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Hindsight 40 years on: An interview with Baruch Fischhoff

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Abstract

This article presents an interview with decision scientist Baruch Fischhoff, who pioneered research on the hindsight bias—the tendency to view an event as more predictable, inevitable or likely once it has taken place. Fischhoff traces his early research on hindsight biases, conducted in Israel in the early 1970s at the beginning of Kahneman and Tversky's influential research program in judgment and decision making. We revisit his less-known writings about social psychologists' attributions about the decision making of leaders regarding pivotal military decisions and Fischhoff's own engagement in security and risk analysis after 11 September. Fischhoff distinguishes two views of past events, a historian's sense of the past as a series of distinct events and the behavioral scientist's view of events as falling into "equivalence classes," allowing for meaningful predictions that outcomes of past events would replicate. We discuss the contribution of decision science to the problematic and contemporary controversies surrounding replicability in experimental social psychology.

Keywords

biography, decision science, hindsight bias, historiography

In this first section, Fischhoff describes coming to work with Amos Tversky and Daniel Kahneman at the Hebrew University in Jerusalem. The "heuristics and biases" approach to judgment and decision making developed first by Kahneman, Tversky, and their students at this time emphasized how limited cognitive resources lead people to seemingly irrational decisions and judgments. This

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research program was a pivotal development in the rise of cognitive approaches within experimental psychology in the late 1960s and 1970s (Heukelom, 2014) and contributed to Kahneman's receipt of a Nobel Prize in Economics in 2002. Fischhoff's dissertation work on the "hindsight bias" was an early success of this research program. The hindsight bias was first reported in Fischhoff (1975). Experimental subjects read accounts of historical events or clinical cases with one of several outcomes. Participants were then asked to estimate how likely each of the focal endings would have seemed had they not known the outcome. Across experiments, participants consistently viewed the outcome they had received as having been more likely than did those who had not been informed of an outcome. Since 1975, thousands of studies have been conducted on this phenomenon.

Biography and early interests

I: This Special Issue will examine such aspects of historical cognition as explanation, flashbulb memories, conspiracy theorizing, and attributional biases. As hindsight bias is central to historical cognition, we thought an interview with you would be appropriate. Perhaps you could begin by telling us how you came to study this phenomenon?

R: I grew up in Detroit and joined a youth movement called Hashomer Hatzair. Founded in Poland in 1913, it encouraged members to live on a kibbutz in Israel. In the mid-1960s, this form of idealism appealed. I had been involved in the civil rights movement in Detroit and it [the youth movement] seemed like it had all the answers—which it sort of did, in theory.

I did an undergraduate degree in math at Wayne State, working my way through college at the counter of a bowling alley a couple of nights a week. Wayne State was (and still is) a large urban school, with 35,000 students, and it has always been a pathway for working-class people. My mother went there, my father, my sister, my brother, my aunt, and my uncle. It had some really good people in the psychology department and you could get a fine education in math. Wayne required 15 minutes of career counseling. The woman who checked my record said "You're close to the 68 hours you need in math. If you take one more psychology course, you would have the 30 hours you need for a double major." I said "What do you do with that?" She looked it up in a book, which had one suggestion: psycholinguistics. Neither of us knew what that was.

I: That makes sense because it was early days of cognitive psychology.

R: Francine Wehmer, the professor in one of my psychology courses, knew that I was a math major and suggested that I meet Samuel Komorita, one of the first people to do what is now called behavioral game theory. Only when reading his obituary did I learn that he was a Japanese American who had been interned during World War II. I became his research assistant for a project on behavioral prisoners' dilemmas, during the course of which he gave me the great Anatol Rapoport's book, *Fights, Games and Debates*. I was accepted by the math psych program at Michigan, but decided that I would rather live on a kibbutz. Still, I wanted to meet Rapoport. Michigan agreed to set up a meeting with him if I would meet their other faculty. They all said that should I ever change my mind, I should look up Amos Tversky. A few years later, my wife, whom I met at a movement summer camp, had the good sense to say that we should move on from the kibbutz. I had this piece of paper with Amos' name, went to Jerusalem to meet him, and had the remarkable good fortune for him to take me on as a student.

I: Tversky was in Israel at this point in time?

R: Yes.

I: Tversky was your PhD advisor?

