An Interview with Ronald A. Howard

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Ronald A. Howard has contributed substantially to defining the field of decision analysis since his first publication using that term in 1966 (Howard 1966). He has been a professor at the School of Engineering at Stanford since 1965, currently teaching an average of 450 students per year and having had 86 Ph.D. students in the last 44 years. In addition to numerous journal publications, Ron Howard has authored five books, including one very recent book on ethical decision making (Howard 1960, 1971; Howard and Matheson 1977, 1984; Howard and Korver 2008). He earned his Sc.D. in electrical engineering from the Massachusetts Institute of Technology (MIT) in 1958. Prior to joining the Stanford faculty, Howard was associate professor of electrical engineering, associate professor of industrial management, and associate director of the Operations Research Center at MIT. In 1986 he received the Frank P. Ramsey Award for “distinguished contributions in decision analysis.” Professor Howard was also a cofounder of Strategic Decisions Group (a private consulting firm) and the Decision Education Foundation (a philanthropic educational foundation). Earlier this year I had the pleasure of interviewing Professor Howard to discuss his life and his experiences in academia and business, as well as his current directions.

Key words: decision analysis; Ronald A. Howard; Bayesian statistics; influence diagrams; decision education foundation

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INT: Interviewer Russ Garber
RH: Ron Howard

INT: To get started, tell me a bit about your childhood. Where did you grow up and did anything in that experience affect your future in academia or decision analysis in particular?

RH: To be brief, I grew up on the South Shore of Long Island, New York, graduated from Lynbrook High School, and went to MIT in 1951 on a Grumman Scholarship. I suppose I was destined to be an engineer since both of my grandfathers helped build the Titanic.

INT: Tell me a bit about your academic background.

RH: At MIT, I received bachelor’s degrees in both electrical engineering and economics and engineering in 1955. I stayed there and received the degrees S.M., E.E. (electrical engineer), and then Sc.D., all in electrical engineering, by 1958.

During that time, I met Bill Linvill,1 who was the professor in some of my classes. Our relationship expanded when, during my senior year as an undergraduate, I decided to go into operations research (OR) (under the umbrella of electrical engineering). This relationship grew the summer after my senior year when I worked for Raytheon Research Labs, where he was a consultant, and we were able to interact on a more personal basis. Once in graduate school,

1 William K. Linvill (1919–1980) was an assistant and then associate professor of electrical engineering at MIT from 1949 to 1956, and in 1960 was the founder and chairman of the Institute of Engineering-Economic Systems at Stanford University, which later became the Department of Engineering Economic Systems. He was a professor at Stanford from 1960 to 1980. In the years between, Linvill worked for the Defense Department in Washington, DC, and the RAND Corporation in Santa Monica, CA. For more information, see http://histsoc.stanford.edu/pdfmem/LinvillW.pdf.
Linvill recommended that I look into the Arthur D. Little Corporation as a place to further my knowledge of operations research.

The Arthur D. Little Corporation was an excellent opportunity for me because, at that time, they had perhaps the premier operations research group in the world doing commercial consulting. I interviewed there and got a summer position in addition to two days per week of work throughout graduate school. During my first year, I applied for the Ramo-Wooldridge Corporation Fellowship for the following year. Bill Linvill was on the fellowship committee and one Saturday morning I got a call telling me that although he thought I should have gotten the fellowship, I did not. Nonetheless, Linvill thought so highly of me that he arranged for a university fellowship to cover my second year in graduate school. I felt extremely honored, and my relationship with Linvill grew closer. The next year, I did receive the Ramo-Wooldridge Fellowship and spent the summer of 1957 working with them in Los Angeles, again in close contact with Linvill. During this time, some notable people that I got to work with were George Kimball and Philip Morse, the authors of the first text on operations research: Methods of Operations Research (Morse and Kimball 1951) based on their wartime experience. They became my advisors for my doctoral thesis on dynamic programming and Markov processes.

**INT:** Tell me a bit about your dissertation.

**RH:** I did my dissertation on dynamic probabilistic systems, and specifically on Markov decision processes. It was motivated by a problem that Sears Roebuck faced in sending out their catalogs. I wrote about this in the 50th anniversary edition of the journal Operations Research (Howard 2002). The idea was to optimize how they should send out catalogs based on customer purchases. However, only in the most unusual circumstances, like the ones that existed at Sears, could you get the data for describing how these transition probabilities would change with different policies. We were also bound by confidentiality requirements, so it was at least 20 years before I could write about that.

