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THE FOUNDATIONS OF BEHAVIORAL ECONOMIC ANALYSIS

Volume IV: Behavioral Game Theory

SANJIT DHAMI

The Foundations of Behavioral Economic Analysis: Volume 4

The Foundations of Behavioral Economic Analysis is also available in seven newly revised volumes published by Oxford University Press

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George Loewenstein, Herbert A. Simon Professor of Economics and Psychology, Carnegie Mellon University.

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Andrew Schotter, Professor of Economics, New York University and Director, Center for Experimental Social Science.

"For someone, like myself, who started by being ignorant of the richness of the conversation within behavioral economics on a variety of issues, this magisterial volume is the ideal introduction, at once lucid and sophisticated."

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Peter Wakker, Professor of Economics, Erasmus University Rotterdam.

"Sanjit Dhami's *The Foundations of Behavioral Economic Analysis* is a major and most impressive achievement. It provides an exhaustive account and a masterful synthesis of the state of the art after more than three decades of behavioral economics. It has proven to be an indispensable reference for researchers in economics and psychology. The second, updated edition comes in seven volumes, and it is bound to become the standard text in graduate and advanced undergraduate courses on behavioral and experimental economics for many years to come."

Klaus M. Schmidt, Professor of Economics, University of Munich.

“This is the most complete and stimulating series of books on behavioral economics. With elegance and unprecedented elaborateness, it ties together a wealth of experimental findings, rigorous theoretical insights and exciting applications across all relevant fields of behavioral research. Sanjit Dhami’s work has been shaped by numerous comments of the leaders in the field. Now, in the years to come, it will be the standard that shapes how the next generation of students and researchers think about behavior and its science.”

Axel Ockenfels, University of Cologne, Speaker of the Cologne Excellence Center of Social and Economic Behavior.

“The expansion of behavioral economics during the past quarter century has been remarkable, much of it concerning strategic interaction and using tools from game theory. Sanjit Dhami’s amazing book, now available in a convenient multi-volume format, summarizes—and even defines—the field, broadly as well as in depth. His coverage of theory as well as of experiments is superb. *The Foundations of Behavioral Economic Analysis* will be an indispensable resource for students and scholars who wish to understand where the action is.”

Martin Dufwenberg, Karl & Stevie Eller Professor and Director of the Institute for Behavioral Economics at the University of Arizona.

“*The Foundations of Behavioral Economic Analysis* will be a central textbook for behavioral economics. One key feature is its appealing focus on the interplay between theory and evidence. For researchers, it will be a great source of information, puzzles, and challenges for the many years to come. It is a major achievement.”

Xavier Gabaix, Pershing Square Professor of Economics and Finance, Harvard University.

“This is a unique and truly remarkable achievement. It is a magnificent overview of behavioral economics, by far the best there is, and it should define the field for at least a generation. But it is much more than that. It is also a brilliant set of original discussions, with pathbreaking thinking on every important topic. An invaluable resource for policymakers, students, and professors—and if they want to try something really special, for everyone else.”

Cass Sunstein, coauthor of Nudge and Founder and Director of the Program on Behavioral Economics and Public Policy, Harvard Law School.

“This is truly an amazing work. It is unique in both comprehensiveness and depth. The author is to be applauded for producing what will surely be the standard reference for both researchers and students. And breaking it into seven volumes will greatly enhance its usability. I highly recommend these volumes to any serious reader in behavioral economics.”

Gary Charness, Professor of Economics, University of California, Santa Barbara.

The Foundations of Behavioral Economic Analysis: Volume 4

Behavioral Game Theory

Sanjit Dhami

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To my Parents, wife Shammi, and son Sahaj

PREFACE TO VOLUME 4: BEHAVIORAL GAME THEORY

The Foundations of Behavioral Economic Analysis (henceforth, FBEA) was published by Oxford University Press in November 2016. It was the culmination of more than a decade of dedicated work. The book was quite well received and it was heartening to receive messages of support, encouragement, and appreciation from many quarters. Several reviews of FBEA have been published and they have praised the comprehensiveness, formal analysis, and the attention to empirical detail in the book. The book is increasingly taught around the world in behavioral and experimental economics courses in the leading economics departments. Encouragingly, it is also being used in more enlightened courses in economic theory, which was always an important objective of writing this book. The practice of ignoring the empirical evidence and the theoretical models in behavioral economics, in many courses in microeconomics, game theory, and contract theory, is one of the most retrogressive practices in the profession and a form of self-handicapping that is difficult to understand.

At 1,796 pages (including unnumbered pages), FBEA is probably one of the longest economics books ever to have been published in a single volume. Binding the book was a major challenge, which Oxford University Press accomplished with great competence. Some friends have written on a lighter note about the physical size and the weight of the book. Samuel Bowles wrote to say that Herbert Gintis had presented him with a copy of the book on Christmas and that he had to hire a truck to take it home. In one of his reviews, Daniel Read congratulated me on writing the "War and Peace" of behavioral economics. Andrew Schotter wrote to say that he keeps one copy at home and another in his office in NYU to avoid carrying it on the New York subway. A friend who had purchased the paperback version took the drastic step of physically separating Part 4 on behavioral game theory (a good 320 pages long) to carry around with him. Xavier Gabaix is one of many readers who prefers the electronic version that makes issues of the size of the book irrelevant. However, at least some readers, and I am part of this group, tend to be old fashioned and prefer the printed version.

We did explore the idea of splitting FBEA into two volumes before it was published and this was put to an informal vote among 30 of the leading behavioral economists. They were almost equally split. OUP took the casting vote to decide on a single volume, understandably because there are not too many multiple volume mainstream texts in economics. As more feedback from the users of the book emerged, Adam Swallow, the commissioning editor at OUP, began exploring with me the possibility of splitting the book into multiple volumes. Just as publishing such a long book and making it available for teaching to several instructors prior to its publication was a novel and bold experiment in publishing, so too is the proposal to split it into multiple volumes. After extensive discussions at OUP, I was given the go ahead to pursue this exciting and unprecedented opportunity.

What we present to you here, after considerable thought, is a seven-volume book on behavioral economics that splits the nine parts of FBEA into the following topics: Behavioral economics of risk uncertainty and ambiguity (Volume 1); Other-regarding preferences (Volume 2); Behavioral economics of time discounting (Volume 3); Behavioral game theory (Volume 4); Bounded rationality (Volume 5); Behavioral models of learning (Volume 6); Further topics in behavioral economics that include emotions, behavioral welfare economics, and neuroeconomics (Volume 7). Other possible splits of FBEA were possible (e.g., combining Volumes 1 and 3; and Volumes 2 and 4), but none of these proposals offers the clean separation into the main topics in behavioral economics that the current split offers.

We believe that these seven volumes improve on FBEA for several reasons aside from just better portability of the print edition. First, it is a welcome opportunity to correct several typos and errors, as well as to improve the clarity of the text in many places. Second, it allows the updating of some of the material to reflect important recent scholarship in the form of a "guide to further reading" at the end of each volume. This allows me to introduce several new concepts and tie them back to the discussion in the main text. Third, it gives readers the option to buy individual volumes, depending on their current research and teaching interests. However, those with a serious interest in economics, certainly all university academics, ought to consider reading all of the seven volumes. Fourth, given how daunting the prospect of revising the 1,800-page FBEA would have been, the split volumes increase the likelihood of a second edition to some, or all, of the volumes in due course.

For the benefit of readers who buy the separate volumes, or just a few of the volumes, we have taken several steps. Each of the volumes will have a new preface, a new introduction, and carry a reprint of the original preface in FBEA. This will give readers an opportunity to get acquainted with how and why this book came to be. The introductory chapter in FBEA covered important ground. In particular, the first 25 pages outlined the antecedents of behavioral economics, the role of scientific methodology, and the rationale for the experimental method. A lack of proper understanding and appreciation of these critical prerequisites may seriously hamper an understanding of the subject matter. For this reason, in each volume, we shall also print an edited version of the first 25 pages in FBEA. In these pages, I have also added a brief new subsection on replication of experiments. The remaining part of the introductory chapter in FBEA (pages 25–64) is printed only in Volume 1. I have taken care to remove as many typos and errors from the introduction of FBEA as I could find, and improved the clarity of the material in many places.

Readers will find that we have done many of the same things that we might have done in bringing out a second edition of FBEA in these seven volumes. We hope that our efforts in this direction will lead to a better understanding and appreciation of the subject matter of behavioral economics.

