CHAPTER 3

From Robbins to Friedman and Beyond

The last chapter focused on why and how Lionel Robbins in the early 1930s delimited economic inquiry. In this chapter, I return to Robbins' scarcity foundation for the discipline with an emphasis on methodological and analytical issues he (largely) set aside, or he viewed as outside of his scarcity boundary for the discipline. I am mainly concerned in this chapter with how Robbins and other economists, before and after Robbins (from Alfred Marshall to Milton Friedman and beyond), have treated scarcity in the external world, and failed to consider the implications of scarcity problem that besets the human brain before it considers the scarcity problems in the external world. My brief methodological history will give context to my shifting the focus of the discipline from scarcity in the external world to the scarcity within the human brain later in the book. As we will see in Chapters 5 and 6, this seemingly slight shift in the scarcity foundation of the discipline will powerfully affect economists' derived "generalizations," the treasured goal of Robbins, Friedman, and the other economists covered.

Robbins and Scarcity, Once Again

Scarcity in Lionel Robbins' (and Robbins' followers') construction of economics as a discipline is not dissimilar to Alfred Marshall's construction of supply and demand models at least in one regard; both constructions can be viewed, metaphorically, as involving two "blades" of a pair of scissors operating against one another. For Robbins, one of the scissor

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blades is the limited means in the external world facing off against the other blade, so many subjectively conceived ends that not all ends can be satisfied. The two blades are interactive and interdependent, one without the other is largely meaningless.

Scarcity doesn't exist—and economics has no scientific function—without both blades working against one another, again, in Robbins' view (employing Marshall's metaphor). The valuations people place on their ends ultimately give economic meaning to means, and limited means and the surplus of ends (without any requirement that ends be unlimited), in both Marshall's and Robbins' constructions, give rise to relative valuations and, on the margin, to relative prices. The two blades come together and find a form of resolution in markets through exchanges and prices, which resolve countless (but not all) conflicts.

Fixed Rational and Purposive Behavior

Under Robbins' construction of the discipline, economics is defined not so much by its methodology as by its core interest, the development of generalizations that emerge from the unifying theme of ever-present scarcity. Any premise of rational or "purposive" (Robbins' preferred adjective) decision-making is assumed and imposed on the analysis. That is, rational or purposive decision-making and behavior is an unchanging feature of the human condition (and brain) and does not change in response to changes in the institutional setting, or anything else, in the external world in which the two blades of scarcity squeeze together (which is a reasonable inference from Robbins' discussion of rational and purposive decision-making because he never considers different levels of rational or purposive decision-making and behavior). And in contemporary times, there is no mechanism in neoclassical microeconomic theory that would integrate *changes* in rational or purposive behavior into matters of, say, price determination. The level of rationality in decision-making (whether perfect or less than perfect) is subsumed, perhaps as a matter of making economic analysis manageable (because Robbins does seem to recognize that, along with economists from Smith to Marshall, the precision with which decisions are made is transparently variable, to one degree or another, in people's decision-to-decision daily lives). In the case of perfect rationality, precision in decision-making can't vary; it's always perfect, at least for analytical purposes.

Subjective evaluations, on the other hand, can change in Robbins' worldview with changes in the availability of means, at least on the margin. (Robbins recognized the prospects of diminishing marginal utility at the individual level, although, as noted, he considered interpersonal utility comparisons beyond the boundaries of economics.[1]) Nevertheless, he considered people's valuations of ends as data *given* to the analysis, with their determination largely, if not exclusively, the providence of other social sciences, most notably psychology. He suggests that subjective evaluations *must* and *should be* beyond the providence of economics.

Clearly, Robbins saw economics as a powerful set of analytical methods for understanding human behavior. At the same time, he saw the methods necessarily providing a partial view, given he excludes evaluation formation, as well as the subject matter of the hard sciences in which evaluations are totally absent. Presumably, he would agree that economists, in order to fully understand human behavior, would have to jump from one intellectual plain (and analytical method) to another, because of the absence of bridges among the plains.

Conventional contemporary microeconomics, which has been largely built on Robbins' scarcity view of the discipline, is grounded in a number of widely accepted postulates that contain the scope of the discipline's analytics:

- At least for analytical purposes, people are considered to be no less rational and purposive under subsistent scarcity conditions as they are under much less pressing conditions.
- As openly assumed by neoclassical economists, while market equilibrium prices are tied to people's evaluations through supply and demand forces, the prices people are willing to pay for goods—for that matter, their demand schedules—do not affect people's evaluations of the goods, in total and on the margin, except under special circumstances that have been posited since the 1930s (e.g., network goods). This means that prices that represent "good deals" do not affect people's evaluations of goods subject to trades. The values of goods are not affected by the difference between the prices people actually pay and the prices they expected to pay. That is, prices and evaluations are assumed to be independent (even though economists, Marshall and Robbins, seem to agree that any founding premise must be founded on "indisputable facts of experience").

- Costs and prices can change with the forces of scarcity, but rational and purposive decision-making and behavior do not change with the degree of scarcity of goods and prices, at least in economic theory at the center of Robbins' construction of the discipline.
- When trade is undertaken to exploit comparative cost advantages, the resulting welfare improvement does not affect the rationality or purposiveness of production and consumption decisions. Moreover, people's rationality is unaffected by how trade allows for greater specialization of, say, labor. No consideration is given to how specialization of labor permits specialization of neuronal resources within the brain, which can affect the efficiency and rationality of decision-making within the brain.
- The competitiveness of market structures (from perfect competition to pure monopoly) does not affect the precision with which resources are used in production and costs are minimized. Costs are everywhere minimized (as represented by conventional cost curves or functions); profits are maximized within the operative competitive restraints. Perfect competitors, who are very likely principals in their respective small firms and who stand at all times on the cusp of being eliminated from the market for any market miscalculation, will minimize costs and maximize profits (to the extent that they can) with the same dedication as the agents within a monopoly firm (even when imagined, for analytical purposes, to be a cartel of former perfect competitors) protected by entry barriers. The monopolist's only assumed meaningful function is limited to making its good scarcer than otherwise in order to collect monopoly rents. A monopolist's protected market position, in other words, leaves the rationality and purposiveness of decision-making unaffected under the Robbins' paradigm.
- When economic activity is shifted from the private sphere to the government sphere, the rationality or purposiveness of decision-making is also assumed to remain unaffected by the institutional shift from perfect competition or pure monopoly to government bureaucracies, and vice versa. The efficiency of decisions and behaviors, of course, change, but only because the external constraints on decisions and behaviors change, not because the shift in constraints affects exactly how people are inclined to make decisions and conduct their behaviors.