**INT:** And what did you do upon graduation?

**RH:** After graduation in 1958, I was hired to stay on at MIT as an assistant professor of electrical engineering and assistant professor of industrial management. In 1960 I extended my thesis work by publishing the book Dynamic Programming and Markov Processes (Howard 1960). In 1959 a couple of things happened that were formative to my future in decision analysis. One was meeting Howard Raiffa, who at this time was at Columbia University and had just written Games and Decisions with Duncan Luce (Luce and Raiffa 1957). The Ford Foundation had funded him to conduct a program called the Institute for Basic Mathematics for Application to Business. This was a program where business school professors from all over the country would come to spend a year at Harvard with their families and learn basic math.

**INT:** You mean, they did “basic math”?

**RH:** Yes! They studied decision theory, linear programming, probability, and the use of computers. You see, at that time, business education did not rely very much on math. Faculty admitted to this program had to be recommended by their deans. Many professors who later became deans of prominent business schools attended. This program was very successful; the attendees went on to change the nature of business research.

**2** George E. Kimball (1906–1967) was a chemistry professor at Columbia University, and during World War II worked with Philip M. Morse in the U.S. Navy’s Operations Research Group on the problem of antisubmarine warfare tactics. From 1956 to 1967, he worked for the Arthur D. Little Corporation. For more information, see http://en.wikipedia.org/wiki/George_E._Kimball#The_George_E._Kimball_Medal.

**3** Phillip M. Morse (1903–1985) was a physics professor at MIT and was the first president of the Operations Research Society of America in 1952. For more information, see http://www.eng.tau.ac.il/~ami/cd/or50/1526-5463-2002-50-01-0146.pdf.

**4** Howard Raiffa (1924–present) is a professor emeritus of managerial economics at Harvard University and a leading member of the field of decision analysis. For more information, see http://en.wikipedia.org/wiki/Howard_Raiffa.

**5** R. Duncan Luce (1925–present) is a professor of cognitive science at the University of California, Irvine. In 2003 he received the National Medal of Science in behavioral and social science. For more information, see http://en.wikipedia.org/wiki/Duncan_Luce.
Another formative force was Myron Tribus, dean of engineering at Dartmouth, because he introduced me to the work of Edwin Jaynes. I still tell the story of how I became converted. One day I had a haircut reading one of Jaynes' works. By the end of the haircut I was completely convinced that he had the correct interpretation of probability (see Jaynes 2003). Jaynes' works were very deep in understanding the meaning of uncertainty. It got to the point where whenever one of his papers came out, I would just drop everything that I was doing so that I could read it.

**INT:** By “correct interpretation,” do you mean subjective probability?

**RH:** What do you mean by subjective probability?

**INT:** A probability based on one's own opinion.

**RH:** Is there any other kind of probability? If you read Jaynes, you will never again entertain the notion of an objective probability. In other words, it is a mistake that is built into the words themselves. You cannot get probability from data.

**INT:** But, if you had lots and lots of trials, like a million, would you not get an accurate probability?

**RH:** Think about the probability that the next shuttle flight will be successful. Is it the number of previously successful flights divided by the number of flights? Since anything that fails or is likely to fail is changed before the next flight, with either beneficial or detrimental results, does the same shuttle ever fly again?

The question is whether the trials are exchangeable, which means that the result of any trial can be exchanged for that of any other trial without affecting any belief based on the experiment. The trials of the shuttle are not exchangeable. I challenge you to find one million exchangeable trials of any uncertain event.

For example, if a drug trial shows that 5% of the people who use a particular drug had their fingernails fall off, will you assign the same probability to your experience if you take the same drug? You will do this only if you consider these people exchangeable with you in terms of sex, age, education, socioeconomic class, physical fitness, ailments, and any other factor you wish to consider. You have to be able to say to yourself, “If it happened to anyone in this group, it is equally likely to happen to me.”

**INT:** But isn’t it better than nothing? And don’t good studies control for many factors?

**RH:** Well, yes, in making my decisions I may use the studies as a guide to my probability assessment and then adjust based on the particular circumstance. You can always think of a factor that was not considered in the control.