PREFACE TO THE FOUNDATIONS OF BEHAVIORAL ECONOMIC ANALYSIS

We print below the original preface to *The Foundations of Behavioral Economic Analysis* in Dhami (2016).

Neoclassical economics is a logically consistent and parsimonious framework of analysis that is based on a relatively small set of core assumptions, and it offers clear, testable, predictions. However, extensive and growing empirical evidence reveals human behavior that is difficult to reconcile within the typical neoclassical models. There has been a parallel development in rigorous theoretical models that explains better the emerging stylized facts on human behavior. These models have borrowed insights from psychology, sociology, anthropology, neuroscience, and evolutionary biology. Yet, these models maintain a distinct economic identity in terms of their approach, rigor, and parsimony. Collectively, these models form the subject matter of behavioral economics, which is possibly the fastest growing and most promising area in economics.

This book is an account of behavioral economics that starts with the basics and takes the reader to the research frontiers in the subject. Depending on how one chooses to use it, the book is suitable for courses at the advanced undergraduate, postgraduate, and research level in economics, and the related social sciences, including, but not restricted to, psychology, management, finance, political science, and sociology. The book should also serve as an essential reference book for anyone generally interested in behavioral economics at any level, and also serve to stimulate the interests of non-specialist academics, specialist academics who are looking for a bird's-eye view of the entire field, and policymakers looking for policy applications of behavioral economics. It would be desirable to assign this book as background reading to courses in economic theory. The book is also, in my view, the minimum subject matter that anyone who writes behavioral economics as their research interest, should be deeply familiar with.

In November 2003, two months after I joined the department of economics at the University of Leicester, I chanced upon an invitation to attend a talk by a colleague, Ali al-Nowaihi, on the subject of *prospect theory*. Ali, a mathematician by training, an economist by profession, and a keen student of the philosophy of science, put forward a Popperian view to evaluate economic theories. He argued that *expected utility theory* was decisively rejected by the evidence, and prospect theory was the most satisfactory decision theory currently available. As a purely neoclassically trained economist, I was troubled by the claims, but also extremely skeptical. For a start, prospect theory sounded like a strange name for a theory, and the evidence was largely "experimental," a data source, that I knew little about. As my defensive instincts started to kick in, I wondered if prospect theory really was so important, then surely my graduate courses, many taught by leading decision theorists, would have found some reason to mention it. Nor was there any mention of such a theory in conversations with colleagues at the two British universities where I had taught so far, or at seminars or conferences that I had attended.

However, rather than just dismiss Ali, a very likeable and respected figure in the department, I decided to put his seemingly extreme views to the test. One of my majors was in public economics, so I decided to conduct a prospect theory analysis of tax evasion in the hope of explaining the *tax evasion puzzles*, which had been outstanding for three decades (details in Part 1). There was already some preliminary work in this area that Ali had mentioned in passing, but none of the papers explained all the puzzles in one fell swoop, using all components of prospect theory. It took me just a few weeks to work out the results. To my utter amazement, prospect theory explained the qualitative and quantitative tax evasion puzzles. By contrast, the predictions based on an expected utility analysis were wrong by a factor of up to 100. This led to my first joint publication with Ali, with whom I have spent many years of fruitful collaboration since then.

This initial, and successful, encounter with prospect theory convinced me that I needed to explore behavioral economics in greater depth. Yet, around 2004, there was no definitive graduate text on behavioral economics. To be sure, there were many excellent sets of collected readings, and several insightful surveys and commentaries on selected aspects of behavioral economics that I eagerly read. In particular, while there were many excellent discussions of the experimental evidence, a full treatment of behavioral economic theory and its applications was missing. One could always pursue the journal articles, but the literature was already enormous, rapidly expanding, and scattered, which made it difficult to spot the links between the various models or to clearly visualize how the various pieces of the jigsaw fitted together. This book was motivated initially by the lack of a serious graduate book on the entire subject matter of behavioral economics, my desire to master behavioral economics, and to support my growing research agenda with Ali. In due course, and as the full range of the subject matter gradually dawned upon me, the scope of the book naturally became more ambitious and daring.

I strive to strike a balance between behavioral economic theory, the experimental evidence, and applications of behavioral economics. The choice of theoretical models in this book is dictated, first and foremost, by their ability to explain the empirical evidence. In some cases, where no decisive empirical evidence is available, I make a judgment on which models are more promising than others, although I give a wide berth to most models.

The main prerequisite for the book is training in the first two to three years of a reasonably good British or North American undergraduate degree in economics, or its equivalent. Any further concepts and techniques are introduced in the book, where needed. A prior course in behavioral economics is not a prerequisite for the book.

The book is divided into nine parts that cover decision making under risk, uncertainty, and ambiguity; other-regarding preferences; behavioral time discounting; models of behavioral game theory and learning; role of emotions in decision making; models of bounded rationality; judgment heuristics and mental accounting; behavioral welfare economics; and neuroeconomics. The book also considers a range of applications of the theory to most areas in economics that include microeconomics, contract theory, macroeconomics, industrial organization, labor economics, development economics, public economics, political economy, and finance. A set of exercises at the end of each part, except the part on neuroeconomics, serves to enhance the reader's understanding of the subject.

Behavioral economics is now a mainstream area in economics. One just has to look at the growing and large number of journal publications and Ph.D. theses every year; the Nobel Prizes to Herbert Simon, Daniel Kahneman, Robert Shiller, Alvin Roth, Vernon Smith, and George Akerlof; the John Bates Clarke medal to Matthew Rabin; the growing importance of behavioral economics among policymakers, as witnessed by the 2015 World Bank Development Report, and the formation of the behavioral insights team in the UK; and the choice of Richard Thaler as the incoming President of the American Economic Association.

It is fair to say that no self-respecting economics department can now afford to omit a course in behavioral economics from its undergraduate or graduate curriculum; indeed, doing so would be grossly unjust to its students and a retrogressive step. Nor can any academic economist, who wishes to retain professional honesty and a balanced opinion on the subject, afford to be unfamiliar with the subject matter of behavioral economics; I am often amused by the ignorance and arrogance of many who pass judgment on behavioral economics with supreme confidence, yet appear to have little understanding of it.

This book has taken more than ten years to write, and my debts are deep and profound. My first and foremost debt and gratitude is to my loving family without which this book could not have been written. To my parents, Manohar and Baljeet, for their unconditional lifelong love and support, and instilling in me the core values of honesty, commitment, and hard work. To my wife, Shammi, and my son, Sahaj, for their patience, sacrifice, unflinching support, and constant encouragement. When I started writing this book, Sahaj was in primary school, and in the month of its first publication, he could be packing his bags to join a university. I do not recommend this as the best template to encourage your son to write any books in the future. However, there are close parallels between Sahaj's educational journey from primary school to university, with my own journey in behavioral economics.

I owe a deep intellectual debt to my long-time coauthor and friend, Ali al-Nowaihi. I first learnt about prospect theory from him. I also owe my appreciation of methodology and the philosophy of science entirely to him. He has undertaken a larger burden of our joint research in the last few years, allowing me to be immersed in the book. For all these reasons, he is very much a coauthor of the book in spirit.

I am extremely grateful to many academics and Ph.D. students who unselfishly and generously contributed their time and efforts to reading drafts of various parts of the book. The participation of so many leading behavioral economists in the making of this book is unprecedented and has really made it into a public project for which I shall always be very grateful. Herbert Gintis, Martin Dufwenberg, and Vincent Crawford deserve special mention for being so very gracious with their inputs into most parts of the book, and very quickly responding to my queries.

Many others also played a critical role in the writing of this book and commented on material closer to their areas of interest, and/or offered valuable encouragement and advice. In particular, I wish to thank Mohammed Abdellaoui, Ali al-Nowaihi, Dan Ariely, Douglas Barrett, Björn Bartling, Karna Basu, Kaushik Basu, Pierpaolo Battigalli, Roland Bénabou, Florian Biermann, Gary Bolton, Subir Bose, David Colander, Andrew Colman, Patricio Dalton, Alexandra Dias, Florian Englmaier, Armin Falk, Ernst Fehr, Urs Fischbacher, Xavier Gabaix, Sayantan Ghosal, Uri Gneezy, Werner Güth, Shaun Hargreaves Heap, Fabian Herweg, Karla Hoff, Philippe Jehiel, David Laibson, George Loewenstein, Michel Marechal, Friederike Mengel, Joshua Miller, Axel Ockenfels, Amnon Rapoport, Ludovic Renou, Alvin Roth, Klaus Schmidt, Andrei Shleifer, Dennis Snower, Joe Stiglitz, Cass Sunstein, Richard Thaler, Jean-Robert Tyran, Klaus Waelde, Peter Wakker, Eyal Winter, and Peyton Young. I owe a profound intellectual debt to many others who did not read the book manuscript but whose work has greatly inspired me. These include Daniel Kahneman, Amos Tversky, Colin Camerer, Matthew Rabin, Herbert Simon, Robert Shiller, and George Akerlof. I am also very grateful to two successive Heads of the economics department at Leicester, Steve Hall and Chris Wallace, who tried to free up as much of my time as possible for writing the book.

