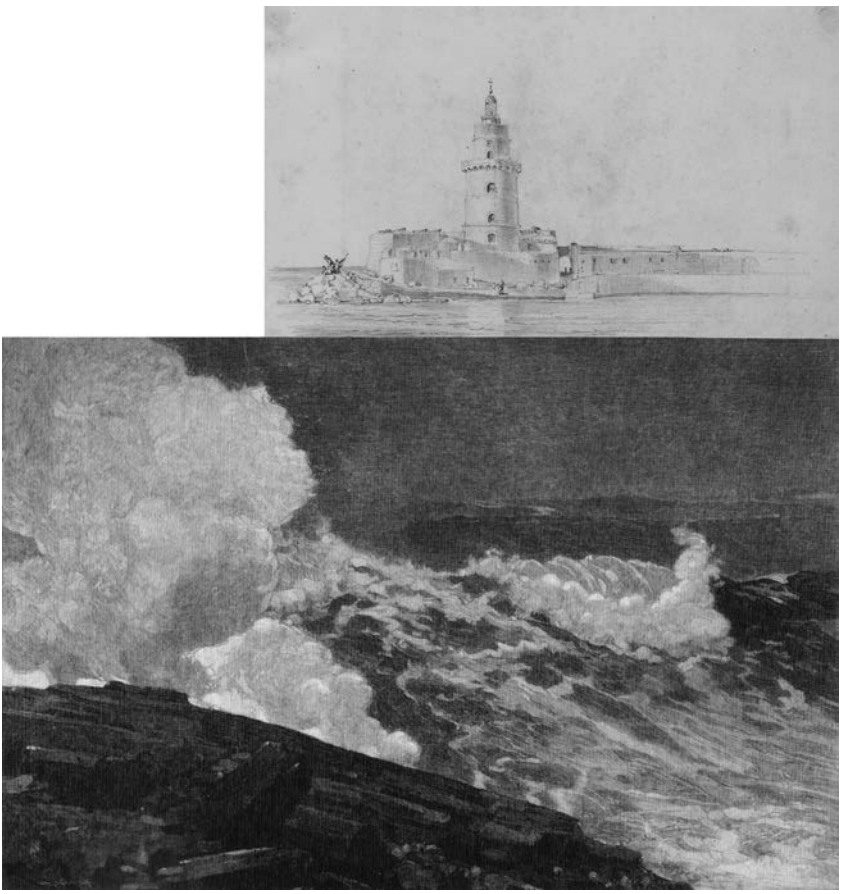
PART II

FORMIDABLE OBSTACLES, FORGOTTEN BEACONS



Compiled from images downloaded from the Smithsonian's Open Access collection. The bottom left image is Henry Wolf's *North-Easter*, 1908. The image to the top right is Miner Kilbourne Kellogg's *Lighthouse, Civitavecchia,* 1843.

5

Frank Knight: The Spark That Did Not Ignite

I shall conclude by recalling a conversation with Professor Ronald H. Coase when he and I were colleagues at the University of Virginia, where Frank Knight had visited for an extended period. Coase and I were walking along Mr. Jefferson's Lawn, and we had been discussing famous economists. Ronald said something like the following to me. "I can think of almost any famous economist, like '_____,' '_____,' or '_____,'" naming the obvious world-renowned figures in our discipline as evaluated from the perspective of the early 1960s, "and I can sort of imagine myself in their position of fame with a bit of luck, persistence, and effort. But I simply cannot imagine myself to be like Frank Knight. I guess that amounts to saying that Knight is a genius." I have always remembered that conversation because Coase put so well what so many of us feel when we think of the professor from whom we learned so much.

—James Buchanan (1982)

The historical tour in this and the other chapters of Part 2 aims to understand why Knightian uncertainty never took off and what could obstruct its modernization.[1]

Frank Knight and his 1921 book are a natural first stop. The legacy of the man and the book are curious. Revered by distinguished students, Knight was by all accounts an incomprehensible lecturer. Skeptical and cantankerous but not a hermit, Knight helped found the University of Chicago's legendary interdisciplinary Committee on Social Thought. His doctoral dissertation, published as *Risk, Uncertainty and Profit* became a classic—a dream result for any PhD student—but no other landmark publications or research followed.

