Chapter 2
The Methodological Constraints on the Rationality Premise

The last chapter described how modern neoclassical economic theorists (mainly those who have an allegiance to the Chicago price-theory traditions) choose to define rationality in economics. That is, in that chapter, I considered the basic rationality premise without any consideration for how that premise might square with the theorists’ more broadly conceived philosophy of knowledge and without consideration of how that premise is influenced by the methodologies and technologies theorists use in their inquiries. This chapter explains more explicitly and formally how the methodologies that theorists employ in their inquiries more or less constrain the assumed nature of the rationality premise and how the assumption of rationality connects with their theory of knowledge.

The Constraints of Model Building and Hypothesis Testing on the Conception of Rationality

Positive economic science is almost always presented as a two-step process, with each step taken independently of the other – the underlying premises used in building and analyzing models, on the one hand, and in testing methods, on the other hand. An economist-qua-scientist develops a model as she sees fit without regard to the available techniques for drawing out hypotheses and running empirical tests. Accordingly, rational-behavior models can be justified on the grounds of simplicity and facility. Why use a complex and more costly behavioral assumption when a simpler, less costly one will do just as well? Or the behavioral assumptions are justified on the grounds that all theories are readily acknowledged abstractions, intended reduced-form parodies of how people actually do behave, which necessarily means that all theories are, to some degree, unreal. The importance of the realism of models is not a debate likely to be settled, a point Milton Friedman made with force (Friedman 1953, pp. 14–15), as noted in Chap. 1. We might as well assume people act “as if” they are fully rational, and test the usefulness of the
underlying model not by the accuracy of the as-if premise, but rather by what the premise and theory are supposed to do: generate useful insights and testable (empirically falsifiable) predictions.

However, such a perspective on scientific conduct fails to recognize how the methodology and technology of model building and empirical testing necessarily influence the choice of abstractions at the level of behavioral premises, which, in our case, is the extent to which people are assumed to be rational (fully rational or some degree less than fully rational). Use of mathematics constrains theorists’ choice of rational premises because mathematically grounded models necessarily limit the complexity of the choices available to choosers and the variety of motivations that can be activating them.¹

An example of the reduction in complexity of theory required by mathematics is the assumptions of independent utility functions and given preferences. Granted, mathematical economic models using interdependent utility functions have been developed to allow for actors who are motivated by concern for others as well as themselves. However, one can imagine the considerable complexity of a model of human behavior energized not only by self-interest, but also by various degrees of love for others, as well as altruism, patriotism, and religious faith (to name just a few possible distinguishable motivations), with the complexity further increasing with the inclusion of potential changes in the separate motivations and in the intensity of motivations in the context of different groups and in different institutional and physical environments. Such a model would surely be far too unwieldy to handle over time for even the most accomplished mathematicians. The use of mathematics – whether to clarify functional relationships, to find flaws in intuitive logic, or to check the validity of deductions from less precise methods of reasoning – must impose constraints on the behavioral model. To keep the equations sufficiently simple to be manipulated and understood, mathematically grounded analyses must be far simpler than the reality of behavior might suggest they should be. For example, one cost-effective model includes economic actors who are assumed to be fully and exclusively rational, making their choices with complete clairvoyance and precision.²

The growing application of mathematics to economic models during the past century surely has provided gains for economists in any number of ways that need not be identified here. Just as surely, mathematics restricts the assumptions about the rationality of people within economic models, as we will see in the following chapters which review the evolution of the rational/self-interested premise from the works of selected classical to modern neoclassical economists – that is, from Adam Smith all the way to Alfred Marshall and then on to Milton Friedman, George Stigler, and Gary Becker, plus those forebears and contemporaries, who used

¹Graphical analytics ever more tightly constrain the choice of options and the motivations for choice than does mathematics because of the limited dimensions available for graphing.
²The more limited the math skills of theorists, I suggest, the greater the tendency of theorists to sterilize and purify the motivations of economic actors.
some variant of Chicago price theory – Aaron Director, Ronald Coase, Richard Posner, and William Landes who sought to study law with economic methods (Medema 2008). (Indeed, mathematics may have given rise to advancements in economic theory because its use has forced a sterilization of the underlying behavioral assumptions that are far from being descriptively accurate of people’s decision making).