- Scarcity is a "problem" that must be solved (or just abated) for welfare to be enhanced. Economists rarely mention, must less explore, how the persistence of the "problem" itself, and its (partial or limited) abatement, can add to people's welfares. The presumption is that the advent of abundance (if it could ever be achieved, which is ruled out by people's capacity to create new wants) cannot lower people's welfares because they no longer have challenges in life, which can have value in themselves. People's rationality is assumed to be the same regardless of the extent of "the problem," or the extent of life challenges.

All said, in Robbins' worldview, shared widely by economists for nearly a century, efficiency in production and consumption is achieved under an (implicit) assumption of constant rationality. If rationality were allowed to change, then the concept of *efficiency* would take on a different meaning.

Under Robbins' construction of economics, the human brain is literally a "black box" that is taken as a given and that does its work "behind the scenes" (not Robbins' characterization), so to speak, outside the scope of economic analysis. Little to nothing is said under the Robbins' scarcity paradigm about the limitations of the brain and how those limitations can affect human decision-making and behavior, or, for that matter, how those limitations can place boundaries on the subject matter of economics or how economists conduct their analyses (other than economists' mental limitations that require them to devise theories that are sufficiently simple to be manageable). The brain works its magic apart from the binding constraint of scarcity that is assumed to be true of all resources capable of delivering value—aside for the human brain. Again, the human brain is simply a given, not subject to variation within individuals or across them. But then, it must be remembered that Robbins, and his disciplinary forbearers and followers for decades to come, had at best a primitive understanding of the workings of the human brain. While people's understanding of the human brain remains primitive today (given how much is not understood), neuroscience has, in recent decades, unmasked some (or is it "much"?) of the prior mystery of how the brain works.

Under Robbins' construction, economists' forte is, first, recognizing the pervasive ends/means conflicts and, second, drawing out economic

generalizations that emerge from thinking of people making unavoidable choices and tradeoffs, all purposively. Scarcity implies choices that imply costs for all choices, which means that people must engage in some form of purposive and optimizing behavior, whether precisely or roughly executed.

In Robbins' construction of the discipline, mental limitations on understanding the external world are not totally absent, just substantially downplayed as central to what economists do and how they do what they do. After all, Robbins noted how rationality at some level is, in the real world of decision-making, necessary to presume people have ends and choose among them: "And thus in the last analysis Economics does depend, if not for its existence, at least for its significance, on an ultimate valuation—the affirmation that rationality and ability to choose with knowledge is desirable."[2] At the same time, he never uses "perfect rationality" because there had to be cost/benefit limits to process of refining decisions. In effect, for Robbins any proposed perfect rationality as a foundation for economic decision-making had to be an evolutionary nonstarter. Similarly, he warned against jettisoning rationality in economics altogether, in favor of theories based only on ever-present influences of external forces or "irrationalities":

> If irrationality, if the surrender to the blind force of external stimuli and uneo-ordinated impulse at every moment is a good to be preferred above all others, then it is true the raison d'etre of Economics disappears. And it is the tragedy of our generation, red with fratricidal strife and betrayed almost beyond belief by those who should have been its intellectual leaders, that there have arisen those who would uphold this ultimate negation, this yearning for the deep unawareness of the unborn state, this escape from the tragic necessities of choice which has become conscious. With all such there can be no argument. In love with death, their love will overtake them. For them there can be no "way out" save the way which leads out of life. But for all those who still affirm more positive values, that branch of knowledge which, above all others, is the symbol and safeguard of rationality in social arrangements, must, in the anxious days which are to come, by very reason of this menace to that for which it stands, possess a peculiar and a heightened significance.[3]

Moreover, Robbins recognized that the economic models economists use are hardly fully descriptive of the world that they analyze, and they cannot be fully descriptive, given the complexity of the intertwined and

interacting physical and social worlds. However, Robbins adds nothing else in the way of economic generalizations, or theoretical implications, to the implied insight that the human brain, which must be used to analyze scarcity, is itself in short supply and has functional limits and imperfections, just like all other means in the external world of pervasive scarcity.

In Robbins' world, the only human brains of any consequence to economic methods and generalizations are those possessed by economists' subjects (consumers, investors, and producers) who have to cope with complex human interactions under conditions of pervasive external scarcity. Subjects' brains might be recognized as a scarce resource that need to be used with care and with concern for costs, but subjects' brains are not considered on par with other factors of production—say, labor, capital, and technology—in economists' analytics. The limits or peculiarities of people's brain powers did not dictate economists' methods and generalizations derived, other than the admission that analytics must be made manageable with simplified models. Again, human brain power is treated as a side issue, if at all. Similarly, the brains of economists-qua analysts are not instrumental to their methods and generalizations, other than, I repeat, they force a reduction of complex reality into simplified economic models.

The Human Brain as a Scarce Resource

By excluding the scarcity of human brain power from economic analysis, Robbins (and his followers, including almost all conventional microeconomic theorists today) gave economists license to shift with conceptual ease from a realistic, or descriptive, assumption that people's decision-making and behavior were grounded in some form of rough and ready rational or purposive decision-making to an assumption of universal perfectively rational or purposive decision-making. After all, any assumption, no matter how refined, of less-than-rational or purposive decision-making necessarily had to be less than descriptive of actual decision-making among real-world people, especially given the (likely) substantial variation in the precision of decision-making across individuals. That is, any less-than-perfect decision-making premise could be (almost) as flawed as an assumption of perfect rationality and would certainly make the analysis more complex, perhaps unnecessarily so, with the models likely far less amenable to mathematical treatment. There is

no basis in neoclassical economic theory for deciding on what level of less-than-perfect rationality premise, among a multitude of such premises, would be used. Unless all less-than-perfect-rationality premises are employed serially (making for an endless testing process), the decision on the founding premise must be arbitrary at best, as is, admittedly, the premise of perfect rationality. Hence, any assumption of less-than- perfect rationality would likely make economic theorizing convoluted and would not likely advance the derivation of economic generalizations that could be subject to real-world testing. Besides, the essential goal of theory in neoclassical economics is not to provide self-evident stand-alone *truths*, which is not possible. Rather, the essential goal of theory is far more modest (and realistic), to provide *insights*—via the generation of hypotheses—about people's behavior that might not otherwise be uncovered but that can be empirically tested at various levels of precision (from casual observations to sophisticated statistical analyses).