How mainstream economics treated Knight's book is noteworthy. As mentioned, Knight's claims about uncertainty and profit never entered formal economics. Incongruously, his book's most enduring, if unnoticed, legacy seems to be its analysis of competition in the *absence* of uncertainty.[2] According to Nobel Laureate and Knight's Chicago student George Stigler, Knight's definition

of perfect, uncertainty-free competition was "an enormously influential part of the book." Thanks to the book's "clear and succinct statement of neoclassical price theory . . . Lionel Robbins made it a basic text at the London School of Economics."[3] Yet Knight had analyzed uncertainty-free competition as a steppingstone to showing how true profit requires uncertainty. Stigler found this part of the book "lack[ing] substantive structure."[4]

The rest of this chapter discusses three possible reasons that Knight's ideas about uncertainty failed to catch on.

- Knight's writing style, possibly reflecting his haphazard education, is challenging.
- Knight did not follow through with his ideas about uncertainty.
- Mainstream economics developed a scientific paradigm—opposed by Knight—that became a formidable barrier to uncertainty-based research.

1. Problems of Writing Style

Knight's writing limits the influence of his book and thus of Knightian uncertainty. Even sympathizers call it "complex and difficult to interpret" and Knight's approach "murky."[5] Others are more scathing. Economists Stephen LeRoy and Larry Singell, for example, express "much sympathy for those who take away from *Risk, Uncertainty and Profit* the opinion that Knight simply had no very clear idea of what he was talking about." They scorn the book's imprecise writing and "extended Austrian-style disquisitions on the foundations of human knowledge and conduct." "Almost all readers," they assert, "will at times despair of extracting any core of original insight from the overripe fruit of Knight's prose."[6]

Knight's inaccessible prose style may reflect his background, the origins of his book, and his skeptical temperament. Knight had a haphazard early education; his teachers likely did not stress elegant prose. He wrote *Risk, Uncertainty and Profit* (RUP) as a PhD dissertation—an oeuvre that does not always require clear writing.

The Making of a Maverick

Frank Hyneman Knight, born in 1885, was the oldest of eleven children of an Illinois farm family. When Frank was twelve, his father took him out of school to work on the farm. Knight nonetheless read avidly, somehow propping books on the frame of a horse-drawn plow. Around that time, Knight was baptized, following the Evangelical custom—but influenced

by a freethinking humanist, he became a skeptical antidogmatist. This attitude would extend far beyond a distaste for organized religion (although he retained a deep interest in theology and delivered sermons in Unitarian churches).[7]

A contract he had earlier made with his father returned Knight to school when he turned eighteen. He enrolled at a high school in Kentucky, lodging with his mother's family, the Hynemans. His mother's younger brother, Uncle Lev, just seven years older than his nephew, became a "major influence."[8] Lev started a bicycle repair shop while still in school and started the Lexington Home Telephone Company in 1897. Knight worked there as a nighttime switch operator when he lived in the Hyneman home. Political economist (and leading Knight historian) Ross Emmett speculates that observing his uncle's telephone business might have influenced Knight's views of entrepre-neurship and uncertainty.[9]

In 1905, the soon-to-be twenty-year-old started at a now-defunct religious college in eastern Tennessee. He left after two years, when the college faced severe financial difficulties, and found a secretarial position in Virginia for an employer who soon declared bankruptcy. A second secretarial position in Milligan College, another small Christian institution in eastern Tennessee, followed. Knight also taught shorthand and typing at Milligan—and took courses that earned him a bachelor's degree from the college in 1911. He then earned a second bachelor's (in the sciences) and a master's in German from the University of Tennessee.[10]

In 1913, Knight, now twenty-seven, won a scholarship for a PhD in philosophy at Cornell University. But, after a year, he switched to economics, banished from philosophy by a professor who had high regard for Knight's intellect but thought Knight's "ingrained skepticism" would "destroy the true philosophic spirit wherever he touches it."[11] Knight wrote his dissertation, "A Theory of Business Profit," and, on the promise of polishing and publishing it later, got a PhD in 1916. The following year, Knight submitted a version of the dissertation in an essay contest. It came in second to "The Results of Municipal Electric Lighting in Massachusetts," which one Edmund Lincoln had written for his Harvard PhD.[12]

In 1921, Houghton Mifflin published a revised and extended version of Knight's second-place essay per their agreement with the contest organizers. With this publication, as a book titled *Risk, Uncertainty and Profit*, Knight finally fulfilled the promise that had secured him a waiver from Cornell's rules for PhD degrees. By then, Knight had joined the economics faculty at the University of Iowa, where he would teach for ten years before moving to the University of Chicago.[13]

