12

Richard Thaler & Co.: Building the New Behavioral Boomtowns

We can compare K-T's three research projects that we just reviewed to Meriwether Lewis and William Clark's epic eight-thousand-mile-long trek, hailed as the first big step in the westward expansion of the United States. Lewis and Clark produced maps of uncharted land, rivers, and mountains. But westward expansion also required others to settle in the West. Similarly, K-T's pioneering research created a boom, as we will see in this chapter, because

- Richard Thaler and other younger economists, supported by the Russell Sage Foundation, formed a strong, cohesive community that built on the base K-T had established.
- The community expanded the scope of behavioral research to finance while maintaining its cohesion and links to K-T's foundational ideas.
- Criticisms and alternatives could not disturb the dominance of the community or deflect behavioral economics from K-T's uncertainty-free path.

1. Strong, Cohesive Community

Capable Followers While, as mentioned, the technical virtuosity of K-T's *Econometrica* article earned them the respect and attention of economists, they remained primarily psychologists. And psychologists could not by themselves create a community whose work mainstream economists would accept. Acceptance required economic researchers capable of publishing in top economics journals. As often happens with new movements, young economists played a significant role in creating this new community, although some established stars like Akerlof were also supportive. Richard Thaler played a leading part through his research and through other younger researchers he helped attract.

A "Sharp and Irreverent" Ringleader

In a warm and generous tribute to Thaler, Kahneman recalls that "sometime in 1976, a copy of the 1975 draft of prospect theory got into Dick's hands, and that event made a significant difference to our lives." The ideas in the draft had resonated with Thaler, then an assistant professor at the University of Rochester's business school. When Thaler learned that K-T would be at Stanford in 1977–1978, he secured an appointment at NBER's Stanford office. That started what Kahneman calls "the second most important professional friendship in [his] life." Thaler, a "young economist, blessed with a sharp and irreverent mind," Kahneman writes, had already "trained his ironic eye on his own discipline and had collected a set of pithy anecdotes demonstrating obvious failures of basic tenets of economic theory in the behavior of people in general—and of his very conservative professors in Rochester in particular." Thaler and Kahneman became friends at Stanford and "ever since had a considerable influence on each other's thinking."[1]

Kahneman also believes that his interaction with Thaler

> was a major factor in my receiving the Nobel Prize. The committee cited me "for having integrated insights from psychological research into economic science. . . ." Although I do not wish to renounce any credit for my contribution, I should say that in my view the work of integration was actually done mostly by Thaler and the group of young economists that quickly began to form around him starting with Colin Camerer and George Loewenstein, and followed by the likes of Matthew Rabin, David Laibson, Terry Odean, and Sendhil Mullainathan. Amos and I provided quite a few of the initial ideas that were eventually integrated into the thinking of some economists, and prospect theory undoubtedly afforded some legitimacy to the enterprise of drawing on psychology as a source of realistic assumptions about economic agents. But the founding text of behavioral economics was the first article in which Thaler (1980) presented a series of vignettes that challenged fundamental tenets of consumer theory. And the respectability that behavioral economics now enjoys within the discipline was secured, I believe, by some important discoveries Dick made in what is now called behavioral finance, and by the "Anomalies" columns that he published in every issue of the *Journal of Economic Perspectives* from 1987 to 1990.[2]

The Anomalies column, according to Camerer, Lowenstein, and Rabin's preface to *Advances in Behavioral Economics*, was "critical" in drawing attention to behavioral economics. It helped to shift many economists from the attitude of "if it works, don't try to fix it" to "it's broken; how can we fix it?"[3]

Thaler was also emphatic about the widespread prevalence of anomalies and errors. Where K-T had claimed that heuristics "sometimes" produce misjudgments, Thaler asserted that "mental illusions should be considered the rule rather than the exception."[4]

Camerer, Lowenstein, and Rabin (like Kahneman) also acknowledge the "unusual and vital role" of Eric Wanner. Wanner did not do any behavioral research., but as a program officer at the Sloan Foundation and then as president of the Russell Sage Foundation, Wanner worked closely with Kahneman, Tversky, and Thaler to support and influence the behavioral economics boom.

