Operations Research

Publication details, including instructions for authors and subscription information:
http://pubsonline.informs.org

Decision Analysis: A Personal Account of How It Got Started and Evolved

Howard Raiffa,

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HOWARD RAIFFA

The Senior Center, Harvard Business School, Boston, Massachusetts 02138, hraiffa@hbs.edu

THE OPERATIONS RESEARCH ANTECEDENT

I am no historian, and whenever I try, I get into trouble. I invariably forget some insignificant figures like Newton, Pascal, or Gauss, or—what is a bit more tragic—I forget some really significant figure like the fellow whose office is next to mine. What I propose to do is much more modest: I am going to concentrate on myself—on who influenced me and why, how my thinking changed over time.

I was one of those returning G.I.s who resumed their college studies at the end of World War II. I studied mathematics and statistics at the University of Michigan in the late 1940s, and as a graduate student in mathematics, worked as an operations researcher on a project sponsored by the Office of Naval Research. In those days, OR was not so much a collection of mathematical techniques but an *approach* to complex, strategic decision making. Typically the decision entity was some branch of government—usually one of the armed services—and they were confronted with an ill-formed problem. Part of the task of the OR expert was to crystallize the problem and structure it in such a way that systematic thinking could help the decision entity to make a wise choice. My impression is that during the war, the OR practitioners were mostly engineers and physicists with only a sprinkling of mathematicians. The problems were usually complex and required some systems thinking. Embedded in these complexities were elements of uncertainty and of game-like, competitive interactions. The most practical analyses resorted to simulations to decide which strategies were reasonable. Very often, profound insights might flow using mathematical analysis not more sophisticated than high school algebra. Indeed, there was a bias against the use of advanced mathematics. The simpler the analytical tool, the better.

As the field of OR matured, mathematical researchers isolated key recurrent problems—like queueing problems—and used advanced mathematical techniques to get analytical solutions. The name of the game became simplification of reality to make mathematical analysis feasible. Then these analytically motivated abstractions were gradually made more intricate as the body of mathematical techniques grew. The trend went from elementary analysis of complex, ill-structured problems to advanced analysis of well-structured problems. In OR departments, mathematical elegance displaced the old quest for making empirically based contributions to messy real problems. A sign of maturity, I suppose.

In the early days of OR, descriptions of complex realities used probabilities and one did not worry too much if these were judgmentally based, but as the field "matured," probabilities were increasingly confined to the objective domain and interpreted as long-run frequencies. There was no hint of how best to elicit subjective, judgmental information from experts about uncertainties or in identifying and structuring multiple conflicting objectives.

I suspect that my continuing interest in *prescriptive* decision analysis had its roots firmly planted in that first academic type of job I had as a so-called "operations researcher." In that role, I was initiated into the cult that examined the world through decision-enhancing binoculars: *What's the problem? Who are the decision makers? What advice would I give?* A blatant, prescriptive, advice-giving orientation.

FROM OR TO GAME THEORY TO STATISTICAL DECISION THEORY

Back to my personal odyssey. In the academic year 1948–1949, I switched from the study of statistics to pure mathematics (at the University of Michigan) and as an OR research assistant working on detection of submarines, I read parts of von Neumann and Morgenstern's (1947) epic volume and started working in the theory of games. Meanwhile, in 1949–1950, the statistics seminar started working through Abraham Wald's (1947) book, *Statistical Decision Theory*, and I gave a series of seminars on that book—especially on those esoteric topics that made extensive use of game theory. About that time, a tragedy occurred: Abraham Wald was killed in an air crash over India and Jacob Wolfowitz, Wald's close associate at Columbia, took a post at Cornell, so what was left at Columbia in the domain of statistical decision theory were a few Ph.D. students with no faculty leaders. Because I reputedly knew something of Wald's work, I got my first academic appointment in

Subject classifications: Decision analysis: theory and practice, risk, sequential. Games and group decisions: negotiations. Professional: comments on.
Area of review: Anniversary Issue (Special).

the Mathematical Statistics Department at Columbia. I was supposed to guide those students of Wald, who knew more about the subject than I.

MY GRADUAL DISILLUSIONMENT WITH CLASSICAL STATISTICS

Besides teaching decision theory a lá Wald, I also had to teach the basic courses in statistics—the usual stuff on testing hypotheses, confidence intervals, unbiased estimation. These courses were mostly concerned with problems of inference, and little attention was paid to the integration of inference and decision. When I studied those problems more carefully, I felt that the frequency-based material on inference was not so much wrong but largely irrelevant for decisional purposes. I began not to believe what I was teaching.