Similarly, econometric techniques are, by their nature, limited in what they can handle in conducting empirical tests. Complicated models of behavior involving decision makers who are less than fully rational or have several and varying motivations across people and time can lead to predictions that are inconsequential to economists of the positive neoclassical tradition because they cannot be tested, or cannot be tested without great, efficiency-impairing costs. As discussed in Chap. 1, Thomas Sargent made this point indirectly when he conceded that actors in rational expectation models know – and must be assumed to know – far more about their circumstances than economists or economic decision makers can know. However, he maintained that macroeconomists in their model building have generally preferred to use rational expectations, which assume fully rational actors, rather than actors whose rationality is bounded because rational expectations offer simplification in model building. Bounded rationality, as conceived by Herbert Simon (1957, 1982), encompasses “satisfying,” not “maximizing,” behavior, being a form of imperfect rationality – but, imperfect only in the sense that people do not have the mental wherewithal to make the kind of flawless decisions implied in perfect rationality (an observation that is at the heart of Chaps. 7 and 8).

Bounded rationality has failed to be widely adopted for model building, Sargent argues, because of “its unfulfilled promise as a device for specifying and understanding out-of-equilibrium dynamics; and its failure thus far to suggest new and fruitful specifications of expectation formation. There has been no rush to use bounded rationality models for guiding macroeconomic empirical work, maybe for the reason that many macroeconomists are in the market for methods for reducing the number of parameters to explain data, and a reduction is not what bounded rationality promises” (1993, pp. 4–5). Surely, Sargent and many other mathematically predisposed economists would have grave difficulty in seeing an assumption of varying bounded rationality across economic actors (which is altogether plausibly realistic, especially once bounded rationality is introduced into economic analytics) as offering the prospect of “reducing the number of parameters to explain data.” Varying bounded rationality across economic actors would surely muck up mathematical models, a point that reveals just how “unreal” bounded-rationality models remain even when touted for their added realism over the neoclassical full rationality premise. To follow on Sargent’s point, the relevant question is again one that Friedman pressed (Friedman 1953): How much is gained in moving from models employing the unreal perfect rationality premise to models employing the unreal bounded rationality premise, at least for the purposes Friedman had in mind?

Again, the techniques of model building and manipulation and hypothesis testing sterilize homo economicus, opening economic analytics to criticisms that
the behavioral foundations of economic models are unreal and seriously, if not fatally, flawed. Such attacks can acquire undue credibility when the discipline’s rationality premise is considered separately from the rest of economists’ scientific enterprise, with model building necessarily dedicated to simplifying the analytics. The critics (many of whom are the behavioral economists and psychologists discussed in Chap. 6) can easily fail to consider the interplay of exact behavioral premises and methodologies employed, or rather that engaging in deductive science requires the sterilization of the rationality premise, at least to some consequential degree, for progress to be made in economics. On the other hand, such criticisms can press economists to minimize the damage by focusing empirical tests on the statistical significance of the variables’ signs and coefficients rather than on the magnitude that the predicted changes in critical independent variables (say, price) may have on the dependent variables (say, quantity demanded).

Theory of Knowledge and Rationality

Many economists, including Friedman, his followers, and his teachers (other than Frank Knight, as we will see in a later chapter) give great weight to knowledge that emerges exclusively from empirical tests that meet scientific standards for statistical rigor. Such predilections on how knowledge is acquired puts demands on the scope and fullness of the presumed rationality of the actors whose behavior is being predicted and tested. Economists, generally, have not recognized that scientific methods profoundly influence the presumptive motivations of actors in economic models. The process of science demands testable hypotheses that are, eventually, subject to empirical tests. But science, as pursued in the physical sciences and in economics, will not allow all people full freedom (or even limited freedom) of choice, nor consider any and all empirical, real-world observations. Stories of real-world happenings and anecdotal evidence, which can be subject to observers’ biases, are generally unacceptable for hypothesis testing within the ranks of those who consider themselves scientists.