Knight's epistemological, ethical, and psychological qualifications make RUP's arguments difficult to disentangle. Thus, while Knight favored the aspiration of theoretical economics to become an exact science like physics, he acknowledged that it was a "human science." And because economic conduct is often not "rational or planned," this imposes "notable restrictions" on theoretical economics. The extensive acknowledgment of the "restrictions" and the paradoxes they produce often swamp Knight's main arguments in RUP.[14]

2. No Follow-Through

In 1927, Knight returned to the University of Chicago, where he had previously taught for two years. Cornell tried to lure him away from Chicago in 1928 and Harvard in 1929. But Knight continued teaching at Chicago until 1958 and remained there until he died[15] in 1972 at age eighty-seven. The university was hospitable to Knight's broad interests and appointed him Professor of the Social Sciences and Philosophy.[16]

Knight is "usually considered to be the founder of the Chicago school of economics."* His doctoral students included Nobel Prize winners and libertarian-conservative icons Milton Friedman, George Stigler, and James Buchanan. Knight cofounded the anticollectivist Mont Pelerin Society with a group Friedrich Hayek had convened that included Friedman and Stigler. Despite their "notoriously diffuse presentation," his lectures turned incoming "soft socialists," like Buchanan, into libertarians.[17] Hayek called Knight "the man who among Americans has probably done most to spread an understanding of the working of a free society."[18]

Yet Knight had a "deep ambivalence" about capitalist societies and refused to "extol the virtues of markets without drawing attention to their manifest limitations and sins." In a 1922 essay, he wrote that the "economic man is the selfish, ruthless object of moral condemnation."[19] Before the 1932 presidential election, Knight addressed the University of Chicago's Communist Club on why "those who want[ed] a change and wish[ed] to vote intelligently should vote

* Stigler (1985 *Preface*). Note the "usually." Moreover, Stigler (1985, 2) writes that Knight was "clearly the dominant intellectual influence upon economics students at Chicago *in the nineteen thirties*" (italics added). And Buchanan (1982, ix–x) pointedly calls Knight, "the primary intellectual source for the *original*, or *pre-Friedman*, 'Chicago school' of economics." Buchanan notes that "Knight put his stamp on several generations of students who learned *economics with philosophical overtones*" (italics added)—not a salient feature of cut-and-dried Friedmanite economic methodologies.

Communist."[20] According to Stigler, Knight relentlessly asserted that a "competitive enterprise system inherently leads to a cumulative increase in the inequality of the distribution of income." Friedman challenged this at "countless lunches." Each time, Knight would "make temporary concessions only to return to his standard position by the next lunch."[21]

Knight's heresies included methodology. In a biting *Journal of Political Economy* essay, he wrote that "the saying often quoted from Lord Kelvin [that] 'where you cannot measure your knowledge is meagre and unsatisfactory,' " was, in the social sciences, "misleading and pernicious. . . . Insistence on a concretely quantitative economics means the use of statistics of physical magnitudes, whose economic meaning and significance is uncertain and dubious . . . The Kelvin dictum very largely means in practice, 'if you cannot measure, measure anyhow!' . . . Perhaps we do not 'know' that our friends really are our friends; in any case an attempt to measure their friendship would hardly make the knowledge either more certain or more 'satisfactory'!"[22]

Here, too, Knight departed from Friedman's methodological approach. Under the emerging norms of disciplinary economics, that Friedman influenced, economists would only accept propositions they could logically deduce from first principles or verify through objective data. But the uncertainty-profit thesis, although intuitive, is purely definitional. Uncertainty and true profit, as Knight defines the terms, are both conceptual, not measurable. Their relationship, therefore, cannot be falsified or supported by objective data. Furthermore, Knight's "no uncertainty, no profit" formulation does not imply "more uncertainty, more profit."

Knight did not try to nurture a community to advance his ideas about profit and uncertainty. RUP was Knight's "only effort in this area. His subsequent career led elsewhere, so he did not engage with subsequent interpretations of this work."[23] Some scholars claim that Knight later renounced or downplayed his distinction between risk and uncertainty.[24] And unlike Milton Friedman, Knight was not interested in a broad audience which might have attracted attention to his work. Friedman produced a popular TV show, *Free to Choose*; wrote columns that went head-to-head with Paul Samuelson's; and advised presidents and prime ministers.

In contrast, writes Buchanan, Knight "did not address his words to the agents who might hold positions of governmental political power over others. Even in a remote conceptual sense, Knight was not an adviser to governments, a characteristic that, in itself, separates Knight from so many of his fellow economists, in his time and now."[25]

3. Paradigmatic Barriers

Overall, during his long career at Chicago, Knight and mainstream economics were ships sailing in opposite directions. The first sentence of Knight's 1921 book tells us that economics is "the only one of the social sciences which has aspired to the distinction of an exact science." [26] In subsequent decades, economics went much further in this direction. It adopted what the philosopher of science Thomas Kuhn called the "paraphernalia of specialization" that gives sciences their "prestige"[27] and a supporting "paradigm."