**INT:** And all these forces framed your early works on decision analysis?

**RH:** Well, yes. You see, there were all these forces coming together for me. One was the work of Jaynes. Then there was the practical nature of the work at Arthur D. Little helping real companies with their decisions. Remember this was the heyday of operations research in the sense that the companies would have OR groups that would report to their presidents. It was wonderful!

At the same time, you had the statistical decision theory with decision trees in addition to the systems engineering approach coming out of electrical engineering with the work of Bill Linvill. So, all these forces were floating around and gave me the impetus for my work in decision analysis.

**INT:** What did you teach when you first started at MIT?

**RH:** At MIT, I started a graduate sequence in dynamic probabilistic systems and a course in probability, which was the first one offered in the Electrical Engineering Department. Then, a year or so later, I developed a course called Statistical Decision Theory, which was mostly balls and urns and decision trees. I wrote a paper for that course about the used car buyer which is all about options. By the way, this

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6 Myron Tribus (1921–present) has held many academic, business, and government positions, including director of the Center for Advanced Engineering Study at MIT (1974–1986). He is best known for his work relating thermodynamics and probability. For more information, see http://en.wikipedia.org/wiki/Myron_Tribus.

7 Edwin T. Jaynes (1922–1998) was a probability theorist and professor of physics at Washington University in St. Louis. He is best known for his work on the maximum entropy principle and probability reasoning. For more information, see http://en.wikipedia.org/wiki/Edwin_Thompson_Jaynes.
course was offered jointly by Electrical Engineering and the Sloan [Business] School, so there were plenty of students from both places.

Soon after, I worked with General Electric (GE) on something called the Modern Engineering Course. This was a six-week residential course for experienced engineers on the “latest things in engineering.” I taught “relevant mathematics” one week in the middle of the course. The curriculum included both Markov modeling and statistical decision theory. By this time, Bill Linvill had left MIT for Stanford and I inherited his courses on sampled data systems.

INT: And how did Linvill’s leaving eventually influence your future?

RH: Well, we kept in touch at professional meetings, and I guess it was in late 1963 that he said, “Why not come out and visit [Stanford] for a year?” I was very committed to MIT at the time, but he said, “Well, you can write this book you’ve been talking about.” So I accepted, thinking this would be a great opportunity for me, my wife, four kids, and dog. So I came out to Stanford for the academic year 1964–1965. While at Stanford, I also taught a course once per week at GE Nuclear in San Jose. This came about because one of the participants in the GE Modern Engineering class was a vice president at GE Nuclear. When he found out I was coming to California, he said, “Why not teach this material to my people a few hours per week?”

After two weeks of teaching this class, one of the engineers asked me, “Could you make real decisions using your methods? We are facing a major decision right now about whether or not to put a superheater on our nuclear reactor power plants. This would increase the steam pressure and the efficiency but there are increased costs, uncertainty about material lifetime, and other issues.” I said, “Why not?”

We spent the next several months focusing on this application, bringing together systems engineering, the modern view of probability, and decision theory.

INT: The modern view?

RH: Yes, the Bayesian method as elucidated by Jaynes. You see, the standard view of probability was that you needed some statistical regularity, repeated trials, to have the notion of a probability. In fact, Philip Morse once told me that operations research could begin when you had 30 data points. But I realized that the kinds of decisions where you had many trials to observe were not the ones that top executives were concerned about.

Certainly, this was true about the superheater problem. This study was the first decision analysis that I did. The analysis showed that it was not, as they originally believed, the lifetime of the material used in the reactor that was essential, but rather the company’s time preference for results. The company benefited from the knowledge that adding a superheater was not a good decision for them at the time. I benefited from seeing that the decision procedures I had been teaching would really help people make important decisions.

INT: Please tell me a bit more about the year at Stanford.

RH: I spent that year writing my books on dynamic probabilistic systems and also doing the superheater decision problem. Toward the end of the academic year, I was invited by Howard Raiffa to speak at a conference in Boston. So I had to figure out what to call the analysis used on the superheater question. I considered different things.

One was decision engineering, which is probably the best title in terms of what you are really doing, but I was concerned about engineering having the connotation of manipulation, so I chose analysis. So there it was, “decision analysis.” I titled the paper “Decision Analysis: Applied Decision Theory.” I presented it at the conference and published it soon thereafter. So that is how I became a decision analyst.

INT: Did you return to MIT?

RH: No, although I liked MIT, I thought Stanford was a wonderful place, and when they offered me a permanent job at the end of the year, I accepted. Believe it or not, I have not gotten a promotion now in 44 years!