Hypotheses testing, generally, demands very exacting standards for the types of data considered, how the data are collected, and how the data are used and manipulated in tests, with all such dictates generally eliminating simple comparisons of total and average values for key variables. Hypothesis testing also requires sophisticated econometric regression techniques to disentangle the effects of various variables introduced into the equations of the underlying behavioral models. Then, to have full credibility, the tests must be repeatable. At every step, the scientific process imposes serious constraints on how human behavior can be modeled. According to Sargent, scientific investigation can become so taxing that practitioners necessarily seek to simplify the process with models that may reduce the parameters used to generate hypotheses and then sterilize the parameters (1993, pp. 4–5).
Paradoxically, economists are bound in their own scientific work to leave the rationality of the actors in their models unbounded. Like the actors in their models, economists do not have a whole lot of choice in the matter once they declare their intentions to do science of the sort that insists on empirical testing of hypotheses. As we will see in following chapters, economists as far back as Adam Smith and as contemporary as Frank Knight, Ludwig von Mises, Friedrich Hayek, and James Buchanan could pursue their economic analyses with full recognition of substantial bounds on their subjects’ rationality, but such economists openly professed to be more engaged in economic philosophy. And they have often been openly hostile to demands that science is the only means to insights about human behavior because of the great difficulties objective measures of the driving force of subjectively conceived wants or ends.

As model building and hypotheses testing have become more formalized during the past two centuries, we should not be surprised that economists have progressively narrowed the range of motivations for choosers in their models and relied more heavily on extreme, or more perfect, forms of rational behavior. As advancing computer and software technologies have reduced the costs of hypotheses testing, more and more economists have moved from philosophical discussions of economic behavior (grounded more or less on the perceived truthfulness of the premises and logical deductions) to empirical tests that meet the exacting standards of statistical validation (relying on narrower views of human motivation, including the as-if perspective on rational/maximizing behavior). Hence, through time, we should expect progressively greater reliance on exacting mathematical model building that presumes more perfect, sterilized forms of behavioral premises, and, not all that surprisingly, growing criticism of neoclassical economics’ behavioral foundations. This criticism may be leveled, especially, at model builders who do not understand the connection between their theory of knowledge and methodologies and their rationality premise and who may have begun to believe that economic actors are as rational as they, the theorists, declare them to be and, in a knee-jerk way, assume their rationality premise to be as descriptive of human motivations as it is useful, for scientific purposes, in modeling human behavior.

Indeed, economists as model builders can open themselves to criticism when they allow the predictive usefulness of their models to determine the descriptive-ness of their behavioral premises. As we will see in Chap. 7, a cottage industry in behavioral economics has arisen that is organized mainly to demonstrate that, by the neoclassical economists’ standard of perfect rationality, irrationalities in decision making and behavior abound (as might have been expected). But then, the behavioralists largely abandon deductive reasoning in favor of allowing experiments to reveal how people, do in actuality, make decisions and behave (an approach that has its own inherent problems, as we will see in Chap. 10).

Of course, a theory of knowledge grounded in empirical tests has to be founded on the presumption that the data selected are actually useful for the intended tests, which requires a presumption that, say, one apple is the equivalent of another apple, which is equivalent to a third apple, which is to say that the addition of three different
apples bought by the same person or different people leads to a meaningful count of three apples that can be used as valid data in regression tests. The presumption that hypotheses testers can meaningfully sum apples (or anything else) to develop data for statistical analysis is crucial to economics as a scientific endeavor, even if fraught with fundamental conceptual problems, which economic luminaries of the past questioned aggressively, as we will see in the following three chapters.

Few economists would agree to add a banana, an apple, and an orange to get a statistically useful sum for scientific purposes. They are transparently three different fruit, and different “goods.” However, the summing of apples could have the same inherent problem, given that different “apples” can also be different goods, used for substantially different purposes, and evaluated by their buyers in radically different ways for radically different ends, all with different quantitative and qualitative values. An apple can be a food item for one buyer, a missile for another, and a decor object for another. Such an observation means that the need for data testing can have feedback effects on what constitutes acceptable premises and on the choice of behavioral and market models, such as the presumption that the market for apples need not be split according to uses, or the apple market is unified, perfectly competitive (or tolerably so) and that people are fully rational and have identical preferences in terms of apples or whatever else is involved in constrained optimization models. The conduct of science requires that the rationality of the actors is directed at goods that are bought, or rather can be counted, whereas in reality the goods (which have inherent variability, as is the case with apples) that are counted for the purposes of science may be only means (or inputs) to higher and varied ends (a satisfying home-cooked meal or apple-bobbing contest at a birthday party) to which the techniques of science cannot be applied because the ends are not knowable or observable, and much less countable.

