The Scientific Status of Geometric Models of Choice and Similarities Judgment

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Introduction

The title of this essay is "The Scientific Status of Geometric Models of Choice and Similarities Judgment." I deliberately use the word "geometric" rather than "spatial" because "geometric" is a much more precise description of what those of us who work in this area do. In addition, another sub-area of statistics uses the term "spatial model" to mean what is literally a *geographic* model; for example, using geographic contiguity to capture similarities in the behavior of voters in legislative districts.

I believe that research on geometric representations of human choice and similarities judgment, encompassing work in Psychology, Political Science, Economics, and other disciplines, is a classic example of scientific progress. I believe that we, and by "we" I mean from now on all of us who work in the area, have developed a body of scientific knowledge using the scientific method. Although I will offer a bit more elaborate argument below, consider the dictionary definitions of scientific method and scientific knowledge. Under the word knowledge Webster's Dictionary (1989) defines scientific knowledge as "a branch of knowledge or study dealing with a body of facts or truths systematically arranged and showing the operation of general laws." Have we shown the operation of general laws? Yes, I believe we have.

Webster's defines the scientific method as "a method of research in which a problem is identified, relevant data are gathered, a hypothesis is formulated from these data, and the hypothesis is empirically tested." It seems to me that there can be little argument about the fact that we have produced many books and journal articles that are exemplars of the scientific method including the papers being presented at this conference.[1] I believe this to be so because of the close correspondence of the work on multidimensional scaling methods in Psychology with the development of geometric theories of choice and similarities in Political Science and Economics. Both approaches have reached essentially the same conclusion – low dimensional geometric maps accurately summarize similarities and preference judgments. But the two fields use different paradigms to reach this end point. The geometric model is based upon Rational Choice Theory while the Psychological approach is based upon Cognitive Process Theory. I will discuss each in turn.

Rational Choice Theory

Bill Riker was on my dissertation committee so you would expect that I would say that we (collectively) are doing science. His goal was to build a science of politics and he was not shy about it. Everyone took Riker's Positive Political Theory course when I was a graduate student at the University of Rochester in the early 1970s. The book we all read was Riker and Ordeshook's (1973) *An Introduction to Positive Political Theory*. On page 1 they make their goal very clear: "Here we begin to build a positive theory of politics…We assume that … people behave as if they sort out and logically arrange the preferences in their bundles. Furthermore, we assume that they are able to use the sorted bundles for decisions so that, when faced with a choice, they can choose as directed by their preferences. Thus, the people in our model have will and reason and use these abilities to make decisions."

Within economics the rational choice paradigm allowed the development of classical microeconomics. The economists always had an advantage over Political Scientists because they had a unit of measurement of individual utility – money. Money solves the problem of the interpersonal comparison of utility. Add the additional assumption that people prefer more to less money with, perhaps, diminishing marginal returns, and the result is a rich body of theory that predicts the outcomes of the aggregated actions of individuals. Thus methodological individualism and the assumption that individuals' actions are *purposeful* have led to the development of a science of economics.

Unfortunately for Political Science typically there is no way to finesse the problem of interpersonal comparison of utility. Simply put, there is no "money". We can observe individuals' choices but we cannot realistically measure how much they care about their choices. This has made the application of rational choice theory difficult and controversial within Political Science.

I have always found this controversy a bit puzzling. It strikes me that the core of the "Rochester approach" is the assumption that an individual's action are *purposeful*; that is, an individual's actions are directed toward achieving a goal. Ferejohn and Satz (2002) argue that "…it is hard to see how we could come to understand, predict or criticize the actions of other people in our environment without something like a belief-desire psychology that permits us to see actions as taken for reasons of certain kinds." Furthermore methodological individualism coupled with the assumption of purposeful action is not incompatible with the behavioralist tradition. Group actions are understandable in terms of individual choice and we can understand social processes and outcomes in terms of people's preferences and choices.[2]

Complicating matters even further is the very artificial division between economics and a large body of Political Science. I taught American Economic and Business history for many years at Carnegie-Mellon University. Teaching economic history convinced me that political and economic history could (and should) not be separated. Indeed, Politics and Economics cannot be separated because "… the state establishes the context in which markets operate and stands ready at any time to upset any particular market outcome" (Ordeshook, 1990). The State sets the ground rules for the economy and there is a clear feedback between the two. Entrepreneurial activity creates new goods and services that change the structure of the economy and produce political effects. Classic examples are the railroad and the telegraph. The political system responds with regulation and taxes thereby changing the structure of incentives.