INT: Did you come to Stanford’s Electrical Engineering Department?

RH: No, I came into what was called the Institute of Engineering-Economic Systems (EES). It was officially an institute before it became the Department of Engineering-Economic Systems.
Let us go back a little bit. Bill Linvill headed this institute (which was probably the reason I was offered the position). Now, technically Linvill was a professor in electrical engineering, but that detail disappeared into the background and it was as if he was in this new department, even before it had been officially created. Operations research was not related to the EES department at that time either.

**INT:** Tell me about the relationship between EES and OR.

**RH:** We were separate departments for many years (until 1996). OR was a big deal here at the time (the faculty included George Dantzig\(^8\) and Ken Arrow,\(^9\) among others), but we had a basic difference in philosophy. The OR group was basically very mathematical in their modeling, but generally lacked hands-on experience. I recall one time I was talking to a professor who was an expert in logistics and I asked him if he had ever been in a warehouse, and the answer was “no.” The professor was and is a valued colleague; I am just trying to point out the difference in philosophy. Our discipline was engineering and theirs was applied mathematics.

**INT:** Can you tell me a bit about your relationship with the Stanford Research Institute (SRI) and how that related to the formation of Strategic Decisions Group?

**RH:** Ironically, SRI was not on the Stanford University campus and, even though it was a very large institution, at the time had only four faculty relationships with the University—all in fields not related to EES or OR. Bill Linvill and I went to SRI (which I had known of from MIT) and met with some of their vice presidents, who liked my decision analysis ideas and Linvill’s innovative educational ideas. We formed the Joint Engineering-Economic Systems Program. Its purpose was to provide internships for our students and to help SRI stay abreast of our latest research.

Jim Matheson played a major role in SRI. I had met him in early 1964 when he was a graduate student at Stanford. At the end of a talk I gave on Markov processes, he came up and asked me a lot of very good questions. Based on that, I offered him a job at the MIT OR Center, where I was associate director. Unfortunately, he had already accepted a job with Westinghouse in Pittsburgh. However when it turned out that I would be at Stanford during the following academic year, he hired me as a consultant at Westinghouse, and we spent some quality time together.

When Linvill and I started the Joint Engineering-Economic Systems Program at SRI, I immediately thought of Jim. He joined SRI and soon became head of the Decision Analysis Group (DAG). I met Carl Spetzler at a technical conference in Mexico in 1966 and he soon joined us. Many decision analysis doctoral students had internships and employment in the group. Some of the work we did in the SRI DAG were pioneering applications such as the decision to seed hurricanes, planning of space missions, the advisability of investing in synthetic fuels, and a very interesting Mexican nuclear power plant decision. In the latter project, four engineers from Mexico spent a year here in Palo Alto, so that was an intensive project. The group also worked on commercial problems in research planning, investment, finance, and other areas of business very successfully from the mid 1960s through the late 1970s. I was always a consultant to the group, since my main responsibility was my research and teaching position at Stanford.

However, SRI was an organization that did not offer many attractive career paths for outstanding people. After gaining a few years of experience, there was a great temptation for people to leave the group and start their own companies in the decision analysis area. In 1978 Carl Spetzler took a position in a San Francisco office of a large firm called Resource Planning Associates (RPA).

After a short time, Carl Spetzler came back and asked Jim Matheson and me to join him at the new firm. We dragged our feet because we had done well for many years at SRI, but we could see the writing on the wall. Finally we agreed to join Carl at RPA, and

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\(^8\) George B. Dantzig (1914–2005) was a professor of operations research at Stanford University. He is known as the father of linear programming and is specifically credited with inventing the simplex method. For more information, see [http://en.wikipedia.org/wiki/George_Dantzig](http://en.wikipedia.org/wiki/George_Dantzig).

\(^9\) Kenneth J. Arrow (1921–present) is a professor of economics and a professor of operations research (now management science and engineering) at Stanford University. He was joint recipient of the Nobel Prize for Economics in 1972 and received the National Medal of Science in 2004. For more information, see [http://en.wikipedia.org/wiki/Ken_arrow](http://en.wikipedia.org/wiki/Ken_arrow).
Jim full time, I, as a consultant. There was one existing partner, Jeff Foran, at RPA, and with Carl, we would be the four partners of RPA in the San Francisco office. We then hired several members of the SRI DAG. We decided to move the office to Sand Hill Road to be near Stanford, with Carl as director of the office.

