And so the whole culture of the company or the public forum has to be shifted to openness.

The other thing I should mention, coming out of the West Coast school of decision analysis versus the East Coast, is that the West Coast came out of engineering. So it was basically electrical engineering background, engineering school mentality. This makes us very comfortable combining lots of models - systems models, large scale ones like the energy model, and using them as part of the decision analysis .

And this way of using models is that you never want to make the model more complicated than you need for the purpose. But the ability to quickly build models, do sensitivity analysis, shape the model for that particular decision, is much more of a notion that came out of Ron Howard and the engineering background. In his initial paper, he advocates about starting with the pilot model. Then you do the full scale model, and then you do the final decision. The process is successive layers of modeling. Maybe do a "back of the envelope" model first. That engineering approach has really been built in to the West Coast decision analysis.

WARNER NORTH: Ron in his talk yesterday brought up Morse and Kimball. I remember reading that in my early career and thinking how wise these people were in the use of mathematics to deal with a problem, and figuring out through use of sensitivity analysis, how much modeling you needed. That's a message that I think many people in the history of operations research have tried to provide to the community.

But then the other side of it is, if I can add something-- new mathematical methods, more complexity-- that's often the way I get a promotion. I have something new to add to the literature. And this balance is, I think, a very important issue for the future of the field. How can you do enough modeling, how can you invent the new ideas that are needed for a particular

problem, and avoid getting too much complexity?

JAMES MATHESON: That's one of the neat things about decision analysis, is once you focus on a decision, then you say, I only need enough modeling to distinguish between the alternatives. Pick the best one. I don't need to predict the future. So you can stop. And so decision analysis provides a guide of when to stop modeling.

But you can also look at it and say, boy, this is really sensitive to some model assumptions, and we've got to really work further. And of course we have things like value of information we can do formally. If information is valuable, you stop and gather the information, so forth. So the secret of decision analysis is that decision gives you the guide you need to manage your work.

WARNER NORTH: Jim, do you have any other thoughts you think should be added to this interview as we conclude?

JAMES MATHESON: I think we're at a pretty good concluding point. Thank you, Warner.

WARNER NORTH: Thank you, Jim.