I would like to specially acknowledge the enormous amount of work put in by two extremely conscientious and able Ph.D. students, Teimuraz Gogsadze and Junaid Arshad. They closely read and commented on successive drafts of the manuscript at all stages, offered very useful advice, and served as excellent sounding boards for new ideas. Jingyi Mao came up with a very nice cover for the book in a burst of creativity, for which I am very grateful. Other Ph.D. students who carefully read and commented on selected parts of the manuscript include: Ala Avoyan, Nino Dognohadze, Sneha Gaddam, Narges Hajimoladarvish, Emma Manifold, Jingyi Mao, Alexandros Rigos, David Tsirekidze, Yongli Wang, Mengxing Wei, and Mariam Zaldastanishvili.

I would be remiss not to thank the large number of other researchers whose work has made this book possible. I must also sincerely apologize to authors who feel that their work has been inadequately cited or not given the importance they feel that it deserves. To such authors, I say, omission of your papers does not mean that I necessarily viewed your papers as unimportant. In mitigation, I do not intend my book to be a survey of all the experimental results on all topics in behavioral economics; there are already excellent sources with this objective. And, quite possibly, I was simply unaware of your important work, which is in keeping with the evidence on limited attention and bounded rationality that plays an important role in this book.

I am very grateful to the team at Oxford University Press who have done an excellent job at all stages of this book. In particular, I would like to thank Adam Swallow, the commissioning editor for economics and finance at OUP for his patience, good cheer, organizational skills, and sound advice. Scott Parris, the economics editor at the US office of OUP, who retired just as this book was about to come out, was the first to spot the importance of this project. He offered very valuable advice and encouragement throughout the writing stage and played a key role in my decision to go with OUP. I must also thank Niko Pfund, the President of Oxford University Press USA, for his continued interest in the manuscript over several years, despite his many other responsibilities. The production and marketing teams at OUP were a pleasure to work with. Jon Billam took on the challenge of copy-editing an unusually large book with great enthusiasm. I am also very grateful to Emma Slaughter, the production editor for the book; Kim Stringer, the indexer; Kim Allen, the proofreader; Carla Hodge-Degler who took over as production editor from Emma; and to Leigh-Ann Bard, the marketing manager for the book.

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Introduction to Volume 4

In Volume 1 we studied the behavior of humans when they engage in non-strategic interaction against nature (game against nature). By contrast, in Volume 4 of the book, we consider strategic interaction among individuals and firms, although nature can be of the possible players. In these problems, moves by nature, the choices of the players, and their beliefs interact to give rise to individual payoffs. It is assumed that readers are familiar, at a bare minimum, with the material in the appendix on game theory.

Classical game theory plays an important role in economics. It has forced economists to think carefully and formally about the players in their models, their strategies, the sequence of moves and the payoffs. It has developed plausible equilibrium concepts in different strategic situations. This has allowed it to make precise, testable predictions. It is probably fair to say that the basic machinery of classical game theory underpins most results in modern economics in a mathematically elegant and parsimonious manner.

Volume 4 is split into three chapters. Chapter 1 considers the evidence on human choices in strategic situations. The main emphasis is on experimental and field evidence that is designed to test the predictions of equilibrium concepts in classical game theory. Chapter 2 considers models of behavioral game theory that are motivated by the evidence in Chapter 1. We also consider the relevant evidence on the behavioral models. These models range from those that are relatively well established and backed by reasonable amount of evidence to others that are promising, yet need further testing to enable us to make a more informed choice. Chapter 3 considers a guide to further reading and to the more recent literature.

Chapter 1 is divided into several sections. In general, human behavior in the early rounds of games and in unfamiliar games, is often not consistent with classical game theory. Humans engage in many important decisions for only a limited number of times. For instance, the choice of a university degree, the choice of a marriage partner, the choice of a house and other consumer durables, and the choice of a pension plan. Many decisions made by firms are also of an infrequent nature. These include capital restructuring, mergers, sunk costs in major machinery and equipment, and choice of a new product. Even in decisions that are taken frequently, the real environment is ever-changing and uncertain, bringing about an element of novelty in most decisions. Thus, arguably the choices made in the early rounds of an experiment are critical in understanding strategic human behavior in many important domains.

It typically takes a substantial amount of experience and repetitions of the game for play to converge towards the predictions of classical game theory. However, this is often not the case in many environments (see also Volume 6), and in many cases, the evidence is equally well explained by behavioral models. It is not clear how one might stringently test models of classical game theory in the field. Testing such models in the lab does provide a stringent test. However, an important caveat of lab experiments, in which incidentally the predictions of a Nash equilibrium often fail, is that the lab environment is often too stationary. By contrast, the real world environment is typically characterized by persistent randomness, where non-equilibrium behavior is the norm. Classical game theory is ill-suited for this environment because it takes an equilibrium approach. We defer a discussion of these interesting issues to Volume 6 where we discuss stochastic social dynamics, complexity, and agent-based modeling (see Chapter 3 and the guide to further reading in Volume 6).

We begin in Section 1.2 by considering the evidence from the simplest games in which a unique Nash equilibrium obtains by *iteratively deleting dominated strategies* in a small number of steps. Yet, the outcome in most experiments differs from a Nash equilibrium if the number of steps is more than two or three.

In a fundamental result, Nash (1950a) showed that every finite game has at least one *mixed strategy Nash equilibrium* (MSE); Section 1.3 shows that the evidence does not support a MSE. In the majority of experiments, individual play does not conform to the predictions of a MSE, although the aggregate play is closer to a MSE. However, a fair number of experiments also reject MSE both at the individual and the aggregate levels. Individuals do play mixed strategies, but not in the proportions that are predicted by a MSE. There is too much serial correlation in strategies in play over successive rounds. Even when players are given a randomizing device, their play does not conform to a MSE. There is evidence from professional sport that tennis and soccer players may choose a MSE. However, the suitability of this evidence that relies on motor skills of individuals, for solving economic problems, is debatable. Furthermore, this evidence is from essentially dynamic problems; serial correlation in the evidence does not support a MSE; and the ability to play MSE in sports is not portable to other contexts.

In Section 1.4, we consider coordination games that have *multiple equilibria* and the aim is to see if players can coordinate on any of the equilibria. We examine the role of *focal points* and the conflict between focal points and *payoff-salience*. None of the existing selection principles such as *payoff dominance* or *risk dominance* can fully account for the data. Newer, behavioral selection principles, based on loss aversion may be promising. The chosen equilibria are also strongly *history dependent*. *Outside options*, *forward induction*, and *preplay communication* influence the degree of coordination, even in cases where classical game theory predicts that there should be no such effect.

Bargaining games are considered in Section 1.5. We consider the neoclassical theoretical framework and the relevant empirical evidence for *normative* and *positive sequential alternating offers models of bargaining*. We also consider models that do not fit into any of these categories, such as models of *self-serving bias* and *focal points*. The support for classical game theory under full information is relatively weak, and at best mixed under *one-sided asymmetric information*. However, under *two-sided asymmetric information*, which requires very high cognitive demands from the players, we find surprising support for classical game theory under *sealed bid mechanisms*. We also consider evidence from experiments that attempt to discover the *cognitive process* underlying choices in games using MOUSELAB experiments. Unless subjects are trained in the relevant game theoretic concepts, their actual pattern of *searches* and *lookups* is not consistent with the expected pattern in classical game theory.

Section 1.6 considers evidence from *signaling games*. In several experiments, while play does not conform to the predictions of classical game theory in the early rounds of play, there is greater conformity in later rounds. But, typically, a significant fraction of the players still do not play in accordance with the classical game theoretic predictions. A much replicated finding is that there is no support for refinements beyond the Cho–Kreps *intuitive criterion*.