The change of firm initially went very well. We were able to hire the people we wanted and to do the work we wanted to do for our clients. Unfortunately, the president of the company, headquartered in Cambridge, Massachusetts, appeared to be ambivalent about our success. My personal opinion is that he saw the growth of our office as a challenge to his hegemony rather than as an important initiative of the firm. One issue that precipitated future events was our decision to hire one of my doctoral students as an intern. The president forbade us to hire the student because of his concern about the effect on our profitability. We did not share his concern and said that we, as the partners, would be happy to pay him out of our own compensation should there be any falloff in our business. Nevertheless, the president ordered Carl to rescind the offer and Carl refused, resigning as office director. We then began negotiations to separate our office from the firm as our own company. This was the start of Strategic Decisions Group, which focused on business decisions, as compared to the SRI DAG, which had a mix of business and government projects. This was a welcome change, as I had some rather unpleasant experiences with government applications.

**INT:** Can you give me a good example of a problematic government application?

**RH:** Oh, that’s easy. The best example was the situation with the Barnwell South Carolina Nuclear Fuel Reprocessing Plant during the Carter administration. The plant was impressive: you could literally see the money spent on it in the form of high-quality materials like half-inch-thick stainless-steel walls. The companies that built it had been under government assurances that the plant would be used. Well, Jimmy Carter won the presidential election and said “no reprocessing”—this plant became a white elephant. A senator from South Carolina called for a congressional investigation, which was undertaken. We were hired as part of a task force formed by the administration to analyze the decision of what to do with the plant. The main alternatives were to dismantle it or to put it in “mothballs.” Logically, the choice hinged on the probability the plant would be used in the future. The obstacle was that we could not consider the event of future use as uncertain because it would never be used under Carter’s policy. The fact that presidents served limited terms and could not control the future indefinitely was immaterial. Nonetheless, we did sensitivity analysis on that probability and produced some very good work that would probably cost over a million dollars today. Still, we were completely stifled. Every time we tried to present our findings, we were told we could not because, as a point of national policy, the plant was not to be used. It was a catch-22 situation: it was national policy that the plant would not be used, but the point of our study was to determine how to use the plant. Go figure.

One trip to Washington was particularly memorable. We were discussing our results with a government official in the executive branch who refused to be cooperative. We pointed out that the study had been commissioned by Congress, but he said that Congress could get “a study,” but that the administration would not pay any attention to it. As our report did not further the administration’s goals, there was not one comma of anything that we did in the final report. And, believe it or not, they were very satisfied and happy about our work.

From this experience, I developed four reasons to avoid working on government problems:

One, government decision-making is an oxymoron. Essentially, there is nobody home, nobody who is really going to make a decision. Analyzing such decisions can turn your brains to mush.

Two, the law of unintended consequences means that virtually any government program will have results that are the opposite of its intentions. Anyone who believes that Social Security will provide comfort in his or her golden years is probably eligible for mental treatment under Medicare.

Three, you are also being paid with stolen money, money stolen from the taxpayers who have provided

10 According to Jim Matheson, “The White House official actually said, in front of about 50 people, ‘You don’t understand. This is a White House study and we don’t give a damn about what Congress wants.’”
it out of fear rather than voluntarily. To me, it’s like
the Mafia hiring me to do something. I do not want
to work for stolen money.

Four, government consulting does not pay as well
as private consulting. This reason convinces people
who are not bothered by the first three reasons.

Anyway, I have not done any government work
for a long time as a result of Barnwell and similar
experiences.

INT: You say government work does not pay well.
Doesn’t it often pay better because of the big con-
tracts and because nobody is really watching what is
going on?

RH: Sure, but not for the best people. Working for
the government you are going to get brain rot. If you
can do anything well, you can do it for a higher rate in
private enterprise. I mean, any great consultant makes
much more than the president of our country. The
people who do better with the government contracts
are not the individual consultants, but the companies
that get the large contracts and sell the government
thousands of products at inflated prices.

INT: Can you explain the difference between a deci-
sion analyst and a decision theorist?