Paradigms, per Thomas Kuhn

According to Kuhn, who established its present-day meaning, scientific paradigms "define the legitimate problems and methods of a research field."[28] In the pre-paradigmatic stage, scientists record several facts and observations but with competing explanations fitting different facts. Paradigm-enabling breakthroughs combine "two essential characteristics." First, they explain a wide range of facts. This "achievement" is "sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity." For example, gravitation explained the parabolic paths of cannonballs on Earth and the elliptical orbits of planets in the skies. Darwin's theory of evolution explained a vast diversity of life forms. A second essential characteristic is that the breakthroughs are "sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve."[29] Modern physics and evolutionary biology began and did not end with Newton's and Darwin's contributions. They were seminal, not terminal.

The two characteristics enable research communities to undertake "normal science." Compelling, unified explanations legitimize agreements that researchers do not question. The "open-endedness" is also crucial because it gives researchers something to do. A breakthrough that solved everything would not; if breakthrough discoveries were a job requirement, few scientists would find employment.

Paradigms are typically tacit and unspoken, cultural rather than codified. They do not require a common acceptance or understanding of the basic assumptions or "axioms." What researchers do and how they do it defines their "paradigm." The doing produces agreements about pertinent problems, methods for solving those problems, and solutions the researchers consider acceptable.[30] Kuhn's scientists do not even need to make their axioms explicit "in order to know how to 'go on,'" writes the Cambridge philosopher of

science John Forrester. "A paradigm is what you use when 'you don't have to have agreement about the axioms.'"[31]

Paradigmatic theories, writes Kuhn, are learned by the "study of *applications* including practice problem-solving both with a pencil and paper and with instruments in the laboratory. If, for example, the student of Newtonian dynamics *ever discovers* [italics added] the meaning of terms like 'force,' 'mass,' 'space,' and 'time,' he does so less from the incomplete though sometimes helpful definitions in his text than by observing and participating in the application of these concepts to problem-solution."[32]

The unspoken agreements and half-understood concepts, internalized through paradigmatic examples, profoundly influence normal scientific research. The agreements accelerate advances by aligning the efforts of many scientists, but they also discourage unconventional research. Only a confidence-shattering crisis, produced by the accumulation of observations contradicting basic, unquestioned assumptions, creates room for a new paradigm.[33]

Like communities in the natural sciences, economists developed and enforced conventions for acceptable methods and results. Deductive equilibrium models—mathematical derivations of what eventually happens under certain assumptions—"axioms"—emerged as the gold standard.

Axioms, Old and New

The dictionary, writes Marsay, defines an axiom as a "self-evident truth." This usage applies to much of Euclidean geometry, which starts with self-evidently valid assumptions. But in modern mathematics, axioms "are just those propositions about the truth of which mathematics is silent."[34] Economics has followed a similar progression. Like ancient geometry, it started with seemingly self-evident tendencies, like the desire for wealth (as stipulated by John Stuart Mill). Now, like modern mathematics, standard economic theory uses axioms as starting points for mathematical deduction. Any claim to real-world validity lies mainly in the correspondence of the deductions with observable statistical data. Moreover, economists, like other scientists, internalize their axioms by doing, by repeatedly solving "pencil and paper" problems, and by application. No deep understanding of what they really mean is required.

Initially, economists merely favored research that followed the emerging paradigmatic methods, but over time, they virtually required it. The top economics journals now summarily reject submissions without an equilibrium model or statistical tests of equilibrium models. And top journal publications make or break careers. According to University of Chicago economics Nobel prize winner, James Heckman, tenure committees often rely on counting papers published in the "top five" journals, instead of carefully reading the candidate's work.[35]

Moreover, observes Stanford economist David Kreps, economics is the only social science that has a robust and cohesive paradigm, although it isn't otherwise more scientific than the other social sciences. The paradigm provides "a way of approaching questions in the economic and social realm, which we [economists] apply the way religious missionaries apply the catechisms of their faiths, namely without doubt or question."[36]

Knight took a different approach. "For Knight, the primary role of economic theory," Stigler wrote in his 1985 tribute, was "to contribute to the understanding of how by consensus based upon rational discussion we can fashion a liberal society in which individual freedom is preserved and a satisfactory economic performance achieved. . . . That is why the larger part of Knight's writings are outside of technical economics; indeed, that is why Knight did not return to the subjects constituting the main contributions of RUP."[37]