Cognitive Process Models[3]

Cognitive process models attempt to represent similarities and choice behavior as a function of how the mind processes external information and translates it into action. These models are typically based on theories of cognition that describe how knowledge is accessed, represented and manipulated in our minds. "Objects that are perceived or remembered receive some internal representation. Various cognitive processes are then assumed to act upon that representation. The particular processes that operate are task dependent -- they will vary depending on whether subjects are asked to discriminate among objects, identify, categorize or recognize them, supply similarity ratings, make preference judgments, and so forth. Thus, to understand performance in tasks involving similarity data requires not only the specification of an underlying similarity representation, but also the cognitive processes that act on that representation" (Nosofsky, 1992, p. 25).

Within Psychology, surveys and experiments over the past 50 years of how people make similarities and preferential choice judgments show that very simple geometric models appear to structure responses to these tasks (Shepard, 1987). When individuals make a judgment of how similar two stimuli are, they appear to base the judgment upon how close the two stimuli are in an abstract psychological space (Nosofsky, 1984; 1992; Shepard, 1987; Gluck, 1991). The dimensions of these psychological spaces correspond to the attributes of the stimuli. A strong regularity is that these psychological spaces are low dimensional – very rarely above two dimensions – and that either the stimuli judgments are additive – that is, a city-block metric is being used – or simple Euclidean (Garner, 1974; Shepard 1987; 1991; Nosofsky, 1992).

Another strong regularity is that the reported similarities are an exponential function of the distance in the abstract psychological space. That is, if d is the distance between two stimuli then the reported similarity tends to be e^{-kd}, where k ($k > 0$) is a scaling constant (Shepard, 1987; Nofosky, 1992; Cheng, 2000). In the Shepard-Ennis-Nosofsky model of stimulus comparison the individual compares two stored *exemplars* (model; pattern; a typical case) in psychological space and reports the distance between them. "The similarity between two exemplars is computed according to a rule that assumes similarity decays exponentially with increasing distance in … psychological space" (Gluck, 1991, p. 50). Ennis (1988a) shows that if these exemplars are treated as multivariate normal distributions in psychological space so as to allow for "irreducible noise in the perceptual/memory system" (Shepard, 1986, p. 60) then the expected value of the reported distance has a Gaussian distribution. That is, if there is some perceptual error, the expected value of the response function tends to be Gaussian, that is $e^{-k\delta}$, where $\delta = d^2$ (Shepard, 1987; 1988; Nofosky 1988; 1992; Ennis, 1988a; 1988b; Ennis, Palen, and Mullen, 1988).

The evidence compiled in Psychology on how individuals make similarities judgments produces results that are very similar to the geometric models of choice used to analyze legislative roll call voting. This is not surprising because the economists' notion of preference can be *reduced* to the psychologists' notion of similarity. Let Ms. Smith prefer **A** to **B** when 1) Ms. Smith has an ideal standard, $\mathbf{X}_{\mathbf{smith}}$, against which she judges stimuli of type **A**, **B**; and 2) Ms. Smith judges the similarity between $\mathbf{X}_{\mathbf{smith}}$ and **A** to be greater than the similarity between $\mathbf{X}_{\mathbf{smith}}$ and **B**.[4] In this regard, geometric models of choice are a subspecies of the similarity model.

Specifically, the random utility model with a normal distribution utility function and random error is essentially the same as the Shepard-Ennis-Nosofsky model of stimulus comparison. Namely, in a roll call choice situation, the individual is comparing her ideal point to the Yea alternative *and* comparing her ideal point to the Nay alternative. In each comparison the individual's utility plays the role of a similarities judgment. By definition, the expected value of this utility when plotted against the distance between the ideal point and the outcome location is Gaussian. In the Shepard-Ennis-Nosofsky model, when the expected value of the reported similarity is graphed against the true distance between two stimuli the response function is Gaussian. Even with a quadratic deterministic utility function there will be a reasonably close correspondence between the two models.

This correspondence between rational choice theory and cognitive process models means that the low dimensional geometric maps that we are producing for preferential choice data such as parliamentary roll calls and for similarities-type data formed from legislative cosponsorships or political rhetoric have a sound scientific basis. Before summarizing the case we need to briefly look at the controversy of just what science *is*. The controversy over the nature of science has had a tremendous impact on the social sciences, arts, and humanities in the past 50 years.

What is Science?