Friedman's Canonization of Robbins' Methodology

In the early 1950s, Milton Friedman canonized for generations of economists Robbins' view of the governing role of founding premises and theories in microeconomics in his *The Methodology of Economics*.[4] Following in the methodological footsteps of John Neville Keynes, whom he quotes with approval at the start of his essay, Friedman posits only two broad categories of economic analyses, *positive economics* and *normative economics*. As is commonly accepted (and parroted) today, the former is restricted to discussions of "what is," and totally "independent of any particular ethical position or normative judgment," at least in principle: "It's [positive economics'] task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. It's performance is to be judged by the precision, scope, and conformity with experience of the prediction it yields." Accordingly, positive economic analysis can be an "objective' science" in the sense that the physical sciences, especially physics, are viewed as objective.[5]

Of course, as Friedman concedes (as Austrian economists, including Ludwig von Mises insisted all economists must[6]), economics has major impediments in achieving objectivity that do not encumber the physical sciences: First, positive economics deals with the interactions of human beings with whom economists can feel intimate and communal

connections. Second, economists themselves are a part of the "subject matter" of economic analysis, with their investigations potentially biased and distorted by their assessments of preferred findings from economic analysis (which means that positive economics can be infused with normative considerations, even when unintended).[7]

Another major difference between economics and physical sciences not acknowledged in Friedman's essay (but surely recognized by him and, for that matter, Robbins) is that a major force in people's economic interactions is their valuations of goods and services and alternative outcomes from changes in circumstances, which can be ephemeral and elusive as compared to the type of concrete and unalterable data points available to the physical sciences. The rocks of geology (presumably) are unconcerned with subjective valuations of their circumstances, or where they are headed or are moved, just as planets have no capacity to assess where they are heading and toward what end and to seek corrective courses of actions to optimize their subjective evaluation of their trips through space–time.

Similarly, chemicals in experiments react in their circumstances according to known physical laws, not to the chemicals' assessments of their circumstances and alternative circumstances they might imagine. Moreover, rocks, planets, and chemicals can't talk back to physical scientists (and influence how they are treated in experiments) and are not typically assumed to be affected by scientists' probes or their findings. There are no potential feedback loops between what the objects do and the scientists' findings, which can change the objects' "behaviors."

On the other hand, people can talk back to economists, or, at least, they can if allowed to do so. And findings of scientific, positive economic analysis can affect people's interactions and the policy constraints they face. Friedman insisted that a major objective of positive economics is to guide people's policy preferences and decisions within the sphere of normative economics: "[D]ifferences about economic policy among disinterested citizens derive predominantly from different predictions about economic consequences of taking action—differences that in principle can be eliminated by the progress of positive economics …."[8] (That is clearly a testable hypothesis that warrants testing, which I've not seen.)

Robbins emphasized the conflict between the limited resources and, essentially, unlimited wants and the generalization that could be drawn from the conflict itself, but left economic methods largely elusive (not that he didn't have a methodology in mind). Friedman subsumed

Robbins-type scarcity and focused on the method economists use to conduct their analyses, more or less exclusively positive in nature (with lots of room for error in predictions because of the complexity of the subject matter and because of the often-time subjective nature of their measurement of their data points).

In Friedman's view of the discipline, the economic method starts with theory, which is an "intermixture of two elements." The first is a "language," or a set of tautologies, that facilitates systematic, organized reasoning. As such, economics provides a "filing system" that facilitates the organization of "empirical material," enhancing understanding. As a filing system, economics can be judged by whether (or the extent to which) the identified categories in the filing system are sufficiently "precisely defined" to reduce, if not eliminate, ambiguity and sufficiently broad that they can be "exhaustive" in coverage of causative considerations. For Friedman, supply-and-demand models of markets work well as methodological "filing systems." The two functions are precisely defined, and virtually all market-moving considerations can be "filed" under "supply" or "demand" (as an acknowledged but often unstated proposition that the supply curve can be construed as a "reservation demand curve," and vice versa).[9]

The second "element," and prime purpose, of (micro) economic theory is, as with Robbins, to generate a "body of substantive hypotheses" that can only be judged by the theory's "predictive power for a class of phenomena which it is intended to explain," a role that requires "factual evidence" to settle the issue of whether the theory is "right" or "wrong," or, better yet, to determine whether the hypotheses are tentatively "accepted" or "rejected" as valid.[10]

Following Karl Popper's dictates for the pursuit of "science," theory is made necessary by the sheer complexity of the world external to the analyst. Complex reality cannot be understood in its full entirety because of limitations of the mind to handle the vast array of interconnected potential causative factors.[11] The best that analysts can hope to do is reduce the complexity to the proportion that are manageable by the human mind of the economist, which calls for "abstract models" that bring together in organized form "essential elements" of the world under study. Such models are hardly "abstract" in the sense that they are vague or have imprecise boundaries. On the contrary, they can be laid out precisely (even in detailed mathematical form) and are abstract only

in the sense they are reduced forms of complex reality, again, encompassing only "essential elements" of complex reality.

Empirical tests of the hypotheses generated are made necessary because of the "highly simplified" nature of the models used, which means the models necessarily lack completeness. Also, the model used to generate hypotheses is, to an extent, arbitrary, given that there are, at least in social sciences, several alternative assumptions undergirding the theory to be presented: "The choice among such alternative assumptions is made on the grounds of the resulting economy, clarity, and precision in presenting the hypothesis," in Friedman's words.[12]

Accordingly, the assumptions underlying theory cannot be tested by their descriptiveness (or conformity to complex reality). To make analyses manageable, assumptions about human motivation can be, and must be, greatly simplified, if not sterilized into unrecognizable forms—perfect rationality, for example—because of the dictates of economy in theorizing, or the need to ease (make less costly) analyses, or just to clarify and make more precise the hypotheses subject to testing. Besides, more descriptive, assumption can often do little more than muddle the analyses: "Complete realism [in founding premises] is clearly unattainable, and the question whether a theory is realistic 'enough' can be settled only by seeing whether it yields predictions that are *good enough* for the purpose at hand or that are *better* than predictions from alternative theories" (emphasis added).[13]