Knight was "unawed by either the 'wisdom of the ages' or the potential censure of his peers in the academy," Buchanan writes. He "did not preach a gospel (despite the old University of Chicago saying that 'there is no God, but Frank Knight is his prophet'). There was, to him, no gospel to be preached. He made no effort to present the 'truth according to Frank Knight.' He taught that 'truth' was whatever emerged from the free discussion of reasonable men who approached the dialogue without prejudice and as good sports."[38]

Continues Buchanan, "As he himself acknowledged, and as many others have recognized, Frank Knight was essentially a critic. His work, aside from *Risk, Uncertainty and Profit*, can be interpreted as a series of long book reviews. His 'social function' was that of exposing the fallacies, nonsense, and absurdities in what was passed off as sophisticated-scientific discourse."[39]

Knight believed that the task for economists was "located squarely at the level of elementary common sense," not science, and had "a highly skeptical attitude" toward "empirical research." He would find "particularly disturbing" the old-fashioned image of man as a wealth maximizer that "the modern emphasis on empirical testability forces on the economist." The reemergence of *Homo economicus* reflects "retrogression into a simplistic and wrongheaded usage of the valuable insights that economic theory can offer. *Homo economicus* exists in every man, but one of Knight's most persistent themes through all his works is that there exist all sorts of other men (the romantic fool, the sportsman who

enjoys the fray, the prejudiced ignoramus, the man who wants to be a 'better' man) alongside the rational maximizer of economic interest."[40]

Knight "categorically rejected the elitism too often met in the academy and at the same time reaffirmed his own faith in a society of free men. While he remained always pessimistic as to its potential realization, such a society was, for Knight, the only one worthy of serious consideration."[41]

For better or worse, Knight was on the wrong side of history. His 1921 prediction that "mathematical economics . . . seems likely to remain little more than a cult"[42] couldn't have been more wrong. Knight's long editorship helped put the University of Chicago's *Journal of Political Economy* (the *JPE)* in the very top tier—and he published many of his own math-and statistics-free articles in it. Now the *JPE* sternly enforces the discipline's methodological purity.

Instead of Knight's free-thinking, economics has "a well-developed orthodoxy," writes Kreps, himself a distinguished and largely mainstream scholar. Economists "respected and played by the same basic rules set forth by earlier generations" and had "a clear conception" of what they did and did not know and "how to work on things unknown." Kreps traces the rules and conceptions to a paradigm established in the 1950s and 1960s, which gave economics "remarkable unity and consensus" and the "ability, through unity, to defend itself and to arrogate to itself particular perks and benefits."[43] It also made economics "a monolithic and smugly self-satisfied scientific discipline."[44]

In my view, the economic paradigm did not follow the patterns of evolution that Kuhn had described in the natural sciences. Its foundational theories did not provide a unifying explanation for many inexplicable facts, and they did not resolve an intellectual crisis. However, they had paradigmatic features that attracted researchers: they provided agreement about core assumptions and many follow-on puzzles for further research—which Knight's book did not. They also conformed to the scientific aspirations of the discipline.

The next chapter examines a cornerstone of the economics paradigm and why it rejected Knightian uncertainty.

6
Practically Omniscient Microeconomics

Milton Friedman was characteristically direct about his erstwhile teacher's construct in his 1962 textbook:

> In his seminal work, Frank Knight drew a sharp distinction between *risk*, as referring to events subject to a known or knowable probability distribution and *uncertainty*, as referring to events for which it was not possible to specify numerical probabilities. I have not referred to this distinction because I do not believe it is valid. I follow L. J. Savage in his view of *personal probability*, which denies any valid distinction along these lines. We may treat people as if they assigned numerical probabilities to every conceivable event.[1]

Kenneth Arrow, a less blunt Nobel Prize–winning economist, had also previously written, "Knight's uncertainties seem to have surprisingly many of the properties of ordinary probabilities, and it is not clear how much is gained by the distinction."[2]

Friedman's claim that Knightian uncertainty excluded numerical estimates has been disputed.[3] But it conforms to the overall spirit of Knight's argument. Knight associated uncertainty with subjective opinions formed when situational uniqueness ("one-offs") makes objective calculation impossible. Knight also observed that the impossibility of objective measurement is ubiquitous. And everyday experience suggests that we often use words, not numbers, to express our opinions when we cannot make objective numerical predictions. Therefore, Friedman's impression that Knightian uncertainty excluded numerical estimates is understandably widespread.