It's not surprising that the rumblings against the Neyman-Pearson school were most pronounced at the University of Chicago, the home of the Cowles Commission, which mixed together mathematical economists (like Jacob Marschak and Tjalling Koopmans) with statisticians (like Jimmy Savage, Herman Chernoff, and Herman Rubin). It was the articulation of Rubin's sure-thing principle in a paper by Chernoff (1954) that led me to embrace the subjective school. My religious-like conversion did not come lightly, since all I was teaching about (tests of hypotheses, confidence intervals, and unbiased estimation) was, in my newly held opinion, either wrong or not central. But my colleagues in the statistics department were so violently opposed to using judgmental probabilities that I became a closet subjectivist. To them, statistics belonged in the scientific domain, and the introduction of squishy judgmental probabilities where opinions differed did not belong in this world of hard science.

The seminal book by Savage (1954) did not so much convert me to the subjectivist camp—I was already converted intellectually by the time I read this bible—but it convinced me that I was on the right track.

At Columbia, I was a member of the interdisciplinary Behavioral Models Project—actually, as the junior faculty member of the project, I was appointed the chair of that project—and Duncan Luce and I worked on what was supposed to be a 50-page expository article (one of many on different topics) and this turned into our book, *Games and Decisions* (Luce and Raiffa 1958). It took us two years to write and in those days, sans e-mail, we spent at most seven days working on the same coast of the United States. Luce was at the Stanford-based Center for Advanced Study and I was back at Columbia, or vice versa.

GO TO HARVARD OR STAY AT COLUMBIA?

In 1957, I received an offer to go to Harvard to take a joint appointment between their Business School (the B-School henceforth) and a newly formed statistics department. I had a tough time deciding between Columbia and Harvard because neither alternative dominated on all objectives. It was rumored that when I told one of my colleagues I was having trouble deciding, he quipped: "Trouble? But, Howard you are supposed to be an expert in making decisions!" "But," I supposedly answered, "this is for real!" Nope, this is not true. I never said such a thing. Actually, my wife and I subjected this decision to a primitive, multiple-value analysis involving 10 objectives. In the formal analysis, Harvard won easily.

Splitting My Time Between Statistics Department and the B-School

I knew so little about business, that in my mind I planned to spend far more time in the statistics department than at the B-School. But the opposite time allocations prevailed for two reasons: (1) My introduction to the case method, and (2) Robert O. Schlaifer.

I was introduced to the case method of instruction, and it blew my mind. The MBA student prepared two or three different cases each day, and most of these were *real* decision problems—each replete with competing objectives, with loads of uncertainties, and all embedded in a game-like environment. A veritable treasure trove of real-world examples that cried out for theoretical attention. *All those cases to be mined*, and I had the wrong set of tools. And then I met the amazing R.O. Schlaifer.

Schlaifer was hired by the B-School as a flexible resource who was smart enough to learn and teach any one of a number of subjects. For one semester he taught marketing, then financial control, and then, because the B-School's only statistician retired, Schlaifer was called in to fill that void. He was trained as classical Greek scholar and never had a previous course in statistics or any mathematics beyond one semester of the calculus. So he read the classics in statistics and independently, without any outside coaching, decided that Fisher and Neyman and Pearson had it all wrong: The way to go was to closely knit the study of uncertainties with business decisions. He also was not aware of the rumblings in the field. So he began teaching his own version of statistical decision theory based on subjective probability assessments of expert, business managers—making it up as he went along. I recognized genius when I saw it and I became his personal mathematics tutor—the best student I ever had—and we worked together on developing statistics for an astute business manager.

From Closet Bayesian to Proselytizer

It didn't take long, working with Schlaifer, that I too got religion and gained the necessary conviction that the subjective school was the right school—certainly for business decisions—and I came out of the closet and began preaching the gospel according to Bayes's disciples.

It was now 1958, and we were convinced that our mission was to spread the gospel. Even statisticians who were on our side in the philosophical debates about the foundations, were skeptical about implementing it. They felt: (*1*) *The subjective (Bayesian) approach is all too complicated, and* (*2*) *real experts won't cooperate by giving judgmental information.*

In 1958, Schlaifer and I set out to prove that whatever the objectivists could do, we subjectivists could also do—only better—and in 1961, we published a compendium of results entitled *Applied Statistical Decision Theory (ASDT)* (Raiffa and Schlaifer 1961). It was much more a reference volume than a text book. We deemed our efforts a success.