An "Unusual and Vital Role"

Wanner started as an assistant professor in Harvard's psychology department after his PhD in 1969 but left academia in 1976 to join Harvard University Press as an editor. He started the Cognitive Science Series at the press, whose advisory board included K-T. In 1982, Wanner moved to the Sloan Foundation as a program officer. There, Wanner proposed applying cognitive science to economics. In 1983, the Sloan Foundation made the first grant for its new "behavioral economics" program. The grant funded Thaler's sabbatical to work with Kahneman at the University of British Columbia.[5]

In 1986, Wanner was appointed president of the Russell Sage Foundation, which had a smaller endowment than the Sloan Foundation. At Sage, according to Camerer, Lowenstein, and Rabin, Wanner made a "big bet," seeing in "behavioral economics the chance for a small foundation to have a big impact in social science and to broaden the language of economics to say more about poverty. He funded research in behavioral economics and invited many behavioral economists to the foundation as fellows in residence." Another "brilliant" Sage Foundation investment was in "biannual 'summer camps,' to teach behavioral economics to advanced graduate students in economics and other social sciences." The camps were "hugely effective in conveying a body of knowledge that campers could not get in Ph.D. courses at their home schools" and "created a social network of students from around the world."[6]

No educational and network-building investment like the one that Wanner coordinated had been undertaken around the old behavioral economics. Thus, while the new behavioral economics had intrinsic advantages over the old, its boom was also strategically engineered.

2. Broadening the Scope

Behavioral Finance As Kahneman's 2002 Nobel announcement noted, behavioral finance was a prominent part of the "recent" behavioral boom. Finance had not interested Simon and the other "old" behavioral economics researchers. Finance also did not feature prominently in K-T's work. Then Thaler, working "predominantly in financial economics in the 1980s," "systematically connected Kahneman and Tversky's biases" to anomalies in financial markets.[7] Thaler's first "Anomalies" column in the *Journal of Economic Perspectives* mentioned earlier was on the so-called January effect in stock markets. In 1993, he edited *Advances in Behavioral Finance* for the Russell Sage Foundation, a collection of papers from the second half of the 1980s.[8] Younger stars, such as Harvard's Andrei Shleifer, who won the John Bates Clark medal in 1999, also researched behavioral finance in the 1990s.[9]

Finance was a strategic choice. In 1985, Kahneman, Thaler, and Wanner (then still at the Sloan Foundation) decided to promote research that would focus, in Wanner's words, on the "contribution of psychology and other behavioral sciences to the study of financial markets," because "financial markets are often considered the most efficient of markets and thus might be thought to be the most immune to non-rational factors."[10]

After Wanner moved to Sage, the foundation funded behavioral finance workshops at the National Bureau of Economic Research (NBER). Thaler (then at Cornell and later at Chicago)[11] and Shiller (then, as now, at Yale) organized the first such workshop in July 1991.[12] Initially held in small meeting rooms at the NBER's office near Harvard, the workshops attracted a few highly engaged participants. Some, like Shleifer, were acclaimed as rising stars in mainstream finance and economics. Top-tier journals would often later publish the papers they presented in the workshops. Eventually, behavioral finance became mainstream, and the workshops moved to large, packed conference halls. (A wide-eyed outsider, I regularly attended the workshops through the 1990s before they became popular.[13])

Finance was methodologically attractive. K-T and other researchers studying judgment and framing undertook experiments with a few hundred subjects. The incentives and subjective interpretations of the subjects and questions researchers openly designed to generate a desired result were controversial. Behavioral finance researchers, in contrast, could analyze databases of "objective" stock prices with hundreds of thousands of records to produce statistically significant results.

Behavioral finance also secured academic respectability from the rationalist theories that had emerged in earlier decades. Hitherto, financiers and many academics (including, as mentioned, Keynes) believed that fear and greed made

markets prone to irrational fluctuations, but researchers could not objectively establish this. And just as Savage et al.'s utility maximization provided a benchmark for K-T's critiques of rationalist decision theory, the rationalist financial theories defined a target for behavioral finance.

Creating a Target for Behavioral Finance

Rationality in finance is often associated with the University of Chicago, particularly the 1970 "efficient markets" paper published by Eugene Fama (who incongruously shared an Economics Nobel with Shiller in 2013). But significant contributions—many Nobel Prize–winning—have been made by researchers at other universities. These include Simon's colleagues at Carnegie, Franco Modigliani and Merton Miller, Paul Samuelson (MIT), William Sharpe (UCLA), John Lintner (Harvard), Jan Mossin (Norwegian School of Economics and Business Administration), and Steve Ross (Yale). Their research provided a benchmark for the rational pricing of financial assets, against which behavioral finance researchers could show "anomalous" risk-adjusted returns earned by investing in some "behavioral" effect.