That said, people do occasionally use numbers to express opinions about one-off possibilities. They may even offer monetary bets on them, like bookmakers offering odds on horse races. But for my modernization project, other aspects of opinions about one-offs are more consequential than their numerical expression. I am concerned about the fallibility and disagreements arising from missing information (e.g., wrongful convictions or hung juries in trials when the evidence is entirely circumstantial) and the implications for entrepreneurship. Friedman and Savage's personal probabilities and the microeconomics they support keep this out: they ignore ignorance, mistakes, and disagreements. Yet as we

see in this chapter, this microeconomics has become paradigmatic. Specifically, the main sections of this chapter examine how

- Personal probabilities became a building block of the subjective expected utility (SEU) theory that
- Became a cornerstone of the modern economics paradigm, which in turn
- Conflicts with my modernization project.

1. The Development of Subjective Utility Theories

Objective to Subjective Choices The mid-nineteenth-century English philosopher John Stuart Mill had, as we will later see, described economics as the study of conduct directed to the acquisition of wealth—and monetary wealth is objective.[4] However, more than a century earlier, Swiss mathematician and physicist Daniel Bernoulli had introduced "ideas of utility and expected utility-maximizing behaviour."[5] Utility—as in the satisfaction derived from wealth or anything else—is naturally subjective, as is its "expectation."

By the end of the nineteenth century, economics had settled on subjective utility rather than objective wealth as the goal of human conduct that the discipline would analyze. However, the core expectation theories, derived from mathematics and statistics, remained objective. Based on analyses of games of chance, such as dice, or data, such as mortality tables, these theories analyzed questions about distributions: What proportion of bets on black in roulette will lose? Or what is the life expectancy of a sixty-year-old? Expectations about situationally unique one-offs remained a "scientifically unfathomable mystery of life and mind," as Knight had put it.

In 1954, Jimmie Savage (referred to by his initials "L.J." in the Friedman quote above) proposed a comprehensive theory that included one-offs in *The Foundations of Statistics*. The book was published when Savage was a professor in the University of Chicago's statistics department, which he had cofounded in 1949.[6] Simply put, the theory expects rational decision- makers to estimate quantified "utilities" of the outcomes of their choices (what satisfaction they expect to get) and the probabilities of each outcome. Multiplying utilities and probabilities leads decision- makers to their "best" option, which maximizes the multiplication result.

Savage's theory specified logical, self-recommending rules—or postulates as Savage called them—for such maximization. Following the postulates ensures that all utilities and probabilities are rational in the sense of being logically consistent. The theory also assumes what Knight had called "practical omniscience"—meaning that decision-makers assume they know everything they

need to know. At the same time, the theory does not require true omniscience—objectively correct assessments of utilities and probabilities. The source of the estimated probabilities—whether they are mathematically deduced, inferred from statistics, or just wild guesses—is also irrelevant. This makes Knight's distinction between subjective uncertainty and measurable risks irrelevant.

Landmark Synthesis Savage's theory did not come out of the blue. Widely considered a genius, Savage had, like Newton, "stood on the shoulders of giants." The giants included:

- Thomas Bayes, an eighteenth-century English clergyman and statistician. Bayes (1701–1761) formulated (but did not publish in his lifetime) what came to be known as Bayes' theorem—a rule for estimating probabilities from statistical distributions that incorporates prior knowledge.
- Frank Ramsey, a precocious British philosopher-mathematician. Ramsey (1903–1930) attacked the ideas of his mentor, John Maynard Keynes, about nonnumerical probabilities. Ramsey proposed expressing all probability estimates, including subjective guesses that have no statistical basis, as numerical betting odds (as we see in the next chapter).
- Bruno De Finetti, an Italian statistician-actuary. De Finetti (1906–1985) independently developed and clarified Ramsey's ideas about subjective probabilities in the 1930s.
- Hungarian-born mathematician John von Neumann[7] and the German-born economist Oskar Morgenstern. Von Neumann (1903–1957) and Morgenstern (1902–1977), who had both immigrated to the United States in the 1930s, made significant contributions to utility theory and several other topics in their 1944 classic *Theory of Games and Economic Behavior.*

2. Paradigmatic Cornerstone

Scientific Aspirations Savage has been credited with changing the "Kuhnian" paradigm in statistics.[8] Savage's theory also became a cornerstone of a paradigm that conformed to economists' goals of a physics-like science.