But make no mistake about it, for Friedman, in economics, as in the physical sciences, the "construction of hypotheses is a creative act of inspiration, intuition, invention; its essence is the vision of something new in familiar material."[14] At the same time, theorizing about economic behavior in the external world (mainly in markets) was itself an economic problem, beset with cost/benefit comparisons with optimization of net gain always a goal. Friedman wrote about how it "does not always pay to use a more general theory because the extra accuracy it yields may not justify the extra cost of using it, so the question under what circumstances the simpler theory works 'well enough' remains important."[15] He suggests that an assumption that billiard players can make their shots *as if* they can make the complex mathematical calculations required to hit and sink balls can work reasonably well in making predictions about the course of play. Similarly, an assumption that firms rationally calculate their most profitable pricing and production decisions in "full knowledge of the data needed to succeed in this attempt *as if* … they knew

the relevant cost and demand functions" can, within a "wide range of circumstances," yield theoretical contributions that make economic sense (emphasis in the original).[16] The fact that economic analysis, as imperfect and "unreal" as it is, has been accepted and used for such a long time and "the failure of any coherent, self-consistent alternative to be developed and be widely accepted is strong indirect testimony to its worth"[17] (a staked-out position that, as we will see in Chapters 4 and 5, the behaviorists now fervently challenge on all methodological margins with practically every study they publish).

Friedman's overriding point is that the founding premises of theories necessarily lack "realism" and are intended to provide an "economical mode of describing or presenting a theory." The founding premises are intended both to facilitate the drawing out of testable implications of the theory and to proscribe the circumstances under which the theory is expected to be valid.[18] Besides, Friedman reasoned that economic analysis did not cover all human behavior, but only that part of behavior that involved people's interactions, mainly their market-based interactions, in which there were checks on people acting in less than cost-minimizing and profit-maximizing ways:

> Confidence in the maximization-of-returns hypothesis is justified by evidence of a very different character. This evidence is in part similar to that adduced on behalf of the billiard-player hypothesis [that he shoots *as if* he can perform complicated calculations] – unless the behavior of businessmen in some way or other approximated behavior consistent with the maximization of returns, it seems unlikely that they would remain in business for long.[19]

By extension, people who act consistently irrational can either be driven to act more rationally than they would normally be inclined to do or they would lose market position to those market participants who acted more in line with the rationality assumption undergirding economic theory. For Friedman, his stretched assumptions regarding cost-minimizing behavior and optimizing behavior worked tolerably well within the sphere of his primary concern, markets: "The process of 'natural selection' thus helps to validate the hypothesis – or, rather, given natural selection, acceptance of the hypothesis can be based largely on the judgment that it summarizes appropriately the conditions for survival."[20] Thus, for Friedman rational decision-making is both a founding premise

and a consequence of a *process* of people's interactions, surely within markets and maybe elsewhere. (This is to say that rational behavior is both a premise of economic modeling and, and not so widely appreciated, a prediction of market processes. Perhaps more accurately, Friedman suggests, in my reading of his "Essay," markets make people more rational, as a group, than they might otherwise be.)

As much as economists-qua-scientists would like their theories to be totally objective, such can't be, because of the embedded arbitrariness of the founding assumptions and the rules devised for the pursuit of science. Moreover, Friedman stresses, "there inevitably will remain room for judgment in applying the rules," and judgment requires experience in doing science within the "'right' scientific atmosphere," in which "amateurs" can be separated from "professionals" and, ultimately, real "scientists" from "crackpots."[21]

In Friedman's view, neoclassical microeconomics in the tradition handed down from Alfred Marshal has been "extremely fruitful," in spite of limiting consideration—ever-present subjective evaluations—in the conduct of economic science, which is a nonissue in the physical sciences, most notably physics.[22] Microeconomics has been "fruitful" because, as he notes in his acceptance of the Nobel Prize in Economics (with reference to the initial acceptance and eventual rejection of the Phillips curve), "[T]he body of positive knowledge grows by the failure of a tentative hypothesis to predict phenomena the hypothesis professes to explain; by patching up of that hypothesis until someone suggests a new hypothesis that more elegantly or simply embodies the troublesome phenomena, and so on ad infinitum."[23] Here, in my reading of Friedman, he leaves himself open, at least somewhat, to revisions in the way economic science is done—maybe some of those revisions proposed by behaviorists and maybe even the more radical revisions I propose later in this book.

Becker and Stigler

Friedman's University of Chicago colleagues Gary Becker and George Stigler adopted Friedman's analytical methodology (in large measure but not totally), especially the part allowing for sterilized unrealistic behavioral assumptions, but dropped altogether Friedman's implied boundaries on where the methodology could be applied. Becker, especially, made a career of applying the "economic approach" beyond the strict boundaries of markets and invading the traditional domains of other social

sciences (1976). He pioneered work in discrimination (1971), human capital (1994), home production (1993), tastes, and the "economics of life" (1997), without apparent concern that Friedman's assumed market force of "natural selection" might not hold, at least not with the same force. Becker implicitly assumed (at least in his modeling and maybe in much of life) that people could be counted on to behave rationally without, necessarily, the constraining and directing force of market (-type) competition. Similarly, Stigler recognized how the economics of information affected people's market searches, making less-than-perfect decisions optimal and rational.[24] Becker and Stigler had no need for the feedback loops of competition on rationality because rationality can't be enhanced beyond perfection, which is embedded in their founding premise.

Stigler and Becker took Friedman to heart in crystalizing (and making unreal) the assumption underlying theory by asserting without qualification the fixity of preferences across people and time, at least for their analytical purposes:

> [T]astes neither change capriciously nor differ importantly between people. On this interpretation one does not argue over tastes for the same reason that one does not argue over the Rocky Mountains – both are there, will be there next year, too, and are the same to all men. … On the traditional view [of tastes], an explanation of economic phenomena that reaches a difference in tastes between people or times is the terminus of the argument: the problem is abandoned *at this point* to whoever studies and explains tastes (Psychologists? Anthropologists? Phrenologists? Sociologists?). On our preferred interpretation, one never reaches this impasse: the economist continues to search for differences in prices or incomes to explain any differences or changes in behavior.[25]

Their goal was to move economic analysis away from the disputes that can easily arise over the explanatory role of taste differences, which are difficult, if not impossible, to handle scientifically. Tastes are necessarily founded in subjective evaluations, which makes them difficult (if not impossible) to quantify. Becker and Stigler intended to direct economists' analytical attention to variables subject to change and to measurement, the external constraints people face (primarily incomes and prices). They sought to add to people's understanding of behavior, in response to changes in constraints, not to their taste changes.