Thomas S. Kuhn's *The Structure of Scientific Revolutions* (written in 1962 with 2nd edition in 1970 with a 1969 postscript) was all the rage in the early 1970s when I was in graduate school. Kuhn made the concepts of paradigm (normal science), paradigm change, and incommensurability part of the discourse about science. The standard view of scientific progress is that scientists figure out how nature works and that scientific knowledge is cumulative. Old theories like Newtonian Mechanics give way to more accurate and comprehensive theories like Quantum Mechanics. Past scientific advances eventually lead to technological wonders like the electronic computer, fiber optics, and the wide range of "gee whiz" devices that populate our world.

The problems with this standard account are that the new theory does not necessarily contain the old theory as a logical subset. In addition, because creating (or discovering – see below) a new theory is an inductive process no deductive account of how a new theory can be produced from an old theory is possible. Rudolph Carnap (1995, p. 33) states the problem very clearly: "…theories, especially the more abstract ones dealing with such nonobservables as particles and fields, use a conceptual framework that goes far beyond the framework used for the description of observation material. One cannot simply follow a mechanical procedure based on fixed rules to devise a new system of theoretical concepts, and with its help a theory. Creative ingenuity is required….there cannot be an inductive machine – a computer into which we can put all the relevant observational sentences and get, as an output, a neat system of laws that will explain the observed phenomena."

Indeed, Kuhn denies that scientific progress consists of producing a "better representation of what nature is really like." "[T]he notion of a match between the ontology[5] of a theory and its "real" counterpart in nature now seems to me illusive in principle." "Rather scientific progress consists of producing "a better instrument for discovering and solving puzzles."[6]

Kuhn denies that he is an epistemological relativist (p. 207) but he clearly believes that there is "no coherent direction of ontological development" of scientific theory. It is a short step from Kuhn's nuanced view of scientific change to a pure relativist position/social constructionist view of science.[7] The premier argument for the social constructionist view is Andrew Pickering's comprehensive history of particle physics from 1945 to 1980. Pickering analyzes the transition from the "old" non-quark high energy physics of the 1960s to the "new" quark-based high energy physics of the 1970s. Pickering in no uncertain terms concludes that Kuhn's view of incommensurability is correct. "Kuhn's argument was that if scientific knowledge were a cultural product then different scientific communities (separated in space or time) would inhabit different worlds" (Pickering, 1984, p. 407). He argues that this exactly describes the transition from the old to the new physics. Contrary to Kuhn, however, Pickering finds that the transition was universally accepted. "The prevailing climate within [High Energy Physics] during the 1970s was one of mutual congratulation rather than recrimination" (Pickering, 1984, p. 411).

Pickering's findings about the shift from the old to the new physics is not unusual. Larry Laudan (1984; 1996) makes the telling criticism of Kuhn that in the history of science paradigm debate is brought to closure relatively quickly. Empirical knowledge builds up to the point that there comes a time when rational disagreement becomes impermissible. In the short run there is disagreement about paradigms. In the long run it is not true that "anything goes" – that is, the view that there is no rational justification for one paradigm over the other and hence no scientific "progress" (e.g., Feyerabend, 1975). For example, if it were true that "anything goes" why don't the losers of the paradigm debate start their own journals (e.g., *The Journal of Cartesian Physics; The Journal of Newtonian Mechanics;* or *The Journal of Phlogiston Theory*)?

Scientists themselves generally reject the viewpoint that what they do is "relative" or "socially constructed." But the acceptance of relativism and its variants social constructionism and multiculturalism is widespread in the Humanities, the arts, and many of the social sciences. Why should this be true? A clue lies in the work of Jacques Barzun.

Jacques Barzun

Jacques Barzun is 100 years old[8] and lives in San Antonio, Texas. From 1955 to 1968, he served as Dean of the Graduate School, Dean of Faculties, and Provost at Columbia University. A cultural historian, Barzun summarized his life's work in 2000 with the publication of his best seller *From Dawn to Decadence: 500 Years of Western Cultural Life*. What I find particularly illuminating about Barzun's great book is his account of the changes in high culture in the two decades before World War I and the effect of the War on what followed (pp. 615 – 744); namely, what he dubs "the death of the philistine" (p.713). Barzun is concerned with high culture but his account meshes perfectly with Barbara Tuchman's description in her book *The Proud Tower* (1966) of the social milieu of the political elites of Europe and American before the Great War.