As I have argued in *Making Economics More Useful*, scientists, as opposed to engineers and humanities scholars, favor simple, universal propositions and precise models that produce "equilibrium" solutions. Newton's second law of motion, $F = ma$, and Einstein's law of mass-energy equivalence, $E = mc^2$, exemplify the gold standard; fuzzy historical or literary explanations for World War I or Hamlet's torment are the antithesis. Engineering blueprints, while precise,

are also not scientific exemplars because of their granular detail and limited generality.

Subjective utility maximization had the desired scientific qualities. Savage expressed his postulates in precise mathematical terms. Calculating expected utility by multiplying the utilities of possible outcomes with their subjective probabilities is self-evidently unambiguous (and its resemblance to *Force = mass * acceleration* is noteworthy, if entirely coincidental).

Extensions and Application The theory also had Kuhnian "paradigmatic" potential for extension and elaboration. Discounting future utilities added a temporal dimension (as in the net present value calculations of future cashflows that became a staple of modern financial analysis). The theory could also be applied to make folklore about the invisible hand more precise: with unfettered competition, individual utility maximization would be the best way of matching what people wanted with what could be produced. At the same time, the inferences and assumptions of the theory provided an attractive target for skeptics: they enabled technical attacks on the invisible hand and rationality. Without such a target, advocates and skeptics alike could only make assertions.

Friedman, who had coauthored a 1948 paper with Savage that presaged Savage's more complete 1954 theory, became an enthusiastic promoter of utility maximization. Friedman's 1962 book (which endorsed Savage's rejection of Knightian uncertainty) was "one of the first textbooks to talk about expected utility" and became a "classic in graduate school [economic] education."[9] Other economists spread subjective utility maximization beyond microeconomics. Macroeconomists used the device in "rational expectations" theories and finance researchers in modeling the risks of portfolios. Business schools taught it in MBA programs in decision trees.

Decision Trees

When I attended Harvard's MBA program from 1977 to 1979, the school did not require coursework in economics. It still does not. However, the business school had a managerial economics department whose members had made pioneering contributions to decision theory. They included mathematicians and statisticians John Pratt and Howard Raiffa, and Robert Schlaifer, a PhD in ancient history. (The three had coauthored a seminal 1964 article on subjective probabilities.[10]) The department taught a required first-year course Managerial Economics, in which decision trees were a basic building block. Solving a decision tree required estimating the probabilities for alternative outcomes and the values realized—subjective utility maximization.

As a twenty-one-year-old who had just endured a five-year bachelor's ordeal at the Indian Institute of Technology, I was dazzled by decision trees and the brilliance of John Pratt, my section instructor. Many older classmates with real work experience but less math in college were not as enthusiastic. Decision trees taught through made-up cases also did not easily fit HBS's tradition of discussing real cases. Eventually, in the 1990s, the managerial economics department was disbanded (at the behest, it was rumored, of Dean John MacArthur), and its flagship course was removed from the MBA curriculum.[11]

The new paradigm helped economics enter remote domains. Gary Becker (who won an Economics Nobel in 1992) used utility maximization to analyze racial discrimination, crime, family relationships, and rational addiction and to argue that seemingly self-destructive choice could be considered utility-maximizing. Herbert Simon (whose 1978 Economics Nobel came fourteen years before Becker's) sharply observed that the choices of Becker's *Homo economicus* extended to the bedroom where "he would read in bed at night only if the value of reading exceeded the value (to him) of the loss in sleep suffered by his wife."[12]

Criticisms Skeptics, including Simon, have also wondered whether earlier and simpler constructs could not have provided the same explanations and predictions. Could subjective utility maximization, they ask, merely offer opportunities to display technical virtuosity and score difficulty points for mathematical gymnastics?*

The practical applications have also attracted critical scrutiny. Savage had acknowledged that his utility maximization theory applied to a "small world" where decision-makers might conceivably anticipate and consistently, if not correctly, estimate the probabilities of all outcomes.[13] Using the procedure for something as simple as "planning a picnic," according to Savage, was "ridiculous."[14] As a young college student, Herbert Simon attempted to apply utility maximization while working for Milwaukee's recreation department. He concluded that this was "hopeless."[15]

* Simon's (1978c) Richard T. Ely Lecture delivered to the American Economic Association questions the value of mathematized maximization. Compelled by "a sense of fairness" Simon cites one of his own papers (Simon 1951) explaining why employment relations are so widely used in society. "My argument," Simon observes, "requires a theorem and fifteen numbered equations. . . ." In fact, "the rigorous economic argument, involving the idea of maximizing behavior by employer and employee, is readily translatable into a simple qualitative argument that an employment contract may be a functional ('reasonable') way of dealing with certain kinds of uncertainty" (1978c, 5).