Clearly, for Stigler and Becker, rational behavior has a nonrational foundation, perhaps grounded in intuition or in what they see as the self-evident objective reality of the equivalence of subjective preferences and goods (and bads). Becker, especially, is renowned for recognizing that many purchased goods are inputs into household production functions.[26] But then, Stigler, Becker, and Friedman can't help but give goods an objective reality (which Austrian economists are reluctant to do). There is really no point in trying to do *science* (with economic science having any claim of being even remotely like the physical sciences), as a predictive endeavor, if subjective preferences don't have objective (measurable) counterparts in the external physical world in the form of identified "goods," "prices," and "incomes" that can be observed and measured with tolerable accuracy. (Of course, Austrians have objected to claims that economics is, or can be, a true *science*, equivalent to the physical sciences. Friedrich Hayek dubbed any attempt to draw an equivalence as "scientism."[27])

Again, Friedman, Stigler, and Becker do not openly acknowledge a need for considering how internal, evolutionary, and neurobiological, constraints affect just exactly *how* rational people can be, although we gather they would not object to extending economic models to allow for maximization under such evolutionary and neurobiological constraints, given how many different and new directions they took economic analysis. Becker introduced "home produced goods" to his "household production function," and Stigler assumed "information" on prices is a "good." They built their esteemed careers on the assumption, with flexibility, of rational behavior variously constrained by sometimes specifically tailored utility functions in which the "goods" are identified for the research topic at hand (e.g., information is good with attendant production costs in one of Stigler's seminal articles [1962]). For Becker, as with Friedman and Stigler, economics was defined much more by its method than by the external scarcity that Robbins emphasized. As Becker asserted, "The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach."[28]

Opening for Criticisms

Modern neoclassical economists, of course, left themselves open for methodological assault on several fronts, which could, eventually, undercut (as the assaults have) the neoclassical economics preeminence among

methods of doing economics. We consider several avenues of economic practitioners' assaults on the discipline's core methodology.

Predictions and Empirical Tests

Foremost, Friedman and other neoclassical economists staked the discipline's credibility of its methods' ability to make predictions, the accuracy and value of which must (like any science) be assessed by empirical evidence. Friedman probably had in mind predictions and policies to the effect: "A higher minimum wage will lead to a reduction in employment among the covered worker groups." However, there are a host of other predictions that emerge out of an assumption of perfectly rational people: Such people can be expected, for example (among many similar deductions), to

- equate (at least, in some approximate fashion) at the margin, both in their production and consumption decision-making,
- ignore sunk costs and consider opportunity costs,
- discount with reasonable accuracy and consistency the costs and benefits for time and risks, and
- take (at least with some consistency) the courses of action with the highest expected net values.

Granted, Friedman (and his followers) insisted that founding premises could not be judged by their "realism" (or maybe even by their coherence with deductions), but at the same time Friedman—and, for that matter, Robbins—was unwilling to say that selection of founding premises was unconstrained, or could be totally divorced from real-world human behavior, or could be made up out of thin air with no expected correspondence between behavior and predictions (deductions) listed above.

Friedman did seem to be comfortable with Marshall's and Robbins' position that the discipline's basic premise had to be connected to "indisputable facts of experience." A theory's predictions' also had to be "good enough" (or better than those of alternative theories to obtain the approval of a community of economists operating as "scientists," not "crackpots."

If such predictions don't hold up to agreed-upon empirical tests with some frequency, then Friedman's methodological position would, so it would seem, lead to a reassessment of the validity of theory, on the order of,

well, maybe people are not nearly as rational as neoclassical economists claim, making the jump to perfect rationality for model purposes an untenable stretch. Such a reassessment might understandably cause practitioners of the discipline to seek some adjustment in the founding premise and methodology. If a large number of such predictions don't hold up to careful empirical (scientific) examination, maybe the rationality premise should be scrapped—or judged inferior to some other approach by recognized authorities within the discipline, to whom Friedman indicated he was willing to defer, and must defer, as final arbitrators, or what constitutes good economic science.

Possible Problems with Empirical Tests

As noted, Friedman writes as if empirical assessments of hypotheses generated with positive (scientific) methods could resolve with some finality and frequency normative policy disputes, or at least could be expected to reduce the frequency of disputes over normative ends (and maybe mitigate conflicts, if not hostilities, in policy debates). Friedman points specifically to the policy debate that arises when a hike in the minimum wage is proposed. He suggests that opponents and proponents are most likely in disagreement, not so much over the goal of helping low-wage workers, but over the unemployment and poverty effects (among a range of possible labor-market effects) of any minimum-wage hike. He seemed to accept the *dominant* empirical finding in the early 1950s and before that minimum-wage hikes undercut employment and seemed to believe that such empirical findings could (potentially, at least) settle the policy debate by undercutting support for hikes.

When he was writing in the early 1950s, Friedman did not seem to anticipate the growth in available data banks and the more rapid decline in the cost of computing power during the following half-century and more, which gave rise to a burgeoning econometric industry focused on assessing the employment and unemployment effects (or lack thereof) of minimum-wage hikes from a variety of competing academic and political perspectives. With the growing ability of economists and policy advocates to choose among data banks to test their theories, with differing measures of employment and unemployment for differing labor-market segments and to choose (with greater ease lower cost) among combinations of dependent variables, as well as independent variables, for their various (and sometimes numerous) regression equations (perhaps sometimes covertly concocted with policy agendas

in mind), support for minimum-wage hikes has ebbed and waned over the past seven-plus decades. Through the late 1960s, proponents of hikes pressed for hikes, which made their way through Congress, with the real value of the federal nominal minimum-wage peaking in February 1968 at $1.60, equal to $12.15 in 2017 dollars, at the end of 2017 (the initial minimum wage of 25 cents passed in 1938 was equal to $4.34 in 2017 dollars).

However, the growing buildup of studies after 1968 predominantly showing negative employment effects of hikes eroded, as Friedman expected, political support for minimum-wage hikes through the 1990s, causing the substantial erosion of its purchasing power. Even editors of the *New York Times* (who first vehemently opposed the first federal minimum-wage law and gradually shifted to full support in the 1970s) reversed their position by the mid-1980s and editorialized in 1987 in favor of a "right" minimum wage of "$0.00," all on the grounds of saving jobs for menial workers, a position founded squarely on economists' then dominant findings of negative jobs effects of minimum-wage laws.[29]

In early 2018, the nominal federal minimum wage had not been hiked from $7.25 since 2009, leaving the real value of the minimum wage a third below real-dollar peak in 1968 (although states and municipal governments have raised their minimum-wage rates to as much as $15 an hour by late 2017).