Barzun argues that the emergence of Impressionism and then Cubism after 1908 (he calls 1905 – 1914 "the cubist decade") inspired furious opposition within the high art community itself. The proponents had to defend their work against the critics – the philistines. All this changed with the Great War.

Before World War I there was no system of visas or passports as we now understand them. Middle and Upper Class people could travel from New York to Liverpool and simply walk off the ship. Indeed, the irony of this system was that large amounts of capital in the form of stocks and gold denominated bonds were carried across the Atlantic for investment without any tariffs being paid. Men such as Andrew Carnegie became wealthy bond salesmen by regularly traveling between America and Europe (Wall, 1970/1989; Livesay, 2007). The same held within Europe itself. The elites traveled freely between the great capitals of Europe – Paris, Berlin, Vienna, Prague, St. Petersburg, and London. As both Barzun and Tuchman discuss, this free movement of the elites gave them a sense that they "were on top of the World" and they had solved the major problems of the age. The rise of industrial capitalism had produced great affluence which in turned fueled high culture. This whole World collapsed into Hell after August, 1914.[9] World War I was the asteroid that struck Western Civilization. "When the [War] was over, illusions and enthusiasms possible up to 1914 slowly sank beneath a sea of massive disillusionment. For the price it had paid, humanity's major gain was a painful view of its own limitations" (Tuchman, 1966, p.463).

The War shattered the unity of the cultural elites who turned to enthusiastic support of their various nations' war efforts. Barzun argues that it also ended the traditional role of the philistines. "By 1920 any that survived had been miraculously transformed, not into aesthetes but into trimmers and cowards. To this new breed anything offered as art merited automatic respect and grave scrutiny" (p.713). In short, *anything goes*. Although some will argue that there is "good" art and "bad" art it is difficult to escape how *relative* those terms are.

Kuhn's work stands alone, of course, but it is hard to imagine his work having the influence it has had without the relativism that pervaded high culture for decades prior to the publication of his work. The two are deeply related in my opinion but that line of thought would take me too far afield. Instead, let us consider the role of Kurt Gödel and Albert Einstein in these controversies.

Kurt Gödel and Albert Einstein

Both men were Platonists who believed in an objective reality. That is, mathematics and science both engaged in *discovery* not *creation*. Einstein was quite clear about it: "Out yonder there was this huge world, which exists independently of us human beings and which stands before us like a great, eternal riddle, at least partially accessible to our inspection and thinking."[10] Gödel believed in the objective existence of mathematical reality (Goldstein, 2005; Yourgrau, 2005). Their Platonism made both Gödel and Einstein outsiders and close friends during their time together at the Institute for Advanced Study. "For both, mathematics was a window onto ultimate reality, not, as for many of their scientific colleagues, a mere tool for intellectual bookkeeping" (Yourgrau, 2005, p.17). In contrast Werner Heisenberg and the other leading lights of the "new physics" thought this was nonsense. "The idea of an objective real world whose smallest parts exist objectively in the same sense as stones and trees exist, independently of whether or not we observe them … is impossible."[11]

The irony of Gödel's Platonism is that his own work lends itself to a relativist interpretation. His masterwork, "On Formally Undecidable Propositions of *Principia Mathematica* and Related Systems" (1931), is perhaps the greatest intellectual accomplishment of the 20th Century. Gödel's paper proved a revolutionary theorem and a corollary that overturned Hilbert's formalism. "By Gödel's theorem the following statement is generally meant: In any formal system adequate for number theory there exists an undecidable formula – that is, a formula that is not provable and whose negation is not provable. (This statement is occasionally referred to as Gödel's first theorem.) A corollary to the theorem is that the consistency of a formal system adequate for number theory cannot be proved within the system. (Sometimes it is this corollary that is referred to as Gödel's theorem; it is also referred to as Gödel's second theorem.)" (Goldstein, 2005, p. 23). Provided a logical system incorporates the rules of Arithmetic, no such system could be *closed*. That is, he showed that there are true statements that could not be proven using the rules of the system. "Gödel destroyed the hopes of those who believed that mathematical thinking is capturable by the rigidity of axiomatic systems, and he thereby forced mathematicians, logicians, and philosophers to explore the mysterious newly found chasm irrevocably separating provability from truth" (Nagel, Newman, and Hofstadter, 1958/2008, p. xiv). "[T]he once-bright hope of mechanizing human mathematical thought starts to seem shaky, if not utterly quixotic. What, then, after Gödel, is mathematical thinking believed to be? What, after Gödel, is mathematical truth? Indeed, what is truth at all?" (Nagel, Newman, and Hofstadter, 1958/2008, p. xv).[12]