R: Yes. One graduate course that strongly influenced me, and indeed helped convince me that psychology could be a worthwhile pursuit, was Danny Kahneman's seminar on applications of psychology. Each week, Danny offered a practical problem and challenged us to anything we knew about psychology to solve it. One week, Amos gave a talk on decision making. Their ensuing discussion brought out many of the issues that their joint research program developed. The public part of the work included an amazing seminar, which drew people from both inside and outside the psychology department. Jerusalem, at that time, had a small town feel, with a core of people who had grown up together and knew and trusted one another across disciplines, creating a unique, wonderful cultural and intellectual atmosphere. And these two great intellects, willing to argue in public. Amos went to the US for a year and I had the continuing good fortune of having Danny Kahneman take me on to finish my Masters. I then went back to Amos to finish my dissertation.

I: Was the Kahneman and Tversky network very different from other forms of psychology that were going on at other universities?

R: It was a special place with a sense of camaraderie that was especially valuable to Andi and me, as we had little family in Israel. Among the graduate students who became a sort of extended family were Maya Bar-Hillel, who is still at the Hebrew University; Zur Shapira now at the NYU Business School; and Ruth Beyth, now head of psychology at Israel's Open University. The research program that we joined had its roots in the axiomatic decision theory laid out by von Neumann and Morgenstern, Savage, Ramsey, and others.[1] At Michigan, Howard Raiffa and Ward Edwards sought to apply the theory in behaviorally realistic ways. Within the math psych program, Clyde Coombs brought a psychometric and choice theory perspective. They were fortunate in having Tversky as a student. Tversky as an undergraduate did psychology and analytical philosophy where I believe that his mentor was Yehoshua Bar-Hillel, Maya's father and one of the logical positivists. Kahneman as an undergraduate did math and psychology.

At this stage, it was, I think, all that they could do to integrate the formalisms of decision theory with cognitive and experimental psychology. Once that work matured, it could begin to accommodate emotions and social psychology. For his PhD, Danny studied personality with J. P. Guilford at Berkeley,[2] after which he retrained himself in perception, including some time at the late, lamented Medical Research Council Applied Psychology Unit at Cambridge, where I also got to spend a year. Thus, he brought those aspects of psychology to the research, which reflected the analytical perspective that he and Amos shared.

Intellectual influences

Like the historian David Fischer, Fischhoff remained committed to interdisciplinary exchange between historians and psychologists regarding theoretical explanation. In this section, he describes the historians' early engagement with Marxism, and critiques psychohistory which searches for confirming evidence of general theories—such as psychoanalysis—in particular historical cases.

I: In your 2007 paper, you cite the paper by Paul Meehl, "Why I do not attend case conferences."

That paper, which describes the greater availability of explanations in hindsight, was important to my thinking because it provided a link between my abiding interest in politics and the world of research. As part of my youth movement and subsequent political activity, I had read a lot of historiography and wondered how well historians' thinking about their craft would match psychological research into thinking about the past. Historians are fully immersed in their topics, but we can create new data and vary experimental conditions.

I: That's right, it's a big difference.

R: I would occasionally sit in on meetings of grad students in medieval history, who knew most of what was ever likely to be known about their periods, hence needed to reflect on its interpretation.

I: That's it. We talk about this often in our group with social psychologists and historians as you can imagine. Do you remember what you read at this time?

R: Yes. I have a chapter in the first Kahneman, Slovic, and Tversky book called *For Those Condemned to Study the Past*. That has references to many of the things I read—Gallie (1964), Carr (1961), and Hexter (1971). And Imre Lakatos' work, which I discovered early on and which has its own version of hindsight bias (the illusion of crucial experiments). I had the benefit of not knowing what anyone in these fields thought about them.

I: You were interested in politics and that led you to history?

R: Yes. Our youth movement thought of itself as Marxist, although that was very dilute (almost homeopathic). Our real commitment was to one another and an egalitarian lifestyle. Nonetheless, it did get me to read more Marxism that I would have otherwise, including a Soviet monthly on sociology, which somehow made its way to our apartment in Jerusalem. A typical 10-page article would have 9 pages of obeisance to Marx and Lenin, with an idea tucked in at the end. That period taught me something about the dangers of ideology, including movements in psychology.

At that time, I also learned psychohistory from a class taught by Saul Friedländer, a French Jew who survived the Holocaust hidden by nuns. He argued that the American New Left shared features with the German Wandervogel, which devolved into the Hitlerjugend. Psychoanalytically oriented psychohistory typically must speculate about the early years of its subjects' lives. I stumbled across a book on child-rearing practices in Detroit during my youth, and wrote a term paper on what those parents' reports said about the psychology of my political buddies—concluding, for what it's worth, that they were not proto-fascists.