At the same time, during this period, we and some of our students did get out into the field and collected judgmental probabilities galore from real experts about real events. (A good many of these experts were design engineers and marketing specialists from DuPont and Ford.) We proved to ourselves that we were on the right path. We were not troubled by the problem of expert cooperation but about the quality of their judgmental inputs. We learned how to ask for the input data we needed by the proper framing of questions. We told our experts to expect incoherencies in their responses to our hypothetical questions and those incoherencies should only prompt them to think even more deeply about their expertise. We learned not to ask questions one way or another way, but to ask both ways and to confront resulting incoherencies in an open manner. We learned that the experts calibrated better if questions were posed in terms of assets rather than in incremental monetary amounts and that by and large their judgmental inter quartile ranges were too tight—rather than capturing half the true uncertain quantities, they were capturing only a third. We became experts in the art of soliciting judgmental information. I'm reminded here of the complaint that it seems wrong to build a logical edifice on such imperfect input data, to which Jimmy Savage responded, "Better to construct a building on shifting sands than on a void." Here, the "void" being no use of judgmental inputs.

Our book, *ASDT*, had at best a very limited circulation when published in 1961, but in the year 2000 it was republished by Wiley in their Classic Series.

Decision Trees

Because many of our business students were bright but mathematically unsophisticated, I formulated most problems in terms of decision trees, which became very standard fare. The standard statistical paradigm, involving a decision whose payoff depended on an uncertain population parameter, was presented in a four-move decision tree:

Move 1 (decision node). Choice of an experiment (e.g., sample size).

Move 2 (chance node). Sample outcome.

Move 3 (decision node). Choice of a terminal act.

Move 4 (chance node). Revelation of the true population parameter.

Associated to any path through the tree was a known payoff value.

When I taught objectivist-based statistical decision theory at Columbia, I had little need for the decision tree because objectivists shunned the use of any probabilistic assessment at Moves 2 and 4. The only probabilities the objectivists would allow were conditional probabilities of sample outcomes given assumed population values. The subjectivists use not only this class of probabilities, but also prior probabilities over the population parameters—*prior* meaning before the revelation of sample evidence—and from these inputs and the use of Bayes' formula the subjectivist can derive appropriate probabilities to enter at Moves 2 and 4. Hence the appellation of "Bayesian." I got so used to the use of decision trees in communicating with my students that I couldn't formulate any problem without drawing a decision tree and I was referred to as "Mr. Decision Tree," and what I did was "Decision-Tree Analysis." It took a few years to drop the "tree" from that appellation.

FROM STATISTICAL DECISION THEORY TO MANAGERIAL ECONOMICS (1961–1964)

The three years from 1961–1964 were frenetic, and in retrospect, quite productive. First, Schlaifer and I were joined by John Pratt and we wrote a more user-friendly version of *ASDT* for classroom consumption, replete with hundreds, if not thousands, of exercises and caselets. McGraw-Hill distributed a preliminary version of this unfinished book (1965), all 1,000 pages, in a cardboard blue binder and the book was called the "blue monster" or *Introduction to Statistical Decision Theory* (ISDT). It was not published as a finished book until a delay of four decades, only after Schlaifer's demise in 1994 and after my retirement in 1994, and the reason is a bit crazy in retrospect. We (Schlaifer and I, and not Pratt) simply lost interest in pursuing the standard and much too binding statistical decision theory paradigm. We no longer thought of ourselves as applied statisticians but as "managerial economists." We thought the transformation in outlook was profound, and no longer did we have time to polish a book that was too narrow in scope. After publishing *ASDT*, I had time to study case after case in the MBA repetoire of cases, and it was crystal clear that managers galore needed some systematic way of thinking about their decision problems, which were overwhelmingly concerned with uncertainties that needed the judgmental inputs of managers. The statistical paradigm was out of kilter, was hobbling. A typical business decision problem might involve several uncertainties, and some of them required the judgments of production managers or marketing specialists or financial experts with no possibility of accumulating sampling evidence. To force those problems into a statistical decision format was too cumbersome. At about that time, I also supervised the thesis of Jack Grayson, who was interested in describing how oil wildcatters made decisions. I got him to adopt a more prescriptive orientation that dealt not only with physical uncertainties—is there oil down there and how much—but of the sharing of risks and the formation of syndicates (Grayson 1960).