Indeed, many of the arguments I heard at NBER's behavioral finance workshops in the 1990s were about whether behavioral strategies actually produced any "excess" returns. Were the higher returns merely fair compensation for taking more risk? Such arguments are not resolvable because what should be considered risk is debatable. Nevertheless, to argue for excess profit, finance behavioralists needed a benchmark to risk-adjust returns and databases of stock prices for statistical analyses. As it happened, believers in market rationality at Chicago and elsewhere had already created models for risk-adjusting returns and set up large databases (which the rationalists expected would help establish rationality).

The underlying psychological theory remained thin. Some financial behavioralists rhetorically invoked K-T's heuristics or prospect theory. Other researchers loosely suggested that feelings and emotions caused financial markets to stray from rational benchmarks. On occasion, Thaler did not even try to explain the anomalies he reported in his column. But financial economists, rationalist or behavioral, cared more about econometric validity than the underlying psychological theory.

Methodological commitments to econometrics encouraged a similar disregard for uncertainty on both sides. Rationalist benchmarks assumed that a stable statistical process, like the repeated spins of a roulette wheel, generated market prices. Likewise, the behaviorists too had to assume a stable statistical

process, like the spins of a systematically biased wheel, which generated predictable "anomalies" such as the "January effect." If prices fluctuated without rhyme or reason, on the unpredictable whim of "Mr. Market" (to borrow from Warren Buffett's metaphor), statistical demonstrations of predictable anomalies would be impossible. Scientific behavioral research would not have added much to the traditional lore about fear and greed. Moreover, unpredictable fluctuations would be indistinguishable from Fama's efficient market outcomes.[14]

Practical Benefits Behavioral finance research had attractions outside academia. Financial journalists could dress up commonsensical advice (e.g., keep calm when markets are in turmoil) in scientific terms. Behavioral finance also helped active investment managers justify their fees. The efficient markets hypothesis had put them on the defensive against low-cost "indexed" funds. A theory that claimed markets were inefficient, with stock prices deviating systematically according to behavioral principles, provided an argument for active management. And just as Michael Porter had started the Monitor Company on the back of his Five Forces framework (chapter 7), some behavioral researchers attempted to exploit the commercial possibilities of predictable financial anomalies. Thaler, for example, cofounded Fuller & Thaler Asset Management in 1993 to "capitalize on cognitive biases such as the endowment effect, loss aversion and status quo bias."[15]

Other Extensions While strategically important, finance was just one of several settlements for the new behavioral economists. For example, behavioralists studied intertemporal choices and strategic bargaining—both of which mainstream economists had long analyzed—and fairness and altruism, which had not been prominent mainstream topics. Behavioral economists made mainstream theories more psychologically realistic and analyzed previously neglected issues in realistic ways. For example, behavioral researchers studied why people "live for the day," excessively discounting future outcomes (and thus don't save enough), or why they don't do what they had planned to do.

The applications of the new behavioral research fit the existing economic specializations, particularly labor economics (e.g., wage setting) and macroeconomics (e.g., saving rates). The theories also prompted innovative policies designed to help people overcome their weakmindedness or short-termism. Thaler and his law school collaborator, Cass Sunstein, provided attractive packaging, calling the interventions "nudges" and "libertarian paternalism." In 2010, the UK government established a Behavioural Insights Team (BIT), unofficially known as the "Nudge Unit." By 2018, according to the OECD, more than two

hundred institutions worldwide were "applying behavioural insights to public policy."[16]

While the new settlers encamped broadly, they continued the basic approaches that had helped K-T and Thaler gain acceptance in mainstream economics. The psychology invoked in theories to explain behavior now more frequently included feelings and emotion, but psychology per se often remained perfunctory and sometimes even absent. Many well- known papers relied on mathematical wizardry to derive their results—and excluded uncertainty. For example, they assumed that people know their future wants (e.g., financially secure retirement) but lack the willpower (e.g., saving instead of consuming) to secure these wants. The "be careful what you wish for because you might get it" problem, arising from uncertain future wants, was not high on the theoretical or policy nudges agenda.