Gödel's results do not in fact undermine science – everything is *not* relative. Rather the incompleteness theorem and corollary simply put a limit to our *purely* logical organization of the world. Penrose (1990; 1994) argues that our minds cannot simply be digital computers. "I argue strongly against the commonly held viewpoint that our conscious mentality – in all of its various manifestations – could, in principle, be fully understood in terms of computational models" (1994, p. v). Goldstein (2005, p. 25-26) states Penrose's case quite clearly: "Penrose's argument, in direct opposition to the postmodern interpretation … understands Gödel's results to have left our mathematical knowledge largely intact. Gödel's theorems don't demonstrate the limits of the human mind, but rather the limits of *computational* models of the human mind (basically, models that reduce all thinking to rule-following). They don't leave us stranded in postmodern uncertainty but rather negate a particular reductive theory of the mind."

Norman Schofield (1996) takes note of the importance of Penrose's argument in his response to Green and Shapiro's (1994) criticisms of rational choice theory. Schofield argues that we need to develop better models of rationality "that incorporate both preferences and beliefs" because rational agents not only must be preference maximizers but they also must model how other agents are themselves modeling (p.206-207). "I believe Penrose's argument implies that there must be fundamental constraints on our ability to model our own behavior" (p. 207). However, this does not mean that we cannot make great theoretical and empirical progress. Only that there is somewhere out yonder a *purely* logical limit to how much of ourselves we can model.

To return to the question posed at the beginning of my talk, are we doing science? The controversies outlined above seem to militate against my making a definitive case. However, recall that scientific knowledge is defined as "a branch of knowledge or study dealing with a body of facts or truths systematically arranged and showing the operation of general laws." I believe we have produced scientific knowledge and, like a good American, I am going to Court to win my case!

William Daubert et al. v. Merrell Dow Pharmaceuticals, Inc.

In 1993 the Supreme Court had to determine the standard for admitting expert scientific testimony in a federal trial. In a 1923 decision by the Court of Appeals for the

District of Columbia – *Frye v. United States* – the appeals court established the "generally accepted" standard for expert testimony: "Just when a scientific principle or discovery crosses the line between the experimental and demonstrable stages is difficult to define. Somewhere in this twilight zone the evidential force of the principle must be recognized, and while courts will go a long way in admitting expert testimony deduced from a well-recognized scientific principle or discovery, the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field in which it belongs." The problem, at least from the point of view of trial lawyers pursing "toxic tort" claims, is that the Frye rule seemed to defer to the scientific community and ruled out novel methods that are not yet accepted.

In the 1975 Federal Rules of Evidence Rule 702 stated: "If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise." At issue in *Daubert* was whether the *Frye* standard had survived the 1975 codification of the Federal Rules of Evidence. All 9 justices agreed that "insofar as 'scientific knowledge' is concerned, Frye had been superseded by" FRE 702 – scientific knowledge replaced general acceptance. In the second half of the opinion seven justices – Blackman, White, Scalia, Thomas, Souter, O'Connor, and Kennedy – went on to try to define scientific knowledge.[13]

In the majority opinion the justices outline a very conventional view of science. Their focus is on "scientific knowledge that will assist the trier of fact" (whether the testimony's underlying reasoning or methodology is scientifically valid and properly can be applied to the facts at issue). Some criteria (not all mutually exclusive or collectively exhaustive) that they discuss: 1) Whether the theory or technique in question can be (and has been) tested (Karl Popper's principle of *falsifiability*); 2) Has the scientific theory or technique been subjected to peer review and publication; 3) What is the known or potential error rate; 4) What is the expert's qualifications and stature in the scientific community; 5) Can the technique and its results be explained with sufficient clarity and simplicity so that the court and the jury can understand its plain meaning?

Scientific decision-making and legal decision-making are very different. "Science searches for a comprehensive understanding, which develops [over time] through a collective process involving many scientists. A trial seeks to resolve a focused legal dispute in a finite amount of time" (Foster and Huber, 1999, p. 17). Scientists *weigh* evidence, they do not distinguish between "admissible and inadmissible". In a trial, a *binary final choice must be made* (the Judge admits evidence and the Jury weighs it). In law inadmissible does not equal *false*. "The scientific community is strongly inclined toward letting new ideas be presented and debated. The legal presumption is against admitting expert testimony…" (Foster and Huber, 1999, p. 18). Under *Daubert*, however, the judge is expected to undertake at least a preliminary assessment of where on the continuum between *conjecture* and *scientific knowledge* an information claim is.