RH: The edifice of decision theory was built by
many theoreticians. Major contributors were Pascal,
Fermat, Bernoulli, Bayes, Laplace, and von Neumann.
The relationship of decision theory to decision anal-
ysis is that of physics to engineering. You cannot
talk about the founders of electrical engineering with-
out talking about physics. Decision analysis has its
roots in engineering. This influences the students
you attract, the precision of language and arguments,
the comfort with mathematical representations, and
its practical usefulness. Electrical engineers have to
believe their models because they cannot directly
sense the phenomena of voltages and the currents
in their designs. Decision analysts have learned that
mathematical representations of alternatives, informa-
tion, and preferences and their interaction phenomena
are useful in performing our trade. Engineers use the
knowledge of science to meet human needs. Decision
analysts use decision theory, knowledge of human
thought processes, and consulting skills to help peo-
ple and organizations make better decisions.

INT: Changing subjects a bit, what do you feel has
been your best technical achievement in your aca-
demic career? Or maybe what are your best works?

RH: I think other people should comment on that.

INT: But I’d like your opinion.

RH: OK, my best works I feel are what is novel,
for example, my work on dynamic probabilistic pro-
cesses in the early 1960s. I also like the work with Jim
Matheson and others on influence diagrams, and my
work on life-and-death decision making.

Influence diagrams actually go back to work spon-
sored by the Defense Advanced Research Projects
Agency many years ago on work on conflicts in the
Middle East. The reason influence diagrams became
necessary was that with decision trees you cannot
determine issues of probabilistic relevance unless you
look at the numbers in the tree. You need a magnifying
glass to say what information is represented and
how it gets modified by new information. The whole
influence diagram, decision diagram, etc., story is the
design of a graphical representation that on the one
hand is readily understood by client decision makers,
whether they be English majors or scientists and, at
the same time, is so logically formulated that it can be
understood by a computer. It has been a great aid in
solving the basic problem of transforming the clouds
in a person’s mind into a formulation that you can
actually compute.

INT: Did you ever publish any of the early work on
influence diagrams?

RH: Well, as you may know, we submitted our
work to Management Science in the mid 1970s. We
received reviews that we found unappreciative. So we
just published it ourselves (Howard and Matheson
1977). What is ironic is that this is probably one of the
most frequently used tools in decision analysis. The
paper was published in 2005 (Howard and Matheson
2005), and in 2007 it was a finalist for the Decision

INT: Influence diagrams are readily misunderstood.
Can you explain why?

RH: Well, I think that is because many diagrams
are used that look like influence diagrams, but are not.
Influence diagrams must be defined to be completely
unambiguous as far as the computer is concerned. For example, the relevance diagram contained within a decision diagram implies an assessment order of the probabilities to be assigned to the uncertainties. There cannot be loops or cycles in the sequence of arrows. If there is an arrow from a node representing uncertainty A to the node representing uncertainty B, that means that first a probability distribution on A will be assigned, then a conditional distribution on B given A. Sometimes people erroneously think that the arrow means “affects.” If A is price and B is quantity, they think price affects quantity and quantity affects price, so they draw two arrows between them, one in each direction. This diagram could never represent any possible assessment order.

**INT:** Moving to something else, can you explain the relation of your work to such groups as the Bayesian Research Group, founded by Ward Edwards?

**RH:** Well, Ward and his colleagues were in the field of cognitive psychology, although that is definitely related to decision making. We don’t really call ourselves Bayesians that much. I don’t really teach Bayes’ theorem per se; my students just know it. In other words, the whole structure of thought about probability and how you use it comes right out of Jaynes much more so than Bayes.

Ward’s work is very important to us because it makes us realize that people do not analyze decisions properly without instruction, and that they must avoid many biases and heuristics. Presentation becomes very important. For example, I was talking recently about the experiment where doctors will recommend different courses of treatment if you say x% will die as compared to (100-x)% will survive. One of our desiderata is if you get the same data, no matter how it’s presented, you ought to end up with the same conclusion. But that is not true of the way people normally think.

**INT:** So I think it’s fair to say that your work is normative. Yet, when we attend the decision analysis conference, we see a lot of people presenting descriptive work in a very mathematical fashion.

**RH:** No one said descriptive can’t be mathematical. As a matter of fact, that’s the beauty of it. You could make up any story that you like. But the ultimate test is, does it describe. The point I am making is, the only test of a descriptive model is how well it describes, not how complicated it is, not whether it’s mathematical.

**INT:** How important do you think it is for a decision analyst to understand the descriptive models? You don’t really teach much of it in your classes.