3. Alternatives and Dissents

Smith's Experimental Economics Behavioral and experimental economics are often confused. Understandably so: both try to provide realistic accounts of behavior, often emphasizing intuitive or subconscious judgments and choices. Additionally, both rely mainly on studying the behavior of experimental subjects.[17] These similarities may explain why the Nobel committee awarded the 2002 Economics Prize to Vernon Smith for his pioneering contributions to experimental economics—the same year it honored Kahneman's work in behavioral economics.

But there are also significant differences between Smith's and Kahneman's work and in the specialties they pioneered.

Smith's experiments observe trading behavior as a function of (a) an "environment" that motivates subjects' trading through monetary rewards and (b) an "institution" defined as the "messages and rules of the market," which are "often computer controlled."[18] Uncertainty, often arising from incomplete information, is built into the experimental design. In contrast, K- T's survey experiments assumed that environments and institutions did not shape behavior. And, as mentioned, K- T's survey designs excluded even mundane "missing information" uncertainty.

Smith's research on experimental markets, which he has continued for seven decades, has produced a formidable body of work. Although much of this work is outside my scope, two striking results do relate to my purposes.

First, repeated trading in experimental markets for consumption goods (bought for one-time use) produced prices that conform to the predictions of

mainstream economics. Crucially, researchers deliberately gave subjects incomplete information, and subjects undoubtedly had limited cognitive capacities and possibly biased intuitions. Yet, to use Simon's terms, the procedural rationality of Smith's experimental markets overcame the bounded rationality of the individual traders. Trading also corrected whatever K-T biases might have existed. "Poorly informed, error-prone, and uncomprehending human agents," Smith writes, produce outcomes "traditionally thought to require complete information and cognitively rational actors."[19]

However, not all experimental market designs converged to prices predicted by the rational paradigm. In Simon's terms, what was decided depended on how it was decided.

A second striking experimental result was the poor performance of trading in resalable assets, such as stocks or bonds. Bubbles—with prices far above their fundamental value—formed and collapsed in such assets. This happened even when all traders had complete information about the interest or dividends the asset would generate—and knew that all other traders had this information. This result which went beyond Keynes's bubbles that occur because future profits are highly uncertain (chapter 8) surprised Smith.[20]

Smith then "proposed a brilliant explanation for the bubble and crash pattern" for assets that are not bought for immediate consumption. "All traders might well be rational, but if this rationality is not common knowledge, traders might speculate in the pursuit of capital gains, and bubbles might arise." [21] Smith also discovered that designing market rules and structures to prevent asset bubbles was much more challenging than getting the prices of consumption goods right.

Returning to my overall argument: these difficulties suggest how mutual uncertainty about mental capacities and temperaments affect collective outcomes, even when there is no asymmetric information. Moreover, arm's-length trades and terse market messages often cannot control dysfunctional interactions. Detailed justifications and dialogue can become crucial for cooperative action (although Smith's experiments do not examine or show this).

Elster's Mechanisms Elster's critique of the "Excessive Ambitions" of the social sciences provides another helpful perspective on behavior.[22] Elster argues that economics and other disciplines that study human conduct cannot produce the kind of reliable general laws that the natural sciences deliver: "There are simply very few well-established general laws in the social sciences. The 'law of demand'—when prices go up, consumers buy less—is well supported, but as laws go it is pretty weak," writes Elster. The law predicts the direction of the change in demand but not its magnitude.[23]

Social science can instead identify "mechanisms," which Elster defines as "frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences."[24] Proverbs summing up folk wisdom of general principles or situations suggest such mechanisms, often in

> mutually exclusive pairs. On the one hand, we have "Absence makes the heart grow fonder," but on the other "Out of sight, out of mind." On the one hand we may think that forbidden fruit tastes best, but on the other that the grapes beyond our reach are sour. On the one hand, "Like attracts like," but on the other "Opposites attract each other." On the one hand, "Like father, like son," but on the other "Mean father, prodigal son." On the one hand, "Haste makes waste," but on the other "He who hesitates is lost." On the one hand, "To remember a misfortune is to renew it," but on the other "The remembrance of past perils is pleasant."[25]

Either side of such paired proverbial mechanisms—or both—can be "triggered under generally unknown conditions or with indeterminate consequences." At most, we know that one pair member will be triggered, "but we cannot tell which." Moreover, "Some people may not be subject to either member of these mechanism pairs," while in other cases, "simultaneous triggering of two mechanisms with oppositely directed effects" makes the net effect indeterminate.[26]

The indeterminacy of Elster's paired proverbs* thus mirrors Knight's observation that uncertainty makes conduct "erratic and extremely various from one individual to another."[27] This indeterminacy differs greatly from K-T's *predictable* biases, documented in uncertainty-free circumstances. Additionally, in my interpretation, indeterminacy creates mutual doubts. A cannot predict what B will do and vice versa, even when A has no doubts about B's incentives or information. Their collaboration thus often requires justificatory discourse, with reason-giving and taking.