Friedman, for example, rejected monopolistic or imperfect competitive models, not because he denied their greater realism (he acknowledged that to be the case), but because of that market models’ great realism. According to Friedman, in their efforts to achieve greater “realism” in descriptions of market structures, Edward Chamberlin and Joan Robinson effectively destroyed the conception of an “industry,” apart from having every differentiated product defined as an “industry” unto itself, which undercuts its usefulness as a model to pursue the kind of science, and sense of knowing, that Friedman held dear:

Definition in terms of “close” substitutes or a “substantial” gap in cross-elasticities evades the issue, introduces fuzziness and undefinable terms into the abstract model where they have no place, and serves only to make the theory analytically meaningless – “close” and “substantial” are in the same category as a “small” air pressure....The theory of monopolistic competition offers no tools for the analysis of an industry and so no stopping place between the firm at one extreme and general equilibrium at the other. It is therefore incompetent to contribute to the analysis of a host of important problems: the one extreme is too narrow to be of great interest; the other, too broad to permit meaningful generalizations (Friedman 1953, pp. 38–39).

To study economics using scientific methodology that will yield knowledge of detailed behavioral outcomes in specific market contexts (say, how rent controls
can affect the quantities and qualities of rental units in given geographical areas), rather than simply knowledge of the broad pattern of market outcomes (say, how markets can coordinate disparate human interests and abilities), Friedman, as well as Stigler and Becker, was willing to make the conceptual leap and practical analytical concession that “firms can be treated as if they were perfect competitors” (Friedman 1953, p. 38, emphasis in original). And since Friedman wrote those words, experimental economists have added credibility to Friedman’s position by demonstrating that although two assumed market conditions – (1) the existence, or potential existence, of numerous producers in the market and (2) the pervasiveness of perfect information held by all market participants – can ensure that the efficiency of perfect competition will be achieved, imperfectly competitive markets also may generate almost all of the efficiency gains that, in theory, perfectly competitive ones will achieve, a point supported time and again by the laboratory work of Vernon Smith and other experimental economists and to which we will return later in the book (Smith 1962, 2008a, citing numerous experimental studies).

For similar reasons, Friedman was willing to give an objective reality to goods, a significant concession that allows for the empirical testing needed to judge the presumption that people act as if they are fully rational. Economics as science, in other words, is full of conceptual and epistemological compromises – such has been done with a clearly identified objective function – to deduce testable insights that can lead to improved (not perfect) understandings of the human predicament. In constraining their laboratory experiments and surveys and in defining acceptable results, behavioralists make no fewer compromises that, perhaps, are as important as the compromises Friedman and other neoclassical economists make, except in a different methodological way.

As Chap. 5 will clarify, Austrian economists, such as Ludwig von Mises and Friedrich Hayek, rejected the neoclassical methodology, which demands that economic knowledge can only be validated and acquired through science, or empirical testing, because they could not accept the proposition that the objective physical reality of goods can be meaningfully disentangled from their subjective origins and content. Besides, Austrian economists are more concerned with understanding the implications of human action (as they conceive of the term), which presupposes a sense of individually conceived improvement necessarily independent of any goods that economic actors might seek to buy or sell. The conception of improvement must occur before action is taken, which suggests some newness to every action and which leaves the process of human action beyond the methods of empirical science, according to Mises and Hayek (as will be documented extensively in Chap. 5).

A science such as physics can rely on empirical tests because its subjects (gases, solids, and stars) do have an objective reality and do not have (as far as anyone knows) a capacity to attribute value to other objects in the world around them. The objects of the physical sciences have no presumed means of self-activation in the pursuit of maximizing internally generated utility functions, the end goals of which might be indirectly related to what people do or buy in the marketplace. By contrast, the subjects of economic analysis are people from whom the basic data – preferences, wants, and ends – do not necessarily have an objective reality apart
from their subjective values, which are known only to the people who create them, hold them, change them, and act upon them. As we will see, Austrian economists (as well as Frank Knight and James Buchanan) insist that the internal, subjective nature of economic data limits economists’ ability to do science in the same mold and with the same types of tests as physical sciences. But then, Austrian economists do not need to make the practical concessions, or seek to equate economics’ scientific nature or goals with physical sciences, as Friedman has done (1953, p. 18). Unlike neoclassical economists, those of the Austrian school do not seek to develop the kind of detailed knowledge of outcomes of the interactions of people, say, through markets, but rather seek only to discover the broad patterns of outcomes, with the details within those patterns left undefined or ill-defined, a major point fully documented in Chap. 5.