Beginning in the early to mid-1990s, economists began to find market segments in which the employment effects of minimum-wage hikes were minimal, if not zero (and possibly positive).[30] Policymakers began to realize that even a substantial majority of past econometric studies of minimum-wage hikes revealed limited employment effects (with job losses most often less than 3% of covered workers), even for the presumed most vulnerable worker group, teenagers.[31] As a consequence, debates over proposed minimum-wage hikes have remained as divisive as ever, and maybe more so, with the policy debate devolving to the state and municipal level and with hope waning that the magnitude of the unemployment effects of wage hikes could be resolved by empirical findings.

This is to say that proponents and opponents can now choose among studies to support their normative—political—positions to an extent not available when Friedman and Robbins were laying out what they saw as core issues in economics. Policymakers can even point to studies that posit that the minor employment effects can be chalked up to employers

forced by market competition to offset the cost effects of minimum-wage hikes with reductions in fringe benefits and increases in work demands.[32]

As of early 2018, proponents of minimum-wage hikes appeared to be gaining ground in policy debates at the state and municipal levels, with an array of hikes instituted in 2015 or scheduled for 2016 and beyond, with no end in sight for the spread of state minimum-wage hikes. California had scheduled annual hikes in its state minimum through 2020.[33]

The Economics of Doing Science

Given that the minimum-wage-policy debate remains unsettled, and as contentious as ever, might not Friedman (and Robbins) have missed something critical to how economics can be done, and maybe, should be done—and how doing economics can be expected to evolve (and will evolve) over time? Might the continuing minimum-wage debate reveal how undertaking positive economics leaves much room for normative judgments, if nothing else, over the quality of the databases and statistical methods used, as well as the extent to which normative considerations can drive the actual science done? After all, different scientific investigations can result in an array of assessments of what exactly are the effects of minimum-wage increases (not to mention healthcare benefits and mandates, farm subsidies, pollution taxes, and income-tax rates).

Put another way, as surely Friedman recognized, "what is" depends on any number of considerations, not the least of which is the ever-evolving technology and databases available for undertaking science. This means that reliance on conceptual models, which guide hypotheses testing, can be expected to be affected by the ease, or cost, of doing empirical research that, in turn, can be expected to affect the complexity and realism of economic models used to guide hypotheses testing. Indeed, no one should be surprised if the cost of accessing progressively larger databanks and doing sophisticated empirical assessments falls enough for economists to be less and less concern with model building of the type Robbins and Friedman thought was necessary for doing science, possibly at some point setting aside model development altogether and conducting science through computer-determined algorithms generated from "big data" bases, as seems to be a decided trend in economics and related fields (say, finance and marketing) in contemporary times.

Indeed, neoclassical microeconomic theory could predict that the way science is done will change with the prices of the various "inputs" of doing

any type of "science," not the least of which for deductive and inductive science are concentrated thought on model building, available databases, and technology available for empirical tests (or just finding algorithms that provide the "best fit" for the available databases). If the costs of data analysis have, over the decades, fallen relative to the cost of model building, which seems highly plausible, then the pursuit of science should be expected to shift from deductive reasoning toward inductive reasoning. With low-cost statistical manipulations, there is less need, so it may be reasoned, for worrying about reducing the count of "relevant" variables to manageable proportions (as once constrained by the cost function of findings and statistically manipulating an ever-greater count of variables).

Under either methodology, inductive or deductive analyses, the prominence and sophistication of data analyses should have risen over the last half century (an empirical issue in itself), but one that seems to have been the case from the most casual perusal of economic journals and policy forums over the past century, with acceleration of statistical testing over the past half-century. During Robbins' career, published philosophical discussions in economic journals were not uncommon. Such publications appear far less frequently today. Indeed, they are rare, and sophisticated empirical tests of hypotheses (carefully derived from highly technical mathematical models) are often thought-to-be mandatory for publications today, as contemporary economics graduate students are taught, and as their programs of study have progressively emphasized mathematical model building and econometric courses, which can further forced sterilization of underlying assumptions and obscure the real-world relevance of the economic analyses to practitioners and to policymakers.[34]

With growing emphasis on mathematical models for economic analytics, "perfect rationality" has evolved in its meaning and has become more open to criticisms. Without the mathematics, "perfect rationality" can be construed as having something of an economic foundation, in that it could loosely be equated with a form of optimal rationality under constraints. When math is introduced, any lingering presumption of a rational rationality is readily jettisoned, just to keep the math manageable. "Perfect rationality" implies mathematical precision in decision-making, with all tangency conditions fully met in equilibrium, which is totally open to attack by critics who can see, by that standard, omnipresent flaws in human decision-making that can be easily validated empirically, even though empirical tests are hardly needed (other than to diehard

economists who insist that their mathematical models are adequately, if not fully, descriptive of people's decision-making).

As economics become ever more mathematically precise in modeling human decision-making during the last half of the twentieth, psychologists, cognitive psychologists, evolutionary psychologists began demonstrating limitations and flaws in human decision-making that does not, and cannot, hope to match the precision economists' vision of mathematically precise rationality, as Richard Thaler and Cass Sunstein, and many others, have demonstrated with ease.[35] They ask their readers to consider drawings of two tables, adapted from the work of psychologist Roger Shepard.[36] One appears much longer and thinner than the other, which is how most subjects in laboratory trials "see" the tables, but both tables are exactly the same length and width. Psychologist Daniel Kahneman makes the Thaler/Sunstein point on people's visual limitations with two lines of equal lengths. However, the lines are displayed with two sets of "fins" on their ends. One set of fins for one line is pointing out and the other point in which makes one line look longer than the other.[37]

Similar visual illusions could readily be shown to exist among many subjects,[38] but this one illusion (among a host of illusions) makes the point psychologists of all stripes and behavioral economists frequently make: People subject to visual and other cognitive limitations cannot be as rational as economists suggest in their models. As we will see in Chapter 4, behavioural economists have expanded on an array of limitations and flaws in human decisions, so much so that some behaviourists reject the rationality premise altogether and argue for a theory grounded in pervasive and predictable irrationalities.[39]

The Emergence of Laboratory Economics

Even though Robbins and Friedman appreciated the need to simplify economic models to their essential features, both spurned laboratory experiments because they necessarily oversimplified complex realities of human interactions with built-in feedback loops that allow for corrections of misguided decisions, from which market outcomes emerge. However, Robbins and Friedman wrote at a time in which economists strongly doubted the credibility of what people would say they would do in imagined circumstances; they felt strongly that what people do, or are

observed to do, in real-world settings was far more reliable and revealing of preferences and behaviors.