Gigerenzer's Critiques Gerd Gigerenzer, who received his PhD in psychology from the University of Munich in 1977, has unwaveringly criticized the research that K-T pioneered and is the only critic to whom K-T ever responded. Behavioral

* Enthusiasm for the indeterminacy of paired proverbial wisdom is not universal. In 1946, a young Herbert Simon had attacked Gulick and Urick's then-classical theory of administration in "The Proverbs of Administration," published in the *Public Administration Review*. Simon's article claimed, disapprovingly, that the basic principles of the Gulick and Urick theory "were not principles at all, but proverbs, full of wisdom, but always occurring in mutually contradicting pairs" without specifying "when and under what circumstances, which proverb is valid." Urick, Simon recalls, "never forgave me for this attack but Gulick became quite friendly in later years. Presumably he made allowance for the hubris of a young man" (Simon 1991d, 269–270).

economics, writes Gigerenzer, "began with the intention of eliminating the psychological blind spot in rational choice theory," but has "ended up portraying psychology as the study of irrationality." It portrays people as having systematic cognitive biases that are "persistent" and "costly in real life—meaning that governmental paternalism is called upon to steer people with the help of 'nudges.'"[28]

Yet there is little evidence that "alleged biases are potentially costly in terms of less health, wealth, or happiness."[29] Hundreds of studies have found little evidence that "irrational" attention to framing is costly.[30] In fact, a "bias bias"—the tendency to spot biases even when there are none—taints much of the research in behavioral economics. The bias bias mistakes "random error for systematic error" and confuses "intelligent inferences with logical errors."[31] In reality, psychological research suggests that people "appear to have largely fine-tuned intuitions about chance, frequency, and framing."[32]

Bias-Bias Examples

The "overconfidence bias" and framing effects are noteworthy targets for Gigerenzer's critique. Gigerenzer cites DeBondt and Thaler's (1995) claim that "overconfidence" is "perhaps the most robust finding in the psychology of judgment and choices."[33] But results are highly sensitive to how researchers frame their questions. For example, asking, "Are you a better driver than average?" (to which more than half of respondents tend to say "yes") subsumes the unposed question—how "better"? If good driving means avoiding accidents, more than half of drivers can objectively regard themselves as better than average. The number of accidents per person has a skewed distribution, and about 80 percent of US drivers have fewer accidents than the average number of accidents. Designing questions to evoke overconfident responses—or interpreting the results as such—suggests that the problem comes from the "bias bias" of researchers rather than from the minds of subjects.[34]

Similarly, Gigerenzer attacks K-T's evidence for framing effects. Recall that K-T used the "lives saved–lives lost" questions to construct "transparently equivalent versions of a given problem" that "yield predictably different choices." Gigerenzer argues that K-T's two framings are not equivalent; there are subtle yet important differences in wording. Remove the differences, and the framing effect disappears. More generally, sensible listeners should "expect that what and how the speaker communicates is relevant." They can anticipate that "a speaker is likely making an unspoken recommendation when using a positive frame for an option, whereas a negative frame likely indicates a warning." Therefore, "The ability to listen carefully and pay attention to how messengers frame messages" indicates "intelligence, not bias."[35]

On the positive side, Gigerenzer has followed Herbert Simon's path of studying heuristics to cope with bounded rationality in its Simonian sense—namely, limits to what we can know and process. He argues that "fast and frugal" heuristics, designed for specific tasks, are crucial when information or time is scarce. Even when choices are routine (as in prioritizing emergency room patients or granting bail), professionals often cannot and do not rely on a com-plex statistical model. They use simple decision protocols with yes/no forks and without any probability estimates.[36]