Hence, Austrian economists do not have to be as careful as neoclassical economists in presuming that people are exclusively rational or only act as if they are fully or perfectly rational. Rather, they have only to presume that people seek to improve their lots (with precise maximization of objective functions unnecessary) through their individual efforts made, perhaps, in cooperation with others, and to show, as Adam Smith and Friedrich Hayek have shown, patterns of outcomes (mainly through market settings) among people and societies.

For example, Smith wrote that people, whatever their level of imperfect rationality, are “led by an invisible hand to make nearly the same distribution of the necessaries of life, which would have been made, had the earth been divided into equal portions among all its inhabitants, and thus without intending it, without knowing it, advance the interest of the society, and afford means to the multiplication of the species,” which suggests that Smith viewed whatever rationality, or maximization, is achieved had to emerge from their interaction with others, given the institutional constraints (Smith 1776, IV.I.10). As we will see in the next chapter, Smith in no way required his subjects to be motivated exclusively by “self-love” that is unbounded by concern for morality and social norms and by concern for others. Smith wove a tale of social improvement – growth in wealth – that actually had three invisible hands at work, one market based, one based on morality, and other based on constitutional governance and the rule of law.

In his Essay on Positive Economics, Friedman does make the useful point that physicists often make calculations about the speed of falling objects on the Earth, using the rate of decent for objects in a vacuum. The assumption about the acceleration of falling objects often works “well enough” because the “extra accuracy it (a more general theory that takes account of air resistance) yields may not justify the extra cost of using it” (Friedman 1953, p. 18). While economics may strive to be like physics in using a simplified premise on rational behavior, it is categorically different when it comes to the objects of the empirical tests. The falling ball does not have a mind of its own that would allow the ball to assess the value of the fall, and even if it did, the ball rate of decent would not be materially affected. In the case of economics, the ball, and what is done with it, depends critically on how people appraise the ball and rate of descent and how their appraisals will cause them to intervene and change the rate of descent or not allow the ball to fall in the first place. There is the added complication in economics that “balls” can be different things to different people, as well as have different evaluations by different people.
Interestingly, Smith’s invisible hands and neoclassical economists’ rationality premise serve a similar methodological and practical function, which is facilitating a narrative about how people interact and how markets work, and when they will not work very well.

Austrians can accept that people are not motivated exclusively by self-interest mainly because Austrians do not stake their professional positions on their mathematical and hypothesis-testing, empirical skills. Neoclassical economists typically assume that supply and demand curves are market constraints that really exist “out there,” or can be treated as if they are out there, mainly because they see them as devices for undertaking comparative statics. By way of contrast, Hayek argued that markets are demand- and supply-revealing processes. If supply and demand emerge as people interact in markets, then it is hardly possible for people to know and order their wants and consistently choose among their wants in some perfect way before or outside the information-revealing process. But then neoclassical economists in Friedman’s camp want to understand the end-result – or more accurately, the directional changes in the equilibrium outcomes (given, for example, an imposed minimum wage) – of market processes while those in Hayek’s camp want to understand the patterns of outcomes in the market process (for example, undirected “spontaneous order” that yields welfare improvements with minimum wages being a contrived intrusion) and not the detailed content of the patterns.

Obviously, different goals require different methods, and different behavioral premises. The behavioralists are simply more interested, as we will see in Chap. 6, in devising “nudges” (changes in decision-making environments) that will lead to personal and societal improvements – as the behavioralists determine that people see “improvement,” a radically different goal from what neoclassical and Austrian economists have in mind. Neoclassical economists, generally, spurn the temptation to divined improvements for others (a position that denies the rationality premise one of its chief methodological defenses, as we will see in Chap. 11).

**Rationality as a Foundation for Process and Institution Analysis**

An analytical perspective grounded in rational/maximizing behavior also can be used to recommend institutions needed to ensure maximum exploitation of unexploited economic opportunities, and to explain how various public policies and private collusive arrangements may prevent market participants from exploiting unexploited opportunities.