Schlaifer not only agreed with my assessment for the need of paradigmatic shift in emphasis away from the statistical model, but in his inimitable style he went to the extreme and would have nothing to do with that blue monster that was holding back progress. We became "managerial economists." In a prodigious effort, he developed, without my involvement, a case-based MBA course in managerial economics for all our 800 entering MBA students. I spent my time on other things, but my admiration for Schlaifer remained steadfast.

FROM BUSINESS AND STATISTICS TO BUSINESS AND ECONOMICS (1964–1968)

I received an offer I could hardly refuse: a joint chair endowed by the Business School and the Economics Department, and because these two behemoth organizations could not agree on a name for the professorship, I was invited to select a name. I became the Frank P. Ramsey Professor of Managerial Economics. I never had a course in economics and was a bit apprehensive, but they appointed me because they realized, as I did, that the theory of risky choice should be an integral part of economics. I also must admit that I needed to establish some space between Schlaifer and myself.

From Managerial Economics to Decision Analysis

Although I now was a member of the Economics Department and my title dubbed me a managerial economist, I increasingly became interested in classes of problems that had little to do with management or economics—problems in governmental policy, in science policy, in public health, in clinical medicine. I no longer thought of myself as a managerial economist but more generally as a *decision analyst.*

Starting in 1964, in the Economics Department, I taught a graduate level course in decision analysis and started preparing material for a book under that title. Much later, I learned that Professor Ronald Howard of Stanford, one of the key developers of the field now called decision analysis, had independently adopted that name for his enterprise (Howard 1966). Evidently the time was ripe. I essentially taught the same thing in the Economics Department that I taught at the B-School, but the cases were different. Instead of maximizing expected profits, the objective functions became more complex and group decision making needed more attention.

My RAND Experience and the Development of Multi-Attribute Utility Theory (MAUT)

I was invited by Charles Wolfe to spend the summer of 1964 at RAND studying and critiquing the methodology used in their reports. Not infrequently, I found that not enough attention was given to the recognition of competing, interrelated objectives of the analysis, and this resulted in a misspecification of the objective function to optimize. Also all too often, the hard drove out the soft. I wrote a RAND report on decisions with multiple objectives, and in that report I first introduced notions of preferential and utility independence (Raiffa 1969). With my very capable doctoral student Ralph Keeney, we further expanded on these primitive RAND results and we—mostly Ralph—helped develop the field of multi-attribute utility theory (MAUT) (see Keeney and Raiffa 1976).

Clinical Medicine

In the mid-1960s, I worked closely with the Chief of Medicine at the New England School of Medicine, Dr. William Schwartz. Bill was an enthusiastic convert to decision analysis and wanted physicians to be trained in its use for diagnostics and treatment of individual patients in real time. If he had his way, all doctors would be required to take a course in decision analysis. He himself, with a hand calculator, made the medical rounds surrounded by a half a dozen eager students, and he would all but lay out a decision tree and work his way backward. I had more modest aspirations: I wanted a group of medical research specialists to write textbooks about the desirable treatment of categories of illnesses and to support their analyses through the use of detailed decision analyses. I envisioned standard reference books with lots of decision trees in the appendixes. In 1970, I think I had the better of the debate, but with the astounding development of computing capacity and speed and with the miniaturization of computers, Bill Schwartz has been vindicated. Today there is a professional society for medical decision analysis that is more than 20 years old.

FROM BUSINESS AND ECONOMICS TO BUSINESS AND THE KENNEDY SCHOOL OF GOVERNMENT (1966–1970)

As a member of the Department of Economics I was asked by President Bok of Harvard to comment on a report that advocated a program for the revitalization of the School of Public Administration at Harvard. I criticized that report as suggesting a watered-down applied economics program with a little public policy thrown in. I suggested the development of a new School of Public Management patterned on the model of the B-School. I opined that the new school should have its own faculty, lay primary stress on a professional master's program, feature about 50%–75% of its instruction by the case method, and have its own mini-campus. In rapid order, I became part of a committee of four that designed the new Kennedy School of Government, and I transferred the economics share of my joint professorship from the Economics Department to the newly formed Kennedy School of Government. Decision analysis became part of the core curriculum for the master's degree in public policy, and a required course in statistics was initially taught with a distinctly Bayesian orientation. That is no longer the case. New faculty, new curricula; what professors teach is primarily dictated by what they were taught.