I: We have been reading the historian David Fischer (1971) from this period, who wrote *Historian Fallacies*. He relies on psychology a lot, sees history as problem solving, and cites a number of psychologists of the day, including David McClelland. We see some overlaps between Fischer's book and your hindsight bias. Were you aware of that work?

R: Yes, absolutely. My goal was to do what Fischer had done, from a psychologist's perspective. I subscribed for many years to *History and Theory*, hoping to complete the project. I am proud

to have had a note (Fischhoff, 1978) published in that journal, edited by the likes of Raymond Aron. However, my opus never got written. From Jerusalem, I moved to Eugene, Oregon, to work with Paul Slovic and Sarah Lichtenstein. We were on soft money, which pulled us into applied areas, in order to pay the bills, and also exposed us to many untouched political assumptions (Fischhoff, 2015). About 1992, I stopped my subscription to *History and Theory*. My scholarship just wasn't going in that direction, although I still have old issues in a box.

I: You wanted to do for psychology what Fischer did for history?

R: Historians have a distinctive form of expertise in working with historical data. They know something that we don't know, just like anthropologists and sociologists do. I thought that if Fischer had tried to shine a psychological light on his own profession, then perhaps I could shine a historical light on our profession.

Looking back on hindsight

Fischhoff's early work was completed in the aftermath of the Six-Day War in June 1967, in which the Israeli army routed the invading Egyptian, Syrian, and Jordanian armies. Fischhoff was finishing his PhD during the subsequent Yom Kippur War (October 1973) in which an initially successful Egyptian–Syrian surprise attack to recover the territories lost in 1967 was eventually repelled by Israeli forces. The failure of Israeli intelligence to anticipate the attack created a major political crisis, including the resignation of the Israeli Prime Minister Golda Meir. The Agranat Commission, headed by the President of the Israeli Supreme Court Shimon Agranat, provided the formal investigation.

During this same period, some social psychologists became interested in the psychological underpinnings of historic political decisions. *Victims of Groupthink* by Irving Janis (1972) analyzes several major foreign policy decisions, including the Kennedy Administration's decision to send troops to the Bay of Pigs in Cuba in 1961 and the US naval high command's decision not to make more serious provisions for a Japanese assault on Pearl Harbor. Janis attributed these failures to "groupthink," "a deterioration of mental efficiency, reality testing and moral judgment as a result of group pressures" to achieve consensus. To avoid such failures, Janis recommended that decision-making groups include a "devil's advocate" to expose group members to counterarguments. Reviewing Janis' book, Fischhoff and Beyth-Marom (1976) argued that poor political decisions could be explained by the cognitive limitations of individuals faced with massive amounts of uncertain information, making group level explanations unnecessary. They also suggested the possibility of hindsight bias in such accounts. Since that time, Fischhoff work has shifted to what is now called decision science, with a focus on basic research relevant to policy analysis. The review of Janis' book reflects this transition.

I: We are thinking about which kinds of specific historical events prompt psychologists to investigate historical cognition. You've mentioned your involvement in youth movements, the kibbutz movement, and the intellectual set in Jerusalem around Kahneman and Tversky. Were there other world events that were material to your political impulse to research the cognition of history?

R: As you know, there are always interesting times in Israel. I was there from 1967 through 1974. I got there right after the 1967 war, but in time for some terror and the 1973 war, with one of the classic strategic surprises playing out while I was writing about hindsight. I actually drafted much of my dissertation during the war.

I: The data were already collected by then?

R: Yes. I had collected the data; that was my year for writing it up. During the war, the university was closed. I had an Olivetti, a gift of my Aunt Blanche and Uncle Mainey, and some aging carbon paper. Every evening there was a blackout. So once a day or so, I would take a carbon copy of my dissertation to the university, about a 2-mile walk away. I figured that if we survived the war, why not get the degree? The recriminations and commissions after the war sounded like the claims in Janis' book—and like hindsight bias. Had we not survived the Cuban missile crisis, another of Janis' case studies, we would not be writing history. Because we were nobodies, Ruth and I could not get an audience for our views. However, we could write a book review, which we got it into an educational journal in Hebrew and which I subsequently translated into English. It was published in *Policy Sciences*. The only time I ever met Janis, who was, I think, a great man, that critique was all he wanted to talk about.

I: It's fascinating to read this review now. The lesson we draw from it is that the only lesson you can learn from history is uncertainty and indeterminacy. You cite, for example, the Wohlsetter (1962) book about Pearl Harbor. When reading work on the hindsight bias from that time, there seems to be some kind of normative perspective emerging. I wonder whether, after all these years, it would be fair to say that the only lesson we can draw from history is that we cannot predict the future?