Robbins and Friedman also had little to no experience with laboratory experiments, and they plied their trade when the technology for conducting tolerably realistic laboratory experiments was highly constrained and primitive, partially because computer technology was embryonic. Might not developments in the technology for conducting laboratory experiments since the 1930s and 1950s, with growing reliance on computer recordkeeping on the interactions of laboratory subjects and computer simulations, have made laboratory experiments more credible and accepted within Friedman's community of scholars who, as noted, could separate real "scientists" from "crackpots"?[40]

The awarding of Nobel prize in economics in 2002 to economist Vernon Smith for initiating the development of experimental economics and to behavioral psychologist Daniel Kahneman for his inaugural work in behavioral economics (with Amos Tversky) that directly challenged economists' rationality premise. Then, in 2017, the Nobel committee saw fit to award the prize to another behavioral economists Thaler for his substantial advancement of the behavioral work of Kahneman and Tversky and many other of their students and converts. These scholars' selection testified to the shift of economic methodology, now ongoing for at least five and maybe seven decades, away from the methodology of Robbins and Friedman and their followers, constrained by deductive theory, to behaviorists' work, constrained by researchers' ingenuity in setting up laboratory experiments, often weakly constrained (if not totally unconstrained) by guiding hypotheses deduced from a general theory of the kind that Robbins, Friedman, Becker, and Stigler had in mind.

With neoclassical economics gradually discredited over the decades in a growing number of scholarly minds with a multitude of laboratory experiments, no one should be surprised if the standard for acceptable inductive science erodes, with experiments being reduced to simple, if not simplistic, survey questions of this sort: "Consider two options A and B [with specified values], which do you prefer?" The subjects' answers might be given credibility from a community of behaviorists, if they violate some predicted neoclassical tenet.

Economics as a Way of Thinking

The economics discipline has moved on in another important way, as noted with far greater emphasis on mathematics, which has made the core concern less about scarcity per se and more about mathematical technique. Granted, practically all (if not all) modern economics textbooks pay homage to Robbins' identification of economics with scarcity, with any number of textbooks dubbing scarcity as *the* economic problem (but often without reference to Robbins as the source of the scarcity paradigm).

However, the defining core concern of the discipline has moved in one major, largely unheralded way after Robbins wrote his *Essay*. Through the work of Friedman, Becker, and their key colleagues at the University of Chicago, the core unifying concern of economics has shifted from being focused on derived implications of scarcity to the *method* economists use to derive and test their generalizations, subsuming scarcity. As noted, Becker has crystalized the economic method as relentlessly following the logic of three key assumptions in economic models: maximizing behavior, market equilibrium, and stable preferences, with a key test of the theory remaining Friedman's, the method's predictive value, not the realism of the underlying premises and analytics.[41]

With the *method* of analysis central to economics, the array of topics open for investigation became unbounded. Economists were no longer tied to a confined area for investigation, for example, "business," as Marshall had confined the discipline, possibly because he, as did Friedman, believed business arenas were heavily populated by self-selected people who were inclined to think in strictly self-interested, cost-minimizing, and profit-maximizing terms. Moreover, business behaviors were heavily guided by competitive pressures that selected out those business people who (and their firms that) were relatively less inclined to think "rationally" (with self-interest, cost-minimizing, and profit-maximizing goals in mind).

Marshall, and to a lesser extent Friedman, would not be surprised that economists' expanded array of topics for investigation to social arenas (for example, families and friends, politics, gangs, marriage and divorce, dying, crime, religion, education, academic governance, and so forth, within which economic-grounded competitive pressures could guide, through feedback loops, decision-making) would reveal weaknesses

in the discipline's predictive powers, as well as to reveal a growing gulf between economists' (perfect) rationality premise and people's real-world rationality (or lack thereof).

By maintaining that economics was a method of doing social science, Becker and those who followed him opened the discipline to a new challenge. If economics is a *method* that has no recognized bounded in application, there is no reason why the method should not be applied to all implications of the disciplines' own core methodology (below the level of, say, price will curb purchases). Rationality, especially the perfect variety, implies stark behavioral predictions, from which other predictions (the law of demand) are derived. As noted; rational people can be expected to equate at the margin, consider opportunity costs, ignore sunk costs, choose consistently, discount the values of choice options for risk and time. Behavioral economics has arisen in part to assess the validity of these predictions and, hence, the relative merit and reliability of economics as a method of doing science.

Concluding Comments

The domain of economic analysis expanded dramatically during the twentieth century. Alfred Marshall focused economic analysis on people as they work their ways through problems relating to business, fairly narrowly limiting its scope to market exchanges in which money plays a major role. Robbins found all such set boundaries for economic analysis limiting (for example, "business" or "material welfare"), because much economic science was pursued outside of business dealings and much production involved nonmaterial goods and services. He tagged scarcity as the pervasive and unifying concern of economists. He argued that economics was delimited by the "relationship between ends and means which have alternative uses," which means that Crusoe, stranded on his island by himself, faced economic challenges.[42]

Friedman accepted Robbins' reformulation of the discipline organized around scarcity, but he suggested, maybe only in passing, that the economic method was more likely to apply fruitfully to market settings. Competitive pressures were an important force that helped to legitimatize the founding rationality, or maximizing, premise of the discipline, which implied cost-minimizing decision-making. Markets helped to select out market participants who failed systematically, to one degree or another, to make rational, cost-minimizing, and profit-maximizing

decisions, causing market outcome to be more rational and efficient than people might naturally be predisposed to be. Adam Smith's "invisible hand" continued to rule in Friedman's methodology, but with greater emphasis on the selective pressures on the rationality of market participants than Smith may have had in mind.

We have to think Friedman would reason (accepting Marshall's "continuity principle") that the less competitive markets (or other social environments) were and the less weeding pressures there were in those environments, the less appropriate economic methods would be. Beyond some point is moving away from competitive market pressures, the economic methods could become more or less impotent in terms of generating insights, or so we (or Friedman's community of authoritative scholars) might surmise, at least in retrospect.