Section 1.7 considers the experimental evidence on *correlated equilibria* in which a choreographer gives recommendations to the players to play particular strategies. In equilibrium, each player finds it in his interest to follow the recommendations, if others do so. When no preplay communication is allowed, then experimental results show that the recommendations are not followed. This is reversed in the presence of non-binding preplay communication. The main result is that a correlated equilibrium is a necessary, but not a sufficient condition for the recommendations to be followed.

Section 1.8 considers experimental evidence from games in which the actions of players are either *strategic complements* (SC) or *strategic substitutes* (SS). The evidence suggests that the actions of players in both cases is positively correlated. This is suggestive of reciprocity considerations among players. We also consider the implications of SC and SS for macroeconomics. In the presence of such considerations, nominal rigidity can be explained by behavioral factors such as *money illusion* and *anchoring*.

Section 1.9 considers empirical evidence for a Walrasian competitive equilibrium, where strategic considerations, in the limit, vanish. The main message arising from the empirical literature is that there exists an institution (double auction) and a particular rule for changing prices (the market improvement rule) that can implement a competitive equilibrium in relatively few rounds of play, even with a small number of players.

The evidence on strategic choices in Chapter 1 has motivated the development of alternative theoretical models, which are often in better accord with the empirical evidence as compared to classical game theory. Chapter 2 turns to a consideration of these models. We now outline the plan for this chapter.

A *perfect Bayesian Nash equilibrium* (PBNE) requires two conditions. First, given any history of the game, each player chooses his best reply, conditional on his beliefs at that stage of the game, and the subsequent equilibrium strategies of the players. Second, beliefs are formed using Bayes' rule, wherever possible, and actions and beliefs are consistent with each other. The models considered in Chapter 2 relax these conditions individually or jointly.

Section 2.2 considers the *quantal response equilibrium* (QRE). This relaxes the first condition of a PBNE. In a QRE players play *noisy best replies*, in the sense that all actions are played with a strictly positive probability. However, conditional on the beliefs, the likelihood of playing actions that give a higher expected payoff is also greater. A QRE requires the extremely cognitively demanding condition that players have the correct beliefs about the noisy play of others. An equilibrium requires the consistency of such beliefs with the actions of players, so the second condition of a PBNE holds. A QRE is able to organize much better the evidence from a range of games as compared to a Nash equilibrium but not all readers will be persuaded by the psychological foundations for a QRE.

Section 2.3 relaxes the second feature of a PBNE but keeps the first. Arguably this leads to a less cognitively challenging model. Level-*k models*, and the *cognitive hierarchy models* (CH), are the leading behavioral models in this category. In these models, there is a hierarchy of types or levels among the players. Each higher level is more cognitively accomplished, relative to the lower levels. The two models differ in the distributional assumptions on the lower levels. In level-k models a player of level $k = 1, 2, \dots$ believes that the opponent player is of level $k - 1$, while in CH models, such a player believes that there is a non-degenerate distribution of opponents of lower levels $k - 1, k - 2, \dots$. Such models are able to make precise predictions in a large variety of games that are consistent with the evidence. In many of these games, a Nash equilibrium is unable to account for the data; perhaps the *p-beauty contest* is the most popular example. The psychological foundations for such models are stronger than the QRE model.

Section 2.4 considers several applications of level-*k* and CH models with particular emphasis on coordination games. The *market entry game*, considered in this section was traditionally held up as a vindication of classical game theory because most experimental evidence was consistent with it. Daniel Kahneman described these results as *magical*. However, it turns out that level-*k* and CH models are able to account for the evidence equally well.

In classical game theory, players do not derive utility directly from their beliefs about the actions of others (*first order beliefs*) or their beliefs about the first order beliefs of others (*second order beliefs*), or from such higher order beliefs. Yet, human morality and a range of emotions, such as guilt, and notions of justice, such as perceived fairness, seem to impart direct utility, based on these beliefs. Since such beliefs may be endogenous, one cannot simply append the psychological payoffs to the material payoffs and use the framework of classical game theory. Rather, this requires new concepts and machinery that is embodied in *psychological game theory*. We consider the relevant equilibrium concepts for both normal form and extensive form games in Section 2.5. We also consider applications to an explicit modeling of the role of *reciprocity* (see Volume 2 of the book for the evidence), and to the role of *guilt* in a range of games.

Section 2.6 is devoted to some of the central ideas in Gintis (2009) that are sufficiently important to merit a section in their own right. He argues that there are no plausible *epistemic foundations* that imply *common knowledge* of rationality in classical game theory. Gintis also ascribes to *social norms* and *conventions* the role of a *choreographer* in a *correlated equilibrium* that coordinates human behavior. He argues that it is inappropriate to interpret Nash equilibria as social norms because many social norms, such as norms of cooperation, are often not Nash equilibria.

Section 2.7 considers a range of other behavioral models of game theory. Each of these models establishes a promising new direction, although the extent of psychological underpinnings of these models, and the evidence for them, varies. Equilibrium concepts based on these models, that we consider, include, *analogy based equilibrium*, *subjective heterogeneous quantal response equilibrium*, *evidential equilibrium*, *cursed equilibrium*, and *noisy introspection*.

The final section, Section 2.8, considers the behavioral economics of auctions. Particular attention is given to *common value auctions* that are characterized by a *winner's curse*. We consider the relevant theoretical explanations and the empirical evidence.

The final chapter, Chapter 3, explores some recent developments in behavioral game theory. Section 3.2 introduces *Kantian rationality* that is based on social rationality rather than individual rationality which underpins a Nash equilibrium. We show how it may explain cooperation in a prisoner's dilemma game. Section 3.3 discusses several topics. This includes the role of communication in choosing between rigid and flexible contracts; portability of levels across games in level-*k* models; team reasoning; and the winner's curse in the presence of public signals only. Section 3.4 explores several determinants of human cooperation. These include the optimization premium, cognitive ability, domain of choice (gains versus loss domains), and preplay communication. Section 3.5 explores the evidence on the play of a correlated equilibrium when the recommendations differ in the degree of payoff inequality that they create. Section 3.6 examines several findings from the recent literature on psychological game theory including the consistency of beliefs and actions, the potential explanation for promise-keeping, and the evidence from public goods games with psychological motivations. Finally, in Section 3.7 we consider the economics of microfinance contracts in a self-contained discussion that highlights the role of psychological factors such as guilt and shame in explaining contractual choice.

Introduction to Behavioral Economics and the Book Volumes

The *neoclassical framework* in economics provides a coherent and internally consistent body of theory that offers rigorous, parsimonious, and falsifiable models of human behavior.[1] Augmented with auxiliary assumptions, it is flexible enough to analyze a wide range of phenomena. In actual practice, the neoclassical framework includes, but is not restricted exclusively to, consistent preferences, subjective expected utility, Bayes' rule to update probabilities, self-regarding preferences, emotionless deliberation, exponential discounting, unlimited cognitive abilities, unlimited attention, unlimited willpower, and frame and context independence of preferences.[2] Neoclassical economics is also typically underpinned by optimization-based solution methods and an equilibrium approach.

In principle, the neoclassical framework is capable of relaxing many of its standard assumptions. For instance, it can allow for reference dependence preferences, social preferences, frame dependent preferences, and non-exponential models of discounting. However, these extensions are rare in actual practice, and when they are made, the neoclassical framework typically does not have fundamental new insights to offer. For instance, adding reference dependent preferences generates few, if any, insights in the absence of a theory about how human behavior differs in the domains of gains and losses relative to a reference point. Similarly, adding other-regarding preferences without attempting to fit such a model to the behavior of humans, particularly to the evidence from experimental games, offers little progress. For these reasons, my use of the term *neoclassical economics* is shorthand for *the typical practice in neoclassical economics*.

The intellectual developments in neoclassical economics are impressive. However, its empirical success in predicting and explaining human behavior is modest. Indeed, an impressive, thorough and detailed body of experimental, neuroeconomic, and field evidence, based on several decades of work, raises serious concerns about the core assumptions and predictions of neoclassical models. This has been matched by impressive theoretical developments, drawing on insights from psychology, biology, anthropology, sociology, and other social sciences, that

[1] I avoid the loaded term *standard economics* to refer to *neoclassical economics* because this might give the latter a certain empirical sanctity.