R: I wrote a short passage in the original *Hindsight ≠ Foresight* paper that tries to distinguish hindsight bias from legitimate learning. It uses an urn-like example, by analogy with the experiments then being done within the Bayesian paradigm.[3] It offered a way to think formally about the structure of the problem, which is how any decision science inquiry should start. I proposed thinking about equivalence classes of events (e.g. struggles between colonial and local forces), allowing one to say something, properly qualified, about the relative frequency of outcomes and their predictability. I still think that there is a wealth of practically important, intellectually challenging topics to be addressed, bringing together history and psychology, with decision science perhaps creating some of the bridge between them.

I: That's a very useful opinion to hear.

R: Can I just mention one other thing which might be useful here? As a result of being involved in politics, here regarding the Vietnam War, then in Israel and back here, I've long been interested in national security issues. One of the conclusions of the Agranat Commission, after the 1973 war, was that Israel should have an alternative analytical authority, like Janis' devil's advocates. Zvi Lanir was asked to set it up and recruited Danny to help him (see Lanir and Kahneman, 2006). Somehow Zvi ended up in New York and Danny recommended that he talk to me, leading to an intermittent conversation for many years.

After 9/11, I was appointed to the Department of Homeland Security Science and Technology Advisory Committee. I was also on the Environmental Protection Agency's Scientific Advisory Board and was asked to chair and, as they say in the jargon here, help to *stand up* its Homeland Security Advisory Committee. In 2009, I chaired a committee of our National Academy of Sciences sponsored by the Director of National Intelligence, through its Office of Analytical Integrity and Standards. We produced a report on how intelligence analysis could make use of social, behavioral, and decision science (National Research Council, 2011).

We had a terrific committee, including Phil Tetlock, Reid Hastie, Barbara Spellman, Gary McClelland, Cathy Tinsley, and Hal Arkes, among the psychologists. In addition to our short report, we produced a book of readings, with a chapter by each member, written for intelligence community people who wanted to know more (Fischhoff and Chauvin, 2011). The short report has an epistemology for any organization trying to think about complicated uncertain systems.

The report is short because most policy people have little time to read. One of my briefings was with Admiral Michael Rogers, now head of the National Security Agency who had printed it out and was ready to talk about it. Based on that conversation and a couple of subsequent ones, it seems like our proposal was consistent with his own epistemology. Our premise was that there are basic analytical methods that everyone should know. For example, everyone should know that game theory is a way to think about interpersonal situations, just as operations research exists for queuing and logistical phenomena.[4] There's a kind of individual wisdom that comes with knowing that these perspectives exist, and group wisdom from being able to have discussions in a team where everyone knows about them. That awareness, or fluency, alone should improve understanding, while creating the option of getting "somebody to run the numbers," for really complicated problems.

My strategy in setting up the committee was to exclude vendors and focus on enhancing the human capital of the intelligence analysts, by helping them to do their jobs better. Recognizing the need for organizational support for such policies, we had organizational behavior and human resource people on the committee as well.

The rationality of the decision sciences

In this section, Fischhoff addresses two epistemological problems facing social psychology. One is the widely disseminated interpretation of Tversky and Kahneman's work on judgment heuristics that it shows that people are "biased" in their judgments and decisions. In this view, humans consistently fail to adhere to "correct" or "rational" judgmental standards because of a lack of capacity or motivation. This view has been the focus of heated debate with critics who argue that—more often than not—the use of heuristics leads to both rational and functional judgments and decisions.

The second concerns the debate surrounding the reproducibility of psychological findings, which erupted after the failure to replicate a well-known study reporting that unobstrusively activating (i.e. "priming") a stereotype led people to behave in line with that stereotype (see Doyen et al., 2012). Seeing that many well-known social psychological studies had never been replicated, critics argued that researchers had favored "sexy" and media friendly, but unreliable, findings at the expense of solid incremental research. Since then, attempts at replicating priming effects and other experimental findings have weakened confidence in the field (Open Science Collaboration, 2015). The focus on "reproducible" effects may contrast with a long-held (but minority) view of social psychological processes as historically situated and contingent (Gergen, 1973). In this view, an experiment could still be valid, even if changing historical circumstances meant that its result could no longer be replicated.

I: Thinking about the policy impact of your work in Israel and in the United States also raises the point that psychologists' work can affect history. What do you think the major effect of the heuristics and bias approach on history has been?