Friedman never registered full frontal attack on economists expanding economic inquirer beyond markets (and commercial spheres). However, from his passing suggestions, I suspect that Friedman was not always fully comfortable with his University of Chicago colleagues (and many other economists) applying economic methods in environments devoid of competitive and weeding pressures to push out decision makers who felt little pressure to respond to market forces and economize. At the same time, Friedman began, in limited ways, to shift economic analyses away from boundaries defined by subject matter or content (business, in the case of Marshall, and scarcity, in the case of Robbins) to boundaries defined by analytical methods, or "approach." For example, Friedman remains well known today for applying simple economic analytics to the efficiency of the military draft (relative to an all-volunteer army), the relative efficiency of different forms of welfare grants, and to educational choice.

For Becker, Stigler, and following economists, key organizing elements of economic analyses were, again, "the combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly."[43] At the same time, any discomfort Friedman may have felt toward economic imperialism could have been modulated by his recognition of the many insights his close, in-house colleagues, and others were generating outside of market settings.

As economic analyses evolved during the last half of the twentieth century, the method of economics became ever more detached from the analytical constraints that are imposed by the brain with which both analysts and subjects confront. Scarcity in the external world remained

centrally important to the economic approach, but the scarcity of mental resources was never fully integrated into neoclassical economic analyses as an analytical constraints or variable. (After all, a working premise of "perfect rationality" sets aside any and all scarcity constraints within the human brain.)

Psychologists, neuroscientists, and economists favoring behaviorists' methods took notice of the many predictions that fell out of economic models were at odds with their casual and research observations and laboratory experiments, but had been sidestepped by neoclassical economists. In the process, behavioral economists cast doubt on the value of achieving the kind of *improved* understanding with economic methods that Friedman held as a treasured goal of "science." The behaviorists across several disciplines began to see the generation of criticisms of economic methods as the equivalent of "shooting fish in a barrel" and set out to supplant the neoclassical microeconomic modeling of Friedman, Becker, Stigler, and many other economists with a theoretical paradigm that the behaviorists believed, and still do, meets with Friedman's criteria for any disciplines that aspire to be *science*, insights, and predictions empirically tested.[44] By the last half of the twentieth century, behaviorists had honed their laboratory and statistical skills in their search for "what is," but this time not through deductive testing of hypotheses, but simply by observing what real people do, and don't do in settings that they find congenial and productive of insights, but that Robbins and Friedman would likely find more than a little problematic, because of the absence of an undergirding general theory capable of producing deduced hypotheses subject to testing.

Notes

1. Robbins (1935, Chapter 6).
2. Robbins (1935, p. 141).
3. Robbins (1935, p. 141).
4. As included in Friedman (1953, pp. 3–46).
5. In his *The Ultimate Foundation of Economics* (1962) and several of his other works, Ludwig von Mises insisted that while the objects, animate and inanimate devoid of any capacity to form preferences, of hard science could be expected to behave in "regular patterns," human beings could form values and make preference judgements and pursue their difficult-to-predict ends. Accordingly, they could change the course of events,

making predictions of human events difficult, if not impossible, to predict. According to Mises, Friedman, and other neoclassical economists, in pressing for the adoption of a variant of logical positivism, were succumbing to their "shocking ignorance of everything concerning the science of human action," which required a manner of thinking not applicable to the hard sciences, with physics believed to be the "paragon of science." Friedman, however, insisted that economics should follow the physics paradigm.

6. Mises (1962, pp. v and 11).
7. Friedman (1953, p. 4).
8. Friedman (1953, p. 5).
9. Friedman (1953, p. 7).
10. Friedman (1953, p. 8).
11. Friedman makes the problems of working with complex descriptive theories clear in this passage:

> A completely realistic theory of the wheat market would have to include not only conditions directly underlying supply and demand for wheat but also the kinds of coins or credit instruments used to make transactions; the personal characteristics of the wheat traders, such as the color of each trader's hair and eyes, his antecedents and education, the number of members of his family, their characteristics, the weather prevailing during the growing season; the personal characteristics of the farmers growing the wheat and of the consumers who will ultimately use it; and so on indefinitely. Any attempt to move very far in achieving this kind of "realism" is certain to render a theory utterly useless. (1953, p. 32)

12. Friedman (1953, p. 40).
13. Friedman (1953, p. 41).
14. Friedman (1953, p. 43).
15. Friedman (1953, p. 16).
16. Friedman (1953, p. 21).
17. Friedman (1953, p. 23).
18. Friedman (1953, p. 23).
19. Friedman (1953, p. 20).
20. Friedman (1953, p. 20).
21. Friedman (1953, p. 25).
22. By way of contrast, macroeconomics had not yet, in the early 1950s, fulfilled the promise of being organized around something approximating "the" theory, again, according to Friedman ([1953, pp. 41–42]).
23. Friedman (1976, p. 267).

24. Stigler (1962, pp. 213–225).
25. Stigler and Becker (1977, p. 76).
26. Becker (1993).
27. See Hayek (1943, 1944).
28. Becker (1976, p. 5).
29. Editors, *New York Times*, July 17, 1987. For a history of the ever-changing position of the *New York Times* editors on the minimum wage, see McKenzie (1994).
30. Card and Krueger (1995).
31. For review of the literature on the labor-market effects of minimum-wage hikes, see Brown et al. (1982), Peterson and Stewart (1969, pp. 151–155), Kosters and Welch (1972), Ragan (1977), and Neumark and Wascher (2008).
32. For a review of the literature of this perspective on the labor-market effects (or non-effects) of minimum-wage hikes, see McKenzie (2014).
33. For a listing of the various state and municipal minimum-wage hikes in 2015 and 2016 and beyond, go here. http://www.xperthr.com/quick-reference/minimum-wage-rates-by-state-and-municipality/7038/.
34. Schabas (2014).
35. Thaler and Sunstein (2008, pp. 17 and 18).
36. Shepard (1981, 1990). Shepard's tabletop illusion is illustrated here. http://www.michaelbach.de/ot/sze_shepardTables/.
37. Kahneman (2011, p. 27).
38. See Michael Bach's collection (n.d.) of 130 optical illusions and visual phenomena here. http://www.michaelbach.de/ot/index.html.
39. See Ariely (2008).
40. Friedman (1953, p. 25).
41. Becker (1976, p. 5).
42. Robbins (1935, p. 16).
43. Becker (1976, p. 5).
44. Friedman (1953, p. 41).

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