**RH:** In making my decisions, I’m always going to use normative methods. When other people (that I cannot influence) are involved, I’m going to want to use the best descriptive models available to predict their actions. There’s no conflict here between descriptive and normative. Normative is the way the decision should be made if one were consistent with the reasoning rules we discuss in class; descriptive describes the way that people actually behave due to their biases, heuristics, or simply their inability to properly process all the available information.

For example, if I am running a marketing campaign and I know that if I use the word “free” in the advertisements, I will sell more products, and then I will use the word “free.” As for myself, I hope I have conditioned myself that when I see the word “free,” I realize it may be an attempt to manipulate me.

**INT:** By the way, Amos Tversky, one of the giants of behavioral decision making and the coauthor of “Prospect Theory” (Kahneman and Tversky 1979), was a professor at Stanford. Did you have a close relationship with him?

**RH:** Well, yes, we did have a close relationship and got along famously. We would have seminars and invite outside speakers, and we would both comment on their work. What was nice about his being here was that he taught a course in cognitive psychology. I would send students to take his class and tell them, look, this is how people tend to behave.

Sometimes, when his teaching assistants took my class, they would tell me that Amos was advocating his work normatively. Now, to me he was always crystal clear that his work was descriptive.

Some people think the goal is to make the normative match the descriptive; to me that is a fool’s errand.

**INT:** In one of your answers above, you mentioned rules of decision making. Speaking of these, you have been known for the five rules of actional thought.
I believe the acronym for them is POES Choice. Can you explain?

RH: Yes, of course. The rules are as follows:

P is for the probability rule, meaning you must describe prospects by distinctions and assign probabilities to them.

O is the order rule, meaning you must be able to order your preferences among the prospects.

E is the equivalence rule, meaning for any three prospects at different levels in the ordering provided by the order rule you must be able to state a preference probability for the best versus the worst that would make you indifferent to the one in the middle.

S is the substitution rule, meaning you are indifferent to replacing any prospect with the equivalent deal established in the equivalence rule when the probability you assign is equal to the preference probability.

Choice means that if you prefer A to B and you have two deals each with a probability of getting A versus B, you should choose the deal that gives you the higher probability of A.

You can make any decision using these rules.

INT: I notice you have not used the phrase “maximizing subjective expected utility.” Why did you not use this term?

RH: The three things wrong with subjective expected utility are the words subjective, expected, and utility. I do not use those three words.

INT: What about the use of utility curves?

RH: The word “utility” has so many disparate meanings that I prefer to avoid it. I use u-curve to refer to the risk attitude on money.

INT: Can you tell us about the courses you teach? Everyone knows about the decision analysis courses, but do you teach any other courses?

RH: Yes, I teach two others, entitled The Ethical Analyst and Designing a Free Society. In the decision analysis courses, we focus on how to make good decisions. However, like any powerful tool, it can be used for good or ill. In the Ethical Analyst course, we help raise students’ ethical sensitivity both professionally and personally. In the Designing a Free Society class, we examine the part of your personal ethical code you are willing to impose on others by force, the legal system.

INT: On ethics, I noticed you have a new book on the market, Ethics for the Real World, with Clinton Korver (Howard and Korver 2008). Can you tell me a bit about this?

RH: This is the book Clint and I wrote to bring the lessons of the ethics course to a wider audience. It was published by Harvard Business Press last June.

INT: Speaking of applying decision analysis in the “right manner,” can you tell us a bit about the Decision Education Foundation (DEF)11 of which you are a founder, along with Carl Spetzler and others?

RH: Sure, I am very proud of the DEF and what we have done there.

INT: Please explain.

RH: We have set up a program to bring decision education to K-12 education. It has a professional staff, volunteers who have been consultants and academics in the field, and some very dedicated supporters. All are committed to improving the decision making of young people, especially teenagers. We want them to realize they have choices, to make reasoned decisions rather than quick, and often emotional, decisions that may have unintended consequences. The consequences can be getting into a fight, getting pregnant, dropping out of school, etc.

We have helped K-12 teachers develop lesson plans that incorporate decision analysis concepts in classes on English, history, and math. Think of being Macbeth’s decision counselor.

INT: That sounds great.

RH: It really is. I’d like to get even more people involved.

INT: Thank you so much for your time. It has been a pleasure interviewing you.

RH: You are most welcome.

11 For more information, see http://www.decisioneducation.org/. See also Abbas et al. (2004, 2007).
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