R: If you look at Kahneman and Tversky's work, and the contemporaneous research of Robyn Dawes, Paul Slovic, Sarah Lichtenstein, and others, you see high citation counts. However, I think subsequent work has drifted off its original analytical moorings. Dina Berkeley and Patrick Humphreys (1982) had an interesting paper around 1980 called the "bias heuristic."

They observed that there was a script whereby one could see a behavior that didn't look quite right and decide that it was a *bias*, by contrasting it with a presumed normative standard. Without discipline in choosing that standard, the research loses its analytical foundations and gravitates to an unsympathetic, often disrespectful, and undemocratic view of people—ready for manipulation, for their own good.

I: So would it be fair to say that Berkeley and Humphreys' argument about the extent to which human thinking is labeled "bias" has become more relevant since the original work in the 1970s.

R: The prevalence of "bias" depends on your unit of observation. What decisions have we made in this conversation? How many biases have there been? How many have been noticed and corrected? Just to say "people are biased" is a meaningless statement. But people make those assertions all the time, without doing the needed analytical work. When I teach "The Law of Small Numbers," Tversky and Kahneman's first paper, I direct students to the footnotes and the care taken in establishing normative standards.

It drives me nuts to see the celebrity psychology of people striving to get overheated claims into the *Sunday New York Times* review section, despite knowing that the studies that they cite have not been replicated, or perhaps even survived peer review. I don't think this is good for science. I don't think it's good for society.

I: To come back to the influence of the heuristics and biases research on political events, are there examples of policies that have been important and that have been informed by this approach in decision science?

R: Yes, I think so. There are some agencies. I've dealt mostly with health, safety, and environment. The Food and Drug Administration (FDA) has been doing a lot (Fischhoff, 2017). Our new Consumer Financial Protection Bureau has a strong behavioral component. It's easier to do it in new agencies than in older agencies, with budgets committed and practices in place. It's also easier to introduce specific programs (e.g. behavioral "insights") than systemic changes, as we have tried to do with the intelligence community and FDA. I think that bureaucracies are also more receptive to manipulative behavioral science than to empowering behavioral science.

I: By manipulative, do you mean nudging approaches and things like that?[5]

R: Yes. Advocates for such programs have identified the conditions for disciplined, ethical interventions. The challenge is to ensure that they are achieved. Given the ways in which most agencies work, that requires a lot of time with them, attending to the details, time that could be spent writing papers—and doing the other things that academic institutions reward.

I: That's a great point. It returns us to the hindsight bias and the difference between knowledge that's used to honestly answer a question prospectively versus knowledge that's used retrospectively to justify an existing position.

R: After 9/11, there was what I thought of as a lot of *hindsight bias-bias*, with the Bush Administration and its defenders saying that "nobody could have known," given the

intelligence available. If that is, in fact, the case, then it implies that the intelligence services were managed so ineffectively that they failed to gather and convey the information that they could reasonably have had at hand. If US leadership could not have known at that time, that was a tactical failure reflecting a strategic failure to collect and communicate the proper information.

I: That almost takes us back to the level of analysis that Janis was interested in. He was interested not just in making sense of information as an individual account for an event, but also in the organization behind the individual cognition, which puts it in context.

R: Agreed. I think he was right regarding the organizational processes and a little shakier on the individual cognition.

I: You have been very open (Fischhoff, 2007) about the fact that in the original Hindsight ≠ Foresight paper, the study materials included a vignette about a homosexual man who received therapy to "cure" his homosexuality. If we look back in hindsight now at this example, it looks very different now. Indeed, if you were to even do this experiment now, it wouldn't even be a replication.

R: Yes. It would be a good example of Kenneth Gergen's (1973) account—and a happy one, given the change in our collective understanding.

I: That example raises the question again of whether psychology experiments are relatively unique events or whether they fall into equivalence classes that can be expected to be replicated in different time and places or are unique events.

R: Another place where you might look is the reproducibility crisis. There are those who would argue that if you fail to replicate their result, it is because you didn't perfectly replicate what they did. Let us assume this is a heartfelt claim and that Danny Kahneman's daisy chain proposal provides a way to address the concern (at the price of spending resources on replication that could be spent on new studies).[6] Nonetheless, it seems like a strategy for winning the battles but losing the war. It implies that the effects in question are such fragile phenomena that they're like orchids that grow in very narrow bands (e.g. between 6500 and 7000 feet on mountains with volcanic soil fed by prevailing westerly winds). If so, then the effects are real, and perhaps beautiful, and their existence will tell you something about pollinators. However, they may not support strong statements about the nature of the human condition and how people should lead their lives.