[2] I have deliberately avoided the word 'rationality' in this description of the neoclassical framework because it would have to be precisely defined. See Dhami and al-Nowaihi (2018) for the various senses in which rationality is used in neoclassical economics.

has come to be known as *behavioral economics*. These models have had much greater empirical success relative to neoclassical models.[3]

There is a danger that one may propose definitions of behavioral economics that are either too broad and have ambiguous scope, or are too narrow with limited scope; each of these outcomes would be unfair for a newly emerging field. Any falsifiable theory that replaces/modifies any of the core features of neoclassical economics, by alternatives that have a better empirical foundation in human behavior is a potential member of the class of behavioral economic theories, if it can pass stringent empirical tests.

The aim of this book is to offer an account of formal behavioral economic theory, its applications, and a discussion of the underlying experimental and field evidence.[4] The standard toolkit in neoclassical economics is adequate for the study of behavioral economics. Most behavioral models adopt an optimization framework, are typically underpinned by axiomatic foundations, are parsimonious, rigorous, falsifiable, and internally consistent.[5]

We do not attempt to pit behavioral economics against neoclassical economics in a paradigmatic battle. As in every science, we progress by taking account of evidence that suggests a refinement and improvement of existing models. In this case, the relevant improvement appears to have the steepest gradient in the direction of constructively incorporating insights from other behavioral sciences. The book outlines a new research program that offers a constructive way forward for economics by highlighting developments in behavioral economic theory, which also uses core insights from neoclassical economics. It is likely that in due course, behavioral economics will cease to exist as a separate field within economics, and this will become the normal way in which we do economics.

A distinction is sometimes drawn between experimental economics and behavioral economics.[6] However, the activity of behavioral economists and experimental economists has turned out to be complementary and collaborative, as in the natural sciences. It is often difficult to spot the dividing line between their work. For instance, experimental economists not only test the predictions of economic models, but their results have often been critical in suggesting further developments in behavioral models. Behavioral theorists on the other hand, often suggest experiments that could test their proposed theories.

The introduction to these volumes is a condensed version of the longer introduction in Volume 1 of *The Foundations of Behavioral Economic Analysis*. Section 1 briefly traces some of the historical developments that have led to modern behavioral economics. Section 2 considers important methodological issues that lie at the heart of how economists 'do' and 'should'

[3] Increasing the explanatory power of neoclassical economics is very worthwhile but Thaler (2015) adds another reason for studying behavioral economics in his inimitable style: "Behavioral economics is more interesting and more fun than regular economics. It is the un-dismal science."

[4] For a non-technical treatment of behavioral economics, the reader can consult the extremely readable and witty account by Thaler (2015) that offers a much more detailed historical account of developments in behavioral economics from the 1970s onwards from a personal perspective.

[5] I use the word "rigorous" purely for its practical appeal to most neoclassical economists but I agree with the sentiments expressed by Gintis (2009, p. xviii): "The economic theorist's overvaluation of rigor is a symptom of their undervaluation of explanatory power. The truth is its own justification and needs no help from rigor."

[6] Loewenstein (1999) gives a nice discussion of the methods in each of these areas and offers the following definition (p. F25): "BEs [behavioral economists] are methodological eclectics. They define themselves, not on the basis of the research methods that they employ, but rather their application of psychological insights to economics. In recent published research, BEs are as likely to use field research as experimentation ... EEs [experimental economists] on the other hand, define themselves on the basis of their endorsement and use of experimentation as a research tool."

practice their craft. Section 3 considers the importance of the experimental method in behavioral economics. Section 4 briefly explains the organization of the book. There are two appendices. Appendix A outlines the random lottery incentive mechanism that lies at the heart of the modern experimental method in economics. Appendix B asks you to think of 50 questions as a problem set, but I deliberately give you very little structure at this stage in order to enable a free-spirited approach to the answers. Rigorous answers to these questions can be found in the book.

1 Some antecedents of behavioral economics

While Adam Smith's justly celebrated book, *The Wealth of Nations*, is widely cited, his other book, *The Theory of Moral Sentiments*, has received less attention. *The Theory of Moral Sentiments* reads like an agenda for modern behavioral economics; it recognizes many behavioral phenomena such as loss aversion, altruism, emotions, willpower, and the planner–doer framework (Ashraf et al., 2005). Classical economists such as Jeremy Bentham wrote about the psychological underpinnings of utility and Francis Edgeworth wrote about social preferences (Camerer and Loewenstein, 2004). Bardsley et al. (2010) trace the beginnings of experimental economics to the classical economists such as David Hume, Stanley Jevons, and Francis Edgeworth; Jevon's marginal utility analysis derived its motivation from experimental observations about the relation between stimuli and sensations.

Two factors contributed to the gradual elimination of psychology from economics. First, around the turn of the twentieth century, there was "a distaste for the psychology of their period, as well as the hedonistic assumptions of Benthamite utility" (Camerer and Loewenstein, 2004). The second was the revealed preference approach popularized by Paul Samuelson that emphasized the observation of *choice behavior* rather than the psychological foundations for choice behavior (Bruni and Sugden, 2007). Glimcher and Fehr (2014, p. xviii) write: "It cannot be emphasized enough how much the revealed-preference view suppressed interest in the psychological nature of preferences, because clever axiomatic systems could be used to infer properties of unobservable preference from choice."

Important, and path-breaking, developments in behavioral economics took place in the 1950s and 1960s that included: violations of the independence axiom of expected utility theory (Allais, 1953); violations of subjective expected utility (Ellsberg, 1961; Markowitz, 1952); demonstration of the importance of bounded rationality (Simon, 1978; Selten, 1998);[7] and early work on quasi hyperbolic discounting (Phelps and Pollak, 1968). However, at that time, this work struggled to get the attention that it deserved.

An important catalyst for the development of behavioral economics was the decline of the behavioralist school in psychology, and the emergence of cognitive psychology. Cognitive psychology emphasized the role of mental processes in the understanding of tasks involving decision making, perception, attention, memory, and problem solving. Some cognitive psychologists naturally turned their attention to testing their models against the neoclassical framework. The two most important cognitive psychologists in this category were Daniel Kahneman and Amos Tversky, whose work in the 1970s helped kick-start modern behavioral economics. Along with Richard Thaler, who was an economist by training, and was struggling to make sense of several

[7] Simon (1978) refers to Herbert Simon's Nobel lecture that traces the historical development of bounded rationality through the 1950s and 1960s. Selten (1998) is an English language version of a paper that appeared initially in German in 1962.

anomalies in neoclassical economics from the mid 1970s onwards, they are some of the earliest and most significant modern behavioral economists.

The second topic is the *role of experimental evidence in economics* that I consider in Section 3. The justification for this section is the continued skepticism of many economists about experimental economics, which constitutes an important part of the evidence base for behavioral economics. The following quote attributed to the Nobel Prize winner Gary Becker from a magazine interview (Camerer, 2015, p. 250) is probably not unrepresentative: "One can get excellent suggestions from experiments, but economic theory is not about how people act in experiments, but how they act in markets. And those are very different things. That may be useful to get suggestions, but it is not a test of the theory. The theory is not about how people answer questions. It is a theory about how people actually choose in market situations."

What follows is a somewhat long introduction, but this is a somewhat long book too. In mitigation, the first one third of the introduction largely deals with background material that reflects the somewhat unsettled nature of economics. My hope is that if a second edition of this book is ever written, then there would be enough convergence of views on this material so that I can safely omit it.

2 On methodology in economics

University degrees in Economics and the natural sciences typically do not require formal courses in methodology. Yet, while all the natural sciences subscribe to the scientific method and students of natural sciences instinctively know that this means, economics has taken a very different, and pernicious, direction that has little basis in the scientific method. Consider, for instance, the following quote from Gintis (2009, p. xvi) that nicely captures the essence of the problem:

> Economic theory has been particularly compromised by its neglect of the facts concerning human behavior . . . I happened to be reading a popular introductory graduate text on quantum mechanics, as well as a leading graduate text in microeconomics. The physics text began with the anomaly of blackbody radiation, . . . The text continued, page after page, with new anomalies . . . and new, partially successful models explaining the anomalies. In about 1925, this culminated with Heisenberg's wave mechanics and Schrödinger's equation, which fully unified the field. By contrast, the microeconomics text, despite its beauty, did not contain a single fact in the whole thousand-page volume. Rather the authors built economic theory in axiomatic fashion, making assumptions on the basis of their intuitive plausibility, their incorporation of the "stylized facts" of everyday life, or their appeal to the principles of rational thought . . . . We will see that empirical evidence challenges some of the core assumptions in classical game theory and neoclassical economics.