THE DECISION AND CONTROL NONPROGRAM (1965–1975)

Harvard never had a department or center in operations research, and there was no substantial support to create such a center. So a few of us dedicated faculty decided to act on our own. Without any clearance from the faculty of arts and sciences or the B-School, a dozen of us met informally and mapped out a coordinated set of eight one-semester courses in what we dubbed *decision and control* (D&C) to be given regularly and spread out over three departments: the Division of Engineering and Applied Physics (now the Division of Applied Science), the Department of Statistics, and the Department of Economics. The courses were designed to be given in different departments, all with the same strong mathematical prerequisites, and care was taken to avoid scheduling conflicts. The courses were decision analysis, statistics, probability models, game theory, mathematical programming, control of dynamic systems, and an integrative seminar. There was no budget, no formal committee, no secretary, no request for any approval of any faculty. Ph.D. students who took these coordinated sets of courses were enrolled in different departments. It was all low key, and students who completed this program were given a letter from me, as the honcho organizer, attesting to their superior training. Imagine—it worked like a charm for a whole decade. It took a bit of chicanery to have these eight courses listed together in the division's course catalogue.

The D&C program worked, but not perfectly because we had no fellowship money to compete with Stanford or Chicago for talented students. So a group of us fought in the 1980s for the creation of a funded Ph.D. program in the decision sciences. We succeeded to get the arts and sciences faculty to approve this program, and initially the B-School funded the program to sustain a yield of four or five doctorates a year. But the B-School withdrew its support a few years later because not enough of these completed Ph.D.s chose a business school career; several opted for public policy or public health or the environmental sciences. The Ph.D. in the decision sciences is still on the books as a Harvard program, but it desperately needs to be funded and reinvigorated. More about this later.

ON BECOMING A NEGOTIATION ANALYST

Negotiating the Creation of IIASA (1967–1972)

From 1967–1972, I was a member of the U.S. team that negotiated with the Soviet Union and 10 other countries, both east and west, in establishing the International Institute for Applied Systems Analysis (IIASA). This took place during the height of the Cold War and was a confidence-building gesture. I learned a lot about the theory and practice of many-party negotiations in the presence of extreme cultural differences.

Directing IIASA from 1972–1975

As the first director of IIASA, I continued in my apprenticeship role as negotiator but I added the skills of an intervenor in disputes (facilitator of group interactions, mediator, and arbitrator). I had, I believe, some natural talents, but it was a self-discovery method of learning. I lacked any prior expertise from a program of training in the art and science of negotiation. In 1975, I decided to return to Harvard to learn about negotiations, rather than continuing at IIASA or becoming a dean at some other university.

Back to Harvard and Learning about the Art and Science of Negotiation (1975–1981)

I taught a very popular course on negotiations at the B-School in the late 1970s that was for me a laboratory with well-motivated student-subjects. The students were partially graded on how well they did on various simulation exercises, and I learned enough about negotiations to write a book about it (Raiffa 1982). This was not decision analysis, but perhaps a claim can be made for its inclusion in the broader field of decision sciences. After all, negotiations are all about group decision making.

The Program on Negotiation (1982–)

In the early 1980s, Roger Fisher, coauthor of *Getting to Yes* (Fisher and Ury 1981) and I—more Roger than myself—established another administrative innovation. We created an interdepartment, interuniversity consortium of academics interested in negotiations (broadly interpreted) and launched the Program on Negotiation (PON), located—note the delicacy of language—located at (but not part of) the Harvard Law School. PON is not an integral part of Harvard's infrastructure and draws no funds from the university. Initially, it was funded by the Hewlitt Foundation, but for the last 10 years it earned its own way by offering executive educational programs with largely donated faculty time. It is financially secure enough now to offer predoctoral and some post-doctoral research fellowships, run a clearinghouse for the distribution of simulated exercises, sponsor a journal on negotiations, offer a prestigious award for the Negotiator of the Year, and run ongoing seminars.