I: At the end of your review of the Janis book, you drew different lessons from the ones Janis does. You wrote the following:

> Janis suggests that presentation of multiple scenarios as a stimulant to the imagination of the members of policy-making groups […]. As a model of the presentation of multiple scenarios, he offers the classic Japanese play "Rashomon." The point of Rashomon, however, is that the facts of any given historical episode are so ambiguous that they can be accounted for by a variety of contrasting explanations. Although the data in the cases studied by Janis can be interpret to fit a groupthink explanation, other observers may reasonably find in them evidence of incompetence, conspiracy, or the hopelessness of standing in the way of world revolution. (pp. 392–393)

This comment speaks to something that's probably very important in the history of philosophy or historiography: Is there a correct narrative? Or is there a multiplicity of stories that are equally valid? What's your take on this now?

R: I stand by our words. Let me give you a puzzle that may come out in your own research on history and psychology. There are various distinct, recognizable narrative forms, each capable of organizing some recurrent elements of human behavior. History is typically told from one of those narrative perspectives, which affords a certain kind of coherence. That could be people's history, or diplomatic history or great person history. However, the integration is left to the reader. It's as though we don't know how to tell complex, integrative stories. If you're interested in an area, then you read these deliberately limited histories and try to make sense of them all. I think this aspect of history writing says something about how people deal with any kind of complicated situation. Like these overheated claims about psychological studies in the *Sunday New York Times* that get my goat—and my wife's even more. (She tends to say that they're not worth knowing, even if they are true!) You learn that there's something called "priming," which occasionally makes a difference. It would be welcome news if the readers of these claims take them nowhere near as seriously as the authors take them themselves, and realize that each result is one more thing worth knowing about, which might come in handy one day, as part of dealing with some complex situation. If you were thinking about the French revolution, it would be good to know the diplomatic history *and* the people's history *and* the great person history *and* the various Annales, and …

I: These sort of single factor explanations that you sometimes see in psychology sound universal but often that's just because the limitations are not clearly stated. It sounds like you're saying two very interesting things here about the relationship between psychology and history. One is a sense that a priming experiment is like an orchid, and if we remember more about the historical context in which experiments are done, then we might understand their results as more orchid-like. The second thing is that sense that we might have upon hearing those explanations we have that sense of "oh yes I've learnt something." But that sense that we have gained an understanding is perhaps illusory.

R: Yes, yes that's nicely put yes.

I: Let's move to our final question … All of us still working in history share some kind of faith—in spite of everything—that understanding the past may help us to prepare for the future. History teaches us that sadly this doesn't always happen. After all of your work in these many domains, I'm wondering what you would say to a historian or a history teacher who nurtures the hope that we can have a better future via a better understanding of the past.

R: I think that's true. And I think that there is something to be learned from the path to understanding that a historical perspective encourages—one that is heterodoxically informed, recognizing that people have different truths with different limits—and that we can learn something from any conscientious effort. We should be grateful when we find ourselves in a community, like those we had in the youth movement or in Jerusalem, or in my engineering department, where people are working together to solve complicated problems.

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Notes

1. Oskar Morgenstern (1902–1977) and John von Neumann (1903–1957), the originators of game theory, were deeply influenced by philosopher-mathematician Frank P. Ramsey (1903–1930). Mathematician Leonard. J. Savage's (1917–1971) work on probability also deeply influenced game theory and decision science in general.
2. Psychometrician Joy Paul Guilford (1897–1987) was known especially for his theory of human intelligence.
3. Bayesian inference is a method of statistical inference used to update belief in a hypothesis in light of additional evidence, following on a theorem by the mathematician Thomas Bayes (1702–1761). The first experiments evaluating lay adherence to Bayesian principles used balls of different color drawn from urns.
4. Game Theory refers to mathematical models of conflict and cooperation between rational agents. It is widely used in economics, political science, and biology. For a short introduction and application to the 2016 US Republican primaries, see Quealey (2016).
5. Nudging involves modifying the decision environment to influence behaviors (cf. Thaler and Sunstein, 2008). A paradigmatic example involves placing an image of an insect into a men's rooms urinal to improve men's aim.
6. See the letter sent by Daniel Kahneman in 2012: http://www.nature.com/polopoly_fs/7.6716.1349271308!/suppinfoFile/Kahneman%20Letter.pdf

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