The actual practice of behavioral economics is influenced, directly or indirectly, by Popperian views on methodology (Popper, 1934, 1963). Popper begins by distinguishing between science and non-science. A scientific hypothesis must be falsifiable in the sense that it must specify the conditions under which the hypothesis can be rejected. Further, one can only refute theories but never prove that they are true. For instance, the observation of a million white swans is consistent with the hypothesis that "all swans are white" but does not prove that the hypothesis is true; for the very next observation could be a non-white swan.

The best recipe for the advancement of science, in the Popperian view, is to subject scientific hypotheses to stringent testing, i.e., expose the hypotheses to tests that are most likely to reject them. In the strict Popperian view, one observation that is contrary to a hypothesis rejects it. For instance, a single observation of a black swan rejects the hypothesis that all swans are white. Science progresses by advancing a new hypothesis that explains everything that a rejected hypothesis explained, but, in addition, it explains some new phenomenon that the rejected hypothesis could not. For an application of the Popperian position to economic contexts, see Blaug (1992), Hausman (1992), and Hands (2001).

One concern with the Popperian approach is that a test of a hypothesis is a joint test of the hypothesis and several auxiliary assumptions. Thus, a rejection may arise because the hypothesis is incorrect, or the auxiliary assumptions might have been rejected, or both; this is known as the *Duhem–Quine thesis* (DQT). For instance, in an experimental test that rejects mixed strategy Nash equilibrium, one might wonder if the rejection was caused by (1) one of several confounding factors, such as an inappropriate subject pool, unclear experimental instructions, and inadequate incentives, or (2) because subjects do not follow a mixed strategy Nash equilibrium. For this reason, a single refutation of a theory is not sufficient unless well replicated to account for all the main confounding factors that might be at play.

While the Popperian position is *prescriptive* (how should we best do science?), a *descriptive* view (how is science actually done?) was offered by Kuhn (1962). Kuhn noted that knowledge in science does not accumulate in a linear manner. He highlighted, instead, the role of periodic revolutions in science, or an abrupt transformation in the existing worldview, a *paradigm shift*. He distinguishes between three phases in the development of any science. In pre-science, there is no central paradigm, but there is an attempt to focus on a set of problems. In normal science, the longest of the three phases, there is the establishment of a central paradigm, great progress is made in answering many of the questions posed during pre-science, and much success is achieved in answering new questions. In a departure from the Popperian prescriptive position, in this phase, rejections of the paradigm are robustly challenged or ignored, and belief in the paradigm is unshakable. However, as anomalies gradually begin to accumulate, and reach a tipping point, a crises takes place in the paradigm. There is a sudden paradigm shift and a new paradigm that subsumes the old paradigm takes its place.

One prescriptive response to the DQT and to Kuhn's descriptive ideas, while retaining a Popperian approach, was proposed by Lakatos (1970) under the name: *The methodology of scientific research programs* (MSRP). Lakatos distinguished between a set of non-expendable statements or assumptions, which is the *hard core of a research program*, and a set of expendable auxiliary assumptions. In a distinctly non-Popperian recommendation, but reminiscent of the normal science phase of Kuhn, the hard core is insulated from refutation; this also addresses the DQT. For instance, Newtonian physics has a hard core that comprises the three laws of dynamics and a law of gravitation. Any refutation of the research program, in this phase, is then ascribed to a failure of the auxiliary assumptions, which are modified to explain the refutation.

One potential defense of this approach is that it allows for a period of time for the development of a new research program that can take account of the emerging refutations. However, a practical downside could be that proponents of a research program might engage in defensive methodology for far too long, and resist the development of a new research program that has a different hard core. To take account of this possibility, Lakatos termed a research program as *theoretically progressive* if refinements that take place by altering auxiliary assumptions but not the hard core, lead to the explanation of existing anomalies and to novel predictions. A research program is *empirically progressive* if the novel predictions are not refuted. Adherence to a hard core is only admissible if research programs are theoretically and empirically progressive.

Eventually anomalies play the most important part in giving rise to new research programs; Lakatos noted that all theories are born into and die in a sea of anomalies.[8] The reader may find below that the actual practice in behavioral and experimental economics appears to be closer to the Lakatosian view than the Popperian view.[9] For instance, in decision theory, the hard core may be thought to comprise completeness, transitivity, and first order stochastic dominance (Bardsley et al., 2010, p. 129). Indeed, neither expected utility theory nor the main behavioral alternatives such as rank dependent utility, theory of disappointment aversion, or prospect theory, are willing to relax the assumption of well-behaved preferences. This makes it difficult for most decision theories to explain framing effects, although prospect theory is potentially able to capture framing effects through changes in the reference point.

With this minimum background, consider "normal" practice in physics; I encourage the reader not to judge natural sciences by a few well-publicized outliers. In a letter to the *London Times*, dated November 28, 1919, Albert Einstein described his *theory of relativity* in comparison to *Newtonian physics*, to a lay audience. Einstein mentioned two predictions of his theory that had been confirmed (both in domains where his theory was most likely to fail, hence, these are "stringent tests"): (1) Revolution of the ellipses of the planetary orbits round the sun, which was confirmed for the orbit of Mercury. (2) The curving of light rays by the action of gravitational fields. He then mentioned one prediction that had not yet been confirmed (displacement of the spectral lines toward the red end of the spectrum in the case of light transmitted to us from stars of considerable magnitude); indeed, at the time Einstein published the theory of relativity, it was not even clear how to test this prediction. Einstein then wrote (p. 4): "The chief attraction of the theory lies in its logical completeness. If a single one of the conclusions drawn from it proves wrong, it must be given up."

I invite the reader to pause for a moment to compare Einstein's approach with the "mainstream" views in economics that I have outlined above. Indeed, as Bardsley et al. (2010, p. 8) note: "But it is surprisingly common for economists to claim that the core theories of their subject are useful despite being disconfirmed by the evidence."

In light of this brief discussion on methodology and an illustration of best practice in the natural sciences, let us return to the "neglect of the facts concerning human behavior in economics" that Herbert Gintis highlights above. Why should such a situation have arisen? In order to understand this state of affairs, consider the following three representative views, written by some of the leaders in neoclassical economics.

Dekel and Lipman (2010, p. 264) write: "Hence the choice of a model will depend on the purpose for which the model is used, the modeler's intuition, and the modeler's subjective judgment of plausibility .... One economist may reject another's intuition, and, ultimately, the marketplace of ideas will make some judgments."

Gilboa et al. (2014, F. 516) write: "In particular, we agree that: economic models are often viewed differently than models in the other sciences; economic theory seems to value generality and simplicity at the cost of accuracy; models are expected to convey a message much more than to describe a well-defined reality; these models are often akin to observations, or to

[8] Closer to home, economists would remember the influential *anomalies feature* that Richard Thaler wrote for the *Journal of Economic Perspectives* from 1987 to 2006. Indeed, in the very first piece, Thaler, keenly aware of methodological issues, quoted from Thomas Kuhn.

[9] For a critique of the Lakatosian approach as applied to economics, see Hands (1991) and De Marchi and Blaug (1991).

gedankenexperiments; and the economic theorist is typically not required to clearly specify where his model might be applicable and how."

Rubinstein (2006, p. 882) writes: "As in the case of fables, models in economic theory are derived from observations of the real world, but are not meant to be testable. As in the case of fables, models have limited scope. As in the case of a good fable, a good model can have an enormous influence on the real world, not by providing advice or by predicting the future, but rather by influencing culture. Yes, I do think we are simply the tellers of fables, but is that not wonderful?"

None of these representative quotes stresses the centrality of the empirical evidence in rejecting economic models or the need to design stringent tests to refute them; in fact economic models are not even meant to be tested. They also take a relativist position (one economist may reject another's intuition, and, ultimately, the marketplace of ideas will make some judgments) and take the role of models in economics as conveying "messages" or telling "fables."

Modern economics has been heavily influenced by the *instrumental position* taken by Friedman (1953), which is partly reflected in the three quotes above. Friedman argued that we should not judge economic theories by the realism of their assumptions but rather, by the accuracy of their predictions. He writes (p. 14): "Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions .... To be important, therefore, a hypothesis must be descriptively false in its assumptions." And shortly thereafter (p. 15) he writes: "To put this point less paradoxically, the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic,' for they never are, but whether they are sufficiently good approximations for the purpose in hand."