Gigerenzer also argues that good "biased" heuristics can be superior to unbiased but erratic models. Darts that consistently hit the dartboard just to the right of the bullseye beat unbiased hits scattered far away.[37] (The late and legendary economist Ziv Griliches once jokingly suggested random selection of students to Harvard's PhD program because "randomness eliminates bias.") And Gigerenzer questions why "consistency" in following the rules of deductive logic or probability theory should be a benchmark for rationality. Heuristics well adapted to specific tasks can be "inconsistent" across different tasks. And there are no recorded examples of "money pumps," operated by Ramsey's imaginary "cunning bettor" (chapter 8), bankrupting violators of axiomatic rationality; money pump arguments are just "logical bogeymen."[38]

Daunting Obstacles

As we end this survey, it will not surprise readers that I agree with Gigerenzer's claim that behavioral researchers often exaggerate mental defects, confusing "intelligent inferences with logical errors."[39] This does not at all mean that I believe people are reliably reasonable. Like Adam Smith, Marshall, Schumpeter, Knight, Keynes, and Simon, I agree that emotions and caprice often drive human conduct. But like Simon, Ellsberg, Gigerenzer, Kay and King, and several K-T critics, I reject using uncertainty-free benchmarks. We live in a world infused with ambiguity and one-offs, both trivial and consequential. Statistical risks are the exception, not the rule. Accordingly, we rely on conventions, authorities, social media "influencers," abduction, contextual inference, and Simon- or Gigerenzer-style heuristics, not statistical models or logical deduction. Uncertainty, even about simple known unknowns, makes this reasonable, not a behavioral error or bias.

Simple uncertainties can also make it impossible to numericize confidence. I may, if asked, incorrectly guess that Rome is south of New York, using the heuristic (as Gigerenzer points out) that warmer places are usually closer to the equator. But I would have no rational basis whatsoever to offer a numerical estimate of my confidence in my guess. I would not even know what it means to

say that I am 40 percent confident rather than 60 percent confident. However, I might make up a number to be polite to a behavioral researcher—without fearing money-pump exploitation by Ramsey's cunning bettor.

Uncertain wants pose similar problems. When researchers ask subjects how much they would pay for some implausible experience, such as kissing their favorite movie star, they receive whimsical responses anchored to some irrelevant piece of data just planted in the subject's mind by the researcher, such as Social Security numbers. Does this show people are irrationally susceptible to framing, as researchers claim? Or that they blurt out the first thing that comes to mind to earn their five dollars for participating in the experiment? ("Snappy answers to stupid questions," a long-ago feature from *Mad* magazine, came to mind when I heard about the kissing experiment at a Columbia seminar.)

Uncertain wants can also preclude logical standards for "now" against "later" choices. We cannot know now what we will want in the future.[40] We cannot even know after the fact whether we justifiably sacrificed our future wants for current pleasures. If I splurge on an expensive car, I may jeopardize my long-term financial security. Or if I work long, unpleasant hours, I could retire early. But even assuming these expectations are correct, how can I know when the time comes if I made the right choice? Yet that is what behavioral models of rational "time-consistent" choice assume—that our future wants are knowable and comparable to our current wants. Real people cannot meet such standards.

Behavioral research also exaggerates mental lapses by ignoring differences between abstract and contextual reasoning. Worse, it treats abstract deduction as a universal gold standard when one-offs require contextual inference.

Abstract Deduction versus Contextual Inference

Abstract reasoning is a recent development of human civilization. Mathematical and syllogistic deduction goes back just a few thousand years at most, and statistical and probabilistic reasoning goes back just a few hundred years. In contrast, contextual choices and inferences go back more than three hundred thousand years to cave-dwelling humans.

The more recent advances in abstract reasoning have certainly been remarkable. Much of modern science and technology depends on the logical and mathematical manipulation of abstract constructs. And over a relatively short period in human history, competence in deductive reasoning has become widespread: high schoolers learn calculus, and college students solve partial differential equations. Moreover, as increasing scores on IQ tests (the Flynn Effect) suggest, our general capacity for abstract reasoning continues to improve.

But abstract reasoning isn't a be-all and end-all. As Keynes suggested in his *Treatise*, numerical probabilities amenable to mathematical treatment are exceptional, and Hayek highlighted the importance of specific contextual knowledge vis-à-vis scientific knowledge. James Flynn (progenitor of the Flynn Effect) argues that IQ tests only measure an "abstract problem-solving ability," not "real-world problem-solving."[41] Contextual inference—and what John Forrester, the Cambridge philosopher of science, calls "thinking in cases"[42]—remains ubiquitous.