So, where on the continuum between conjecture and scientific knowledge does our field "geometric models of similarities and choice" lie? Clearly, close to the scientific knowledge end of the scale. Our approach is close to that of econometrics and general statistics. The courts have admitted much expert testimony based upon econometrics and statistics. In contrast, sociology and psychology/psychiatry fare less well.[14]

In terms of the five criteria, first, our methods are clearly falsifiable. Our models are built upon straight-forward assumptions of *purposive* behavior. We assume that individuals have single-peaked utility functions over some *low-dimensional* policy space and that in a choice situation they choose the alternative closest to them. These assumptions are clearly falsifiable. For example, violations of single-peaked preferences can be detected in the pattern of the errors from the choice model (Poole and Rosenthal, 2007, chapters 3 and 7).

Second, has the scientific theory or technique been subjected to peer review and publication? Absolutely.

Third, what is the known or potential error rate? In our case this is akin to a repeat of the falsifiability criteria. It is possible that we are all poor computer programmers so we have all made the same errors but I think this is unlikely.

Fourth, what is the expert's qualifications and stature in the scientific community? Well, I will leave myself out of this. The quality of my colleagues in this area (it seems to me) is beyond question.

Fifth, Can the technique and its results be explained with sufficient clarity and simplicity so that the court and the jury can understand its plain meaning? Yes. This is why I prefer to call our models of similarity and choice as *geometric* rather than spatial. Left and Right, Liberal and Conservative, Converse's (1964) notion of *constraint* or correlation between issues, is something that can be easily explained using visual aids. Our work produces *pictures*.

Finally, have we shown the operation of general laws? Yes. The basic random utility model is consistent with Shepard's exponential law of response functions (Shepard, 1987).[15]

In sum, in my opinion, we pass the *Daubert* test.

Conclusion

In the past 30 years I believe that our field has come from almost nowhere to establishing itself as a true scientific sub-discipline. Minimal rational choice assumptions coupled with the availability of fast computers have enabled us to design models and empirically test them in practical ways. Although we do not wear white lab coats, we gather data, we develop theory, we build measuring instruments in the form of computer programs, and we uncover regularities. I believe that we have the right to be proud of what we have accomplished. As I note in my book (Poole, 2005, p. 201), "it seems to me that if science is the observation, identification, description, experimental investigation, and theoretical explanation of natural phenomena, the study of geometric models of human choice and judgment -- encompassing work in Psychology, Political Science, Economics, and other disciplines -- is a textbook case of scientific progress."

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Endnotes

[1] For examples see the references in my book *Spatial Models of Parliamentary Voting* (2005).

[2] See Ordeshook (1986, chapter 1) for a clear discussion of the foundations of rational choice.

[3] This portion of the essay draws on chapter 7 of Poole (2005).

[4] I thank Gary Cox for suggesting this formulation to me.

[5] Ontology is the science of being or reality; the branch of knowledge that investigates the nature, essential properties, and relations of being.

[6] These quotes are taken from Kuhn (1962/1996, p. 206).

[7] For a lucid account of the controversy of science being "socially constructed," see Hacking (1999, chapter 3).

[8] I gave the lecture in December, 2007. Barzun was born in November 1907.

[9] Andrew Carnegie, who befriended the Kaiser, who built the "Peace Palace" in The Hague, and indefatigable international peace activist, thought that Peace had finally arrived. By 1912 he believed that the "millennium was at hand" (Wall, 1970/1989, p. 1006). He was devastated by the War and never recovered his old optimism (see Wall, 1970/1989, chapter XXV).

[10] Quoted in Goldstein (2005, p. 43). The original is from the forward to a volume of essays in honor of Einstein's 70th birthday (Schilpp, 1949).

[11] Quoted by Goldstein (2005, p. 42, footnote 14). Also see the discussion in Yourgrau (2005, p. 17).

[12] This seems to echo the famous passage in the Gospel of John (18:38): "'Truth?' said Pilate. 'What is that?'" Jesus did not answer.

[13] The two dissenters were Rehnquist and Stevens.

[14] See the website http://www.daubertontheweb.com/ for long lists of relevant cases by knowledge area.

[15] See the extensive list of citations in my book of work done by Psychologists that is essentially equivalent to our basic geometric approach to choice.