FROM DECISION ANALYSIS TO POLICY ANALYSIS AND TO SOCIETAL RISK ANALYSIS

Nuclear Energy Policy Study (NEPS)

When I returned to Harvard from IIASA in 1975, I was invited by McGeorge Bundy, then president of the Ford Foundation, to join a group of scholars to prepare a report for the incoming president of the United States—it turned out to be Jimmy Carter—on what to do about nuclear energy. For the purposes of this article, there were two lasting impressions from my NEPS experience: (1) It served as a model for me about how to structure facilitated group interactions leading to a group report—not necessarily to group consensus; and (2) it kindled my interest in really broad public policy issues and how an analyst like myself could contribute to such an endeavor. Is this decision analysis or societal risk analysis? It is not statistical decision theory or managerial economics. It certainly belongs in a broad program of decision sciences.

The Committee on Risk and Decision Making (1980–1982)

Following my experiences at IIASA, following my participation in the NEPS group and my ongoing involvement in an independent committee overseeing the Three Mile Island cleanup, in 1980 I chaired the Committee on Risk and Decision Making (CORADM 1982) for the U.S. National Research Council of the National Academy of Sciences. The committee's task was to report about the status of societal risk in the United States, about how large societal risks are handled and how they might be handled better, and how we should be preparing future analysts to cope with these problems. I found my task daunting and in the end completely frustrating, although along the way the experience was absolutely fascinating. Besides my disappointment with the discontinuance of the Ph.D. program in the decision sciences at Harvard, my CORADM experience was my only other major disappointment in my professional life. I give myself only a marginally passing grade as facilitator, mainly because the members of the committee were not completely engaged in the problem—after all they had their own responsibilities back home—and I was too involved in the substance to be both chairman and facilitator of meetings. Along with the help of John Graham, a master's degree candidate at Duke who has had a distinguished career since that time, I wrote a draft of a behemoth report of close to 700 pages on the deliberations of our distinguished committee. That report had difficulty with the peer review system, and the only tangible product was a watered-down report of about 50 pages. It didn't say what I thought should have been said, and the pressures of time kept me from fighting the system. Secretary Ruckelshaus, who was a member of the CORADM and who later became the leader of the EPA, was most enthusiastic about the original draft report, and he adopted several of the recommendations found in that report. Some comfort can be taken from the observation that each reviewer professed different reasons for their unhappiness. The satisfying part of that experience is that I learned a lot about the group decision-making processes—a lot of what should and should not be done.

POST-RETIREMENT REFLECTIONS

Decision Making—A Critical Life Skill

I completely missed the boat when I published *Decision Analysis* (Raiffa 1968). I was so enamored of the power and elegance of the more mathematical aspects of this emerging field that I ignored the nonmathematical underpinnings: how to identify a problem or opportunity to be analyzed, how to specify the objectives of concern, how to generate the alternatives to be analyzed. All this was given short shrift. All that nonmathematical starting stuff was ignored. Well, Hammond et al. (1999), in *Smart Choices*, try to correct that. It reaches out to the general public and offers coaching advice to practically everybody on how to make better personal and workplace decisions.

A New Look at Negotiation Analysis

The boundaries between individual decision making (decision analysis), interactive decision making (game theory), behavioral decision making, and joint decision making (as in negotiations) should be porous but aren't. A partial synthesis of these strands is attempted in Raiffa (2002, in press). How negotiators and intervenors (facilitators, mediators, arbitrators) do and should behave (mostly should) in seeking equitable joint gains in exploiting deals as well as in resolving disputes. For example, a negotiator who has to decide whether he or she *should* continue negotiating or break off and pursue an independent strategy (a prescriptive orientation), typically must address an individual decision problem in an interactive (game-like) environment, not knowing what others might do (a descriptive orientation).

The Need for a New Field Called Decision Sciences

I truly believe that decision making—both individual and group; descriptive, normative, and prescriptive—is an important life skill that can be and should be taught broadly in our society. I think the right umbrella term for what I have in mind is *decision science*, and I hope that in your lifetime, if not mine, there will be departments of decision sciences created in our universities that will give undergraduate and graduate courses in this subject with many, many electives. Game theory (extended far beyond equilibrium theory) and negotiation theory (broadly interpreted to include conflict management, resolution, and avoidance, as well as the growing field of alternate dispute resolution) should be a part of this developing discipline. In this department of decision sciences there should be courses on societal risk analysis, and even on organizational design and on the structures of constitutions. There's a lot to be taught and a lot more to be learned: two prerequisites for a field of study. The decision sciences department should establish strong ties to the professional schools (especially business, public policy, public health, medicine), to the engineering school, to the departments of economics, psychology, government, mathematics, statistics, philosophy, and especially to the school of education. So let's get on with it.

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