Accordingly,

- Since the days of Adam Smith, economists have long made the case against restricted trade of all kinds on the grounds that the restrictions lead to unexploited economic opportunities and denial of potential welfare gains and have made the case for unfettered prices, which can guide people toward equilibrium or maximum societal gains from trade.
• Economists also have argued for the establishment of tolerably well-defined and enforced property rights on the grounds that they can ameliorate, if not eliminate, “tragedies of the commons,” which means that property rights encourage people to create new economic opportunities and to exploit other opportunities that would otherwise be lost in the overuse of resources (Hardin 1968).

• Economists have elevated the importance of “residual claimants” who have the requisite incentives to find and exploit unexploited economic opportunities, and they have stressed how the growing mobility of resources across markets and improved incentive structures within firms can enhance the exploitation of unexploited opportunities.

• Economists have stressed how problems of asymmetric information can lead to unexploited gains from trade, and to lowering the values of certain goods in trades (Akerlof 1970; Spense 1973; Rothchild and Stiglitz 1976). For example, new cars lose a significant portion of their selling price as soon as they are driven off dealer lots, not from wear-and-tear but from the public’s presumption that cars on used-car markets are heavily weighted toward “lemons,” those with not-so-easy-to-detect mechanical problems (Akerlof 1970). But then economists, such as John Lott (2007), have explained that the so-called “lemon problem” has been greatly exaggerated because with time people will seek to exploit the unexploited profitable opportunities in any persistence of the lemon problem. The relevance of these rationality postulates depends to a significant extent on market participants being able to operate within an institutional setting that allows and encourages – even presses – exploitation of unexploited economic opportunities. The rationality postulates would have been of much less (perhaps little) use in understanding much economic activity within the former Soviet Union because for six-plus decades choices were fettered and prices were uninformative of underlying economic realities. People were not allowed (for the most part) to use their localized information to find and exploit opportunities and to change the societal pattern of outcomes not dictated by the Soviet planning authorities.

Although the issue of people’s level of rationality is a lesser concern to economists seeking to do science, it is crucial to economists who seek to design institutional/constitutional settings within which people are encouraged to find and exploit unexploited economic opportunities. If people have no (or very limited) rational capacity to search for and exploit unexploited economic opportunities (as followers of B.F. Skinner believed and all too often behave much like “goslings,” as one behavioral economist has suggested), then institutional/constitutional design is irrelevant and a waste. For that matter, all science and economics is irrelevant and of no consequence. The critical issue, as Friedman recognized explicitly, is whether or not people’s likely imperfect rational capacities are “good enough” on which to found predictions regarding the directional changes in behavior, given market

4Experimental researchers have found that market institutions do influence the extent to which people are rational, exploit unexploited opportunities, and achieve the known potential market efficiency gains (V. Smith 2008).
pressures to correct errors and given changes in constraints. Neoclassical econo-
mists seem to have great confidence, derived from experience with employing
rationality-based models, that the rationality-based models pass scientific muster.
The success of neoclassical economics in applying their methods to a variety of
novel areas of human behavior – from human capital to information to law and
economics to public decision making – has been substantial over the past half
century. The rationality premise might be flawed, but still useful.

Concluding Comments

Rational behavior is an explicit premise on which economists found their science;
however, another implicit premise is that objective data are available and can be
used to test theories that are grounded exclusively in subjective evaluations. These
premises may not always be valid. Most people, including Friedman, agree that the
premise of full rationality may not at times be a “sufficiently good” approximation
of people’s motivations. The same can be said for the data premise. Economics is
not like a physical science that starts with the presumption that objective data has no
subjective origin. Such concerns ultimately mean that economics is often as much
art as science, founded on reasonable “fits” among people’s constellation of moti-
vations, the rationality premise, perception, and evaluation of “goods” and the
particular data series used to represent goods.

Throughout this book, I can be more accommodating of multiple motivations
than working neoclassical economists because here I do not seek to do science and
generate testable hypotheses. Rather, this book seeks to understand both the advan-
tages and limitations of economics as a science, and why economists might be
attracted to the premise of rational behavior to understand the world around them,
in spite of its descriptive flaws.