Kay and King's *Radical Uncertainty* further argues that applications outside Savage's "small world" promote dangerous complacency, particularly in financial markets and macroeconomic policymaking.[16] They also point out that many outcomes (e.g., "winning the war on terror") are so fuzzy that specifying numerical probabilities and utilities is impossible.

3. Conflicts with Uncertainty Modernization

Subtle Omissions The disconnect between Savage's approach and my modernizing project goes beyond the impossibility of anticipating all outcomes or estimating numerical probabilities. For me, simpler omissions are problematic. Notably Savage's model ignores errors from obviously missing information about "known" unknowns—such as the absence of a radar map in predicting rain. As in Knight's preliminary analysis of uncertainty-free competition, Savage implicitly assumes "practical omniscience." But omniscience precludes profit (as Knight defines it) and produces an irreconcilable conflict between entrepreneurship and paradigmatic microeconomics.

Banishing Entrepreneurship from Microeconomics

Early-twentieth-century economists, according to Humberto Barreto, had treated the entrepreneur as "a key agent."[17] But after the 1930s, "rapid intellectual changes" in microeconomics that produced a "perfectly interlocking, self-contained model"[18] required banishing entrepreneurship. The choice between a model whose pieces "fit perfectly together to form a grand, unified whole" and entrepreneurship was "an 'either-or' proposition" with "no happy medium."[19]

As Barreto further observes, excluding entrepreneurship created a tension between the informal discussions and formal theories of economists like Milton Friedman. Informally, they painted a picture of "brave, buccaneering" entrepreneurs. In contrast, their formal theories reduced the buccaneer to an ordinary ship hand with no room for initiative and any real decision-making rendered empty by the absence of uncertainty.[20] Friedman's popular book and TV show, *Free to Choose*, thus celebrated enterprise, while his scholarly writing excluded it.

Ignoring Disagreements Besides excluding entrepreneurship—the main application of my proposed modernization—Savage's utility maximization model also deviates sharply from my general conjectures about collective

conduct (previewed in Part 1). As mentioned, I aim to analyze interconnected and collaborative enterprise rather than atomistic or autonomous entrepreneurship and, more generally, problems of agreement and justification. A microeconomics that assumes practical omniscience ignores how people form judgments about one-offs and the consequences of their disagreements.

Recall from earlier chapters that judgments about one-offs draw on diverse—and typically contextual—information. The information may be widely available and public, precisely described (though not necessarily as statistical distributions), and the implications obvious. Or the information may be private and confidential, hard to codify, and ambiguous in its implications. These attributes, in turn, affect disagreements and their possible resolutions.

For example, a racehorse's track record (for once, literally) naturally influences the odds offered and bets placed. But what about a young horse that has never raced before? Here, bookmakers and bettors will rely on other nonstatistical information. This information might include the horse's pedigree—whether it is the offspring of a famous champion, the times it has clocked in pre-race workouts, the look of the horse's gait, and gossip in racing circles. These considerations have, to varying degrees, ambiguous implications. Pedigree and gaits are both observable, but pedigree is more precisely described and has clearer implications for a horse's prospects than gait. Gossip isn't public, is difficult to codify, and has ambiguous consequences for betting odds: Might the gossip be false? Or, if true, is this gossip known to bookmakers who have already included it in their odds? If not, how inaccurate might their offered odds be? Private, hard-to-codify, and ambiguous information will, in turn, produce wider divergences of opinions and more disagreements. One bettor might take gossip seriously, while another may not.

Divergent opinions and disagreements may not obstruct arm's length trades between individual buyers and sellers. Differences of opinion can even help businesses and markets that rely on speculation. Bookmaking requires gamblers who disagree with the odds offered. Stock exchanges similarly depend on differences in judgments about values and future prices. An assured consensus would drive out the speculation that keeps trading cheap and active.

But disagreements can obstruct entrepreneurial initiatives that individuals cannot unilaterally undertake. As I argue in Part 3, promoters must overcome the doubts of investors and consumers. Without convincing justifications, promoters cannot "make the sale" regardless of the prices or terms they offer or

how strongly or sincerely they believe in their schemes and products. Examining the interplay of doubts and justification can significantly improve our understanding of enterprise. But "practically omniscient" uncertainty-free microeconomics ignores these issues in entrepreneurship and, more broadly, in most other kinds of joint human activity.