A natural progression of Friedman's position can be found in Gilboa et al. (2014, F. 514): "Why does economic theory engage in relatively heavy technical analysis, when its basic premises are so inaccurate? Given the various violations of fundamental economic assumptions in psychological experiments, what is the point in deriving elaborate and carefully proved deductions from these assumptions? Why do economists believe that they learn something useful from analyzing models that are based on wrong assumptions?" Their answer to these questions is based on an identification of economic models with *case-based reasoning* rather than *rule-based reasoning*. Rule-based reasoning requires the formulation of general rules or theories. In contrast, case-based reasoning requires one to draw inferences based on similar past cases. The purpose of economic models, in this view, is to add to the bucket list of cases and analogies that can be used to draw inferences now, or at some point in the future.

These views give a fair bit of insight into contemporary thinking in economics about how we should go about practicing our craft. I also believe that acceptance of these views is widespread in the economics profession and many economists challenged on these views are surprised and outrightly dismissive. Initial intuition about economic models, whether motivated by existing empirical evidence, or a desire to make novel predictions, must begin from somewhere. Here, the role of initial conjectures as parables, useful stories, or fables to inform one's intuition about better and more complete models is surely important. But this cannot be the justification for continued reliance on a set of models that have faced persistent refutation, or to wish to shield them from refutation by seeking a special status for them.

Indeed, and it has to be said with great regret, many of the contemporary methodological views in economics are retrogressive and a license to engage in defensive methodology to protect the status quo. Friedman's approach has been much misused in economics. Consider the following entirely reasonable description of Friedman's approach to *model building* (as distinct from evaluating theories) in Gintis (2015, p. 223) that this book concurs with: "The goal of model-building [is] to create a tractable analytical structure, analyze the behavior of this structure, and test the fruitfulness of the results by comparing them with empirical data."[10]

The tendency to ignore or to discount experimental evidence in economics, despite its growing importance and prominence, when it contradicts neoclassical models is an indictment of the methodological approach taken in economics. Another important factor is that Friedman's instrumental position has been used as a license by some to make ad hoc auxiliary assumptions, and others to genuinely believe that their assumptions are literally true in an "as if" sense. Any empirical rejection of the "as if" assumptions is often rejected on the grounds that the evidence is flawed, untrustworthy, based on dubious experimental methods, or lacks external validity. This is a form of defensive methodology that is inimical to the progress of economics, and I urge the reader to resist it.

Behavioral economics offers an easier resolution of the "as if" approach. There is now compelling evidence, which shows that some of the central tenets of neoclassical economics are neither true in an "as if" sense, nor are their predictions always satisfactory when subject to stringent tests. So even on the grounds that Friedman favoured, *predictions of the relevant theory*, some of the central elements in neoclassical economics, such as self-regarding preferences, expected utility theory, exponential discounting, Bayes' Law, Nash equilibrium and its refinements, must either be significantly modified or abandoned. This book is replete with evidence that supports such a view. In particular, it is untenable to continue teaching the entire corpus of the existing status quo in economics on any scientific or logical grounds.

Schotter (2015) offers the following critique of Friedman's position. Suppose that assumptions x, y, and z lead to some theory T. Suppose also that one or more assumptions are violated by the empirical evidence, yet T makes a successful prediction. Then there are three possibilities. (1) The violated assumptions are superfluous for the theory, at least in the context where the theory was tested. (2) The violated assumptions counteract each other perfectly, so they do not affect the prediction. (3) The successful prediction is a fluke. Conversely, if the assumptions are correct and the model is complete then we expect T to make successful predictions anyway. Thus, it is difficult to justify a theory based on patently false assumptions. Schotter (2015, p. 63) observes, correctly: "after all, the assumptions are the theory."

My colleague, Ali al-Nowaihi, likes to give the following example that applies to birds who cannot swim (e.g., gannets can swim, so they are excluded). Birds fly, so one may theorize that they behave "as if" they understand the laws of aerodynamics. This is an admissible hypothesis, but then one must test the "as if" assumption. Given that air is basically a fluid, so birds might also be assumed to know the laws of hydrodynamics. If the "as if" presumption were true in this case, then birds released under water should try to swim, but they actually try to fly, and drown. Thus, the original "as if" supposition is false. If the "as if" assumptions are not tested properly, then we can never have any degree of confidence in the models based on these assumptions.

A common view in economics (shared unfortunately by some behavioral and experimental economists, I must add) appears to be that there is something rather difficult and unique about testing economic theories, relative to the natural sciences. So, at least implicitly, the argument goes, one needs to accord a "special status" to economic theories. Consider the following representative quote from Richard Lipsey's wonderful introduction to economics (Lipsey, 1979, p. 8)

[10] Readers interested in pursuing this approach further can consult Godfrey-Smith (2006, 2009) and Wimsatt (2007).

cited in Bardsley et al. (2010, pp. 6–7) that, I suspect, many economists would agree with: "Experimental sciences, such as chemistry and some branches of psychology, have an advantage because it is possible to produce relevant evidence through controlled laboratory experiments. Other sciences, such as astronomy and economics, cannot do this." A similar view is expressed in another celebrated text in economics (Samuelson and Nordhaus, 1985, p. 8): "Economists (unfortunately) . . . cannot perform the controlled experiments of chemists or biologists because they cannot easily control other important factors. Like astronomers or meteorologists, they generally must be content largely to observe." This mainstream view is contestable, and must be contested. There appears to be a misunderstanding about the relative difficulty of testing theories in the natural sciences and in economics.

The view that testing of theories is somehow easy or easier in the natural sciences, as compared to economics, must surely be deeply offensive and insulting to experimenters in the natural sciences. The Higgs boson or Higgs particle was proposed by British physicist Peter Higgs in the early 1960s, and it took 50 years of incredibly hard efforts to confirm the particle in 2013. Particle physicists did not seek a *special status* for this theory that could insulate it from rejection. The enormously high energies required to test for the Higgs particle required the construction of a very expensive and complex experimental facility, CERN's Large Hadron Collider, that eventually confirmed the theory. Note also that Peter Higgs was made to wait 50-odd years and given the Nobel Prize in physics only after his theory was confirmed. He was not given the Nobel on any of the following criteria: elegant and beautiful theory, useful model that helped the intuition of particle physicists, or a fable or useful story that aids in the understanding of how the universe began.

Astronomers who dealt with the question of the distance of earth from distant objects, or the chemical composition of stars that are millions of light years away, did not also seek a special status for their subject. They got on with the difficult job of seeking the relevant measurements, often using indirect evidence and clever implications of theory. They were eventually successful after several decades of work. Are economists seriously arguing that their measurement problems are more difficult than the problems in the natural sciences? Cosmic microwave background radiation was first proposed in 1948, but experimentally confirmed due to an accidental discovery in 1964. DNA was first isolated in 1869, but it took the most part of a century to find the double-helix structure of DNA, and confirm it by experimental evidence in 1953. The germ theory of disease was proposed in the mid sixteenth century, yet confirmation of the theory occurred in the seventeenth century. The pool of such examples is very large. The process of discovery, measurement, and of testing the theory, can be a long and arduous one; seeking a special status for the subject is defeatist and put bluntly, lazy.

Economists opposed to lab/field data are likely to argue that the behavior of humans is too noisy, heterogeneous, and fickle, which is not a problem in the natural sciences (e.g., atoms are, after all, not subject to mood swings). This overstates the degree of difficulty in testing economic theories, relative to those in the natural sciences on at least two grounds.

1. Experimental economics has discovered systematic human behavior in many of the most important domains in economics. A small sample includes reference dependence, loss aversion, non-linear probability weighting, conditional cooperation, intention-based reciprocity, present-biased preferences, and the importance of emotions such as regret, guilt, and disappointment. These behaviors are also underpinned by neuroeconomic evidence. Replication of standard experimental results is routine, and if similar subject pools and protocols are used, experiments produce replicable data. Examples are results from double

auction experiments, and a range of games that demonstrate human prosociality, such as the ultimatum game, the gift exchange game, the trust game, and the public goods game; these examples can be multiplied manyfold, as the results in this book attest.

2. If indeed human behavior is inherently too noisy and heterogeneous, then economic theory needs to focus more efforts in this direction. When Brownian motion was discovered in 1827 by Robert Brown, in the behavior of pollen grains, physicists did not throw up their arms in despair. Important work in the late part of the nineteenth century, and by Einstein in the early twentieth century, paved the way for describing not only the mathematics of Brownian motion, but also predicting the probability distribution of particles in Brownian motion. Perhaps, in an analogous manner, economic theories need to predict the probability distribution of economic behavior, which can then be tested in experiments.