As it is argued in Chaps. 7 and 8, discussions of rationality are often focused on
the appropriate choices of methodologies, as if peoples’ and economists’ choices
over such matters are unconstrained. Actually, as will be seen, evolutionary forces
of long ago have seriously constrained our methodological choices for understand-
ing human behavior as well as the alternatives that we consider viable choices. The
human brain now may very well be structured so that we (individual actors in
market and nonmarket settings and economists who study people in such settings)
seek to find some semblance of order amid the pervasive apparent disorder or
nonorder, if not chaos, of everyday life. Through evolution, human beings may
have developed the presumption of rational/maximizing behavior as one method to
ferret out a sense of order from all the behavioral noise we observe, with the
methods having fitness and survival value. After all, we can detect ourselves
often making the kinds of cost–benefit/maximizing decisions that the actors in
economic models are assumed to make.

In earlier evolutionary epochs, people did not have computers or the statistical
and laboratory methods by which to undertake meaningful inductive, scientific
research to determine how and why people do, what they do, and nor did they have the time to undertake such studies. Deductive, rationality-based reasoning was then far more cost-effective, and rationality may have developed as an effective heuristic by which people could develop a sense of order to behavior.

There is something of a quasi-scientific hypothesis that has emerged in our discussion of the methodological constraints on the rationality premise: Any evolution in economic methodology toward formalization, as revealed in the progressively greater use of mathematics and graphical devices, can be expected to be accompanied by a narrowing and growing sterilization of the presumed underlying human motivation toward, if not to, a premise of full or perfect rationality across all analytical subjects. We can also expect (with some understandable delay) growing criticisms from inside and outside the economics discipline over how a variety of human behaviors observed in laboratory experiments and surveys diverge in stark ways from what would be predicted from economists’ assumed full or perfect rationality. I describe my hypothesis as quasi-scientific because while I do seek to test the hypothesis, I do not use strictly scientific methods to do so. That is, I do not try to devise numerical counts to be used in regression equations.

Instead, in the next three chapters, through a review of the ways in which leading economists have conceived of buyers’ and sellers’ behavioral motivations, I examine how the concept of rational behavior has evolved throughout the intellectual history of the discipline. This review tends to support my methodological hypothesis. Early economists, including Adam Smith, who did not use formalized methods of argument – that is, graphs and equations – generally, took an expansive view of their subjects’ motivations. Even Alfred Marshall, who introduced graphical analysis into economics, followed Smith in assuming that people were activated by motivations other than self-interest, but then Marshall saw graphs and mathematics as devices for checking the logical flow of key points, not a substitute for broadly conceived logical arguments constructed in words. Similarly, Frank Knight, Ludwig von Mises, Friedrich Hayek, and James Buchanan openly acknowledge that the economic view of behavior founded on rationality and self-interest is only a partial view, but then, again, they largely spurn the use of mathematics and lament the growing focus of the profession on mathematical methods founded to a growing degree on full rationality.

In Chaps. 6 through 8, I will take up burgeoning major strands of criticisms of rational behavior from inside and outside the economics profession. Along the way, I will show how an assumption of full or perfect rationality, as a presumed

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Footnote: As we will see, critics of standard economics have made an academic sport of revealing human decision frailties and “irrationalities,” all with a growing fan base within disciplines outside of economics and within the educated public (as evident by the growth in the count and sales of trade books describing economic “anomalies” and “irrationalities” (consider, for example, books by Thaler and Sunstein, *Nudge*, 2008 and by Ariely, *Predictably Irrational*, 2008, both cited in Chaps. 6 and 10 on behavioral economics). In key ways, the research agendas of behavioral economists and others have all the markings of shooting fish in a barrel.
description of human motivations, does not make sense from the perspectives of evolutionary biology and neurobiology nor does it make economic sense.

Nevertheless, my ultimate intent, somewhat paradoxically, in undertaking these critical reviews is the development of defenses for full rational behavior’s continued use in economic analysis, despite acknowledged logical and empirical problems. Those defenses, however, will be accompanied by a plea for economists to remember the intellectual roots and function of the full rationality premise. I seek a return of professional thinking to the view, held by many of the discipline’s luminaries covered in chapters to come, that the economic way of thinking provides only a partial view of the behavioral forces that motivate and activate life as modern human beings are capable of experiencing it.
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