Notwithstanding its impressive contributions to science and technology, abstract deduction is most dependable for entities governed by the laws of nature or, in the case of software, logical rules. Predicting and managing human conduct—to whatever degree possible—still relies mainly on informal contextual inference. Even in the natural sphere, general principles by themselves rarely get the job done. Technologies—"technical recipes" in Carliss Baldwin's evocative metaphor[43]—invariably embody extensive contextual inferences.

Similarly, discourse about subtle contextual inferences often demands natural metaphorical language rather than abstract mathematical symbols. Using algebra instead of words would not make the arguments Supreme Court justices hear and the opinions they write clearer or more logical. Even mathematical deduction requires natural language to convey practical meaning. And as philosopher Nancy Cartwright points out, "thick" verbs and metaphors enrich causal accounts even of natural phenomena and inanimate devices. Thus, the sun *attracts* planets, and the carburetor *feeds* gasoline.[44]

The ubiquity of uncertain contextual inference raises questions about many behavioral experiments and results. What do they test or demonstrate? Do they really establish widespread biases and other cognitive defects? Is it plausible that Tversky's Stanford subjects—including many students who must have aced college entrance tests and demanding calculus courses—were incapable of trivial logical deduction? An alternative interpretation suggests that the behavioral experiments deliberately mislead subjects. They posed simple reasoning problems in natural language rather than through abstract symbols in uncertain, highly contextualized settings. This tricked subjects into modes of reasoning that are useful or even unavoidable in such conditions.

As Kay and King argue, people do not "reason probabilistically" when responding to the questions of behavioral researchers. Instead, they interpret questions "in the light of their broad contextual knowledge."[45] But unlike Ellsberg, who worried about trickery and misunderstandings and regretted violations (chapter 10), K-T did not. Kahneman was explicit (as mentioned)

in seeking to elicit biased responses. Contextual misdirection, which magnifies the usual extent of mistakes, is a helpful device for bias hunters.

Disregarding contextual uncertainty also exaggerates the predictability of biases as well as their severity. As Knight argued and Elster's propositions about paired mechanisms suggest, uncertain circumstances spur uncertain responses. Yet behavioral economists claim to find systematic, predictable biases regardless of context. The claim seems exaggerated, most obviously in financial markets. Like many, I believe markets can fluctuate for no rhyme or reason because of illogical mood swings. I have traded against such swings, expecting sanity to return when they pass. But I do not rely on a systematic formula or algorithm; my assessments are contextual "one-offs."

To my knowledge, legendary traders like George Soros and value investors like Warren Buffett also examine specific circumstances when they place bets. Per Forrester, they "think in cases." Their investment style does not at all require systematic biases. As Buffett has told Berkshire Hathaway's shareholders, "Occasional outbreaks of those two super-contagious diseases, fear and greed, will forever occur in the investment community. The timing of these epidemics will be *unpredictable*. And the *market aberrations* produced by them will be *equally unpredictable*, both as to duration and degree. Therefore, we never try to anticipate the arrival or departure of either disease. Our goal is more modest: we simply attempt to be fearful when others are greedy and to be greedy only when others are fearful"[46] (italics added).

Conversely, I am unaware of fortunes made from trading systematic anomalies like the January effect. The Fuller and Thaler behavioral fund has not been a runaway success.[47] Market anomalies are, at best, only temporarily persistent, and individual booms and busts have a unique, unpredictable character.[48]

That said, Gigerenzer's critique has failed to secure mainstream attention or acceptance in economics. The German psychologist has published prodigiously, attempted to create a community through extensive collaborations, and has won the respect of elite economists like Vernon Smith. But his work remains unknown to the rank-and-file economist. In contrast, behavioral economics and finance have blended into the mainstream. And their uncertainty-free popularity, like that of information economics, discussed in chapter 7, poses a daunting obstacle for my Knightian project.

As we have repeatedly seen, incorporating new ideas into economics requires alignment with its paradigmatic norms and forming cohesive communities of like-minded scholars. Once the discipline assimilates new ideas, they are nearly impossible to dislodge. Further deviations must coexist with accepted approaches. For example, information economics coexists with the earlier microeconomics, and behavioral finance coexists with—and indeed relies on—theories that assume practical omniscience.

Therefore, although I agree with Gigerenzer's critique, I will not take it any further. Instead, I try to demonstrate how uncertainty can provide a helpful com-plementary view of modern enterprise. My demonstration, following Simon's approach, also analyzes how procedures and routines affect what boundedly rational organizations do and do not do.