Experimental economics in the lab, and in the field, has made enormous progress in developing new econometric techniques for small samples, and in novel experimental methods. It has also deeply enhanced our understanding of human behavior and allowed for stringent testing of economic theory. This progress is inconsistent with the view that we should grant a special status to economic theories that exempts them from careful and stringent testing. The differences in experiments in economics and the natural sciences are much smaller relative to the differences in attitudes and institutions in the two fields of study. Progress in economics will be substantially enhanced if we learn from best practice elsewhere, and give up our implicit demand for special status.

3 The experimental method in economics

Work on experiments in behavioral economics gained momentum following the seminal work of Daniel Kahneman and Amos Tversky in the 1970s. However, a number of important experiments in economics were also conducted in the late 1940s, the 1950s, and the 1960s. These include Edward H. Chamberlin's testing of general competitive equilibrium (Chamberlin, 1948); Maurice Allais's work on demonstrating violations of the independence axiom in expected utility theory (Allais, 1953); Vernon Smith's work on induced value elicitation and double auction experiments in competitive settings (Smith, 1962); and Sidney Siegel's experiments on bargaining (Siegel and Fouraker, 1960). Other prominent figures who were either involved in experimental economics, or expressed an interest in it during the 1950s and 1960s included Ward Edwards, Reinhard Selten, Martin Schubik, Herbert Simon, Charles Plott, Donald Davidson, and Pat Suppes; for a brief historical sketch, see Guala (2008) and Bardsley et al. (2010).

Experimental economics is now mainstream by most yardsticks, particularly in terms of its presence in peer-reviewed journals in economics. In his early surveys on experimental economics, Roth (1987, 1988) hoped that experimental economics would perform three kinds of functions: speaking to theorists (testing economic theory), searching for facts (generating novel empirical regularities that could be modeled by subsequent theory), and whispering in the ears of princes (offering reliable policy advice). Roth (2015) takes stock of experimental economics on these criteria and finds that it is thriving. One of his case studies, on bargaining behavior, is outlined in detail in Volume 4 of the book.

At one level, there has been a complete denial of the usefulness of experiments in economics. Friedman (1953, p. 10) views the domain of empirical testing in economics to be naturally occurring field data: "Unfortunately, we can seldom test particular predictions in the social sciences by experiments explicitly designed to eliminate what are judged to be the most important disturbing influences. Generally, we must rely on evidence cast up by the 'experiments' that happen to occur." A modern critique of the experimental method in economics is offered by Levitt and List (2007). They list several objections to experimental results that I address in subsequent sections.

(1) Participants in experiments are subjected to unprecedented experimental scrutiny. Since subjects may perceive that they are being watched over by the experimenter, they may give responses that the experimenter really desires (*experimenter demand effects*; see Zizzo, 2010) or they may not reveal their true underlying preferences. For instance, they worry that participants may engage in more prosocial behavior than they really intend to.

Whilst I reserve my detailed responses to later sections, I find it somewhat curious that if subjects are accused of being influenced by experimenter demand effects, say out of reciprocity, guilt, or shame, then they appear to exhibit social preferences (or emotions reflected in beliefs may directly enter their utility functions, as in psychological game theory), which is precisely what is being disputed by the critics.

(2) In actual practice, human decisions are context-dependent and influenced by cues, social norms, and past experiences. It is not clear that experiments can capture these factors. For instance, participants in experiments may import an inappropriate "outside context" into their responses in experiments.

(3) Actual human behavior is strongly affected by stake sizes in experiments. Experiments are typically conducted with small stakes, so they might not capture the richness of human behavior that arises from varying stakes.

(4) There could be self-selection biases caused by student volunteers who might be particularly prosocial, younger, more educated, and have a higher need for approval, as compared to the average human population. In contrast, people who self-select themselves into real market situations, might be particularly suitable to do well in real markets.

(5) Choice sets in experiments might be particularly restrictive relative to the real world. For instance, there could be more prosocial options in experiments relative to the real world.

(6) The results of lab experiments may generalize poorly to real-world behavior for all of the reasons mentioned in (1) through (5), above. This issue of *external validity* of lab experiments is the main concern raised by the authors who write (p. 170): "Perhaps the most fundamental question in experimental economics is whether findings from the lab are likely to provide reliable inferences outside of the laboratory."

This discussion briefly encapsulates the modern case against experimental economics. Let us now briefly examine these claims.

3.1 *Experiments and internal validity*

Experiments allow for unprecedented control over the economic environment, hence, they have high *internal validity*, which is critical for stringent tests of economic theories. Internal validity is reduced when there are, for instance, selection issues, confounds in treatments, and unclear experimental instructions, all of which are carefully addressed in modern experimental work. Thus, in well-conducted experiments, the complicated identification strategies of field studies can be replaced by clever and much simpler experimental design.

For instance, suppose that a researcher is interested in testing if higher wages elicit higher effort in a firm; this is known as a *gift exchange game*. A field experiment is likely to be influenced by strategic behavior and reputational concerns of the workers and firms; field experiments in general, are likely to have lower internal validity. However, in a lab experiment, these factors are easily controlled, allowing one to cleanly separate the relation between a fair-wage and effort. The high degree of experimental control in lab experiments allows for replication of lab results. For the converse reason, the results of field experiments are more difficult, and sometimes impossible to replicate when one is given access to a unique field environment.

Experiments can also test the predictions of theory in a parameter space that might be difficult to observe in the field. This is similar to extreme stress tests of aircraft frames under conditions that are not normally encountered in the actual operation of the aircraft, or the exposure of bridge designs to extreme environmental conditions. In a nutshell, all this allows for more stringent tests of economic theory. Experiments are sometimes criticized on the grounds that the sample sizes are small. Falk and Heckman (2009) term this issue as a "red herring" on the grounds that there have been important developments in small sample econometrics, and many experiments do use large subject pools.

Camerer (2015) argues that there is no evidence of experimenter demand effects, despite the suspicion that there might be such effects; see also his discussion of the alternative interpretations of experimenter demand effects in Hoffman et al. (1998). There are several reasons why experimenter demand effects may be weak or non-existent. Such demand effects require two conditions. First, subjects must know the experimenter's preferred hypothesis. Second, they should be willing to sacrifice their own experimental earnings in order to favor the experimenter's preferred hypothesis.

On a-priori grounds, arguably, it is often quite hard for subjects to know the experimenter's preferred hypothesis. This arises particularly when (i) experimental instructions are carefully worded to prevent any such inference, and (ii) the experimenter might not be sure which of the competing hypotheses actually hold. However, if subjects can somehow guess the preferred hypothesis, then stakes can be raised to levels where they are too difficult to sacrifice for the sake of pleasing the experimenter. However, in most cases, the results with high stakes are not dramatically different from those with modest stakes (Camerer and Hogarth, 1999).[11] In three preference reversal experiments, Lambdin and Shaffer (2009) find that the percentage of subjects who were successfully able to guess the preferred hypothesis of the experimenter was 7%, 32%, and 3%.

The degree of anonymity in lab experiments can be varied, so it is an ideal environment to test for the effects of variation in the degree of anonymity (Bolton et al., 1998). One's actions are often observed by others in real-world situations, and in many field situations, where controlling for such scrutiny, and varying its level, is arguably even more difficult. The criticism of lab experiments on grounds of scrutiny (by the experimenter and other participants), also applies to field experiments, insofar as field subjects realize that they are in an experiment. Such experimenter demand effects may arguably, in many cases, be even stronger in field experiments, which are typically run in collaboration with governmental and semi-governmental bodies, and NGOs.

It is indeed the case that when subjects are observed in dictator game experiments in the lab, they give higher amounts (Dana et al., 2007; Haley and Fessler, 2005). In many real-world giving situations, actions are also observed by others; for instance, church collections that take the form

[11] Andersen et al. (2011) consider extremely high stakes ultimatum game experiments; the stakes vary from the equivalent of 1.6 hours of work to 1,600 hours of work. The median offer by the proposer is to give 20% of the share to the respondent, but the rejection rate falls with the increase in the stake. In real life, we rarely make decisions involving 1,600 hours of work, yet social preferences were not eliminated in the experiment.

of passing along a collection plate/basket, or having to declare one's charitable contributions for tax purposes. However, the effect of being observed disappears if one introduces a minimal element of strategic interaction as, say, in an ultimatum game (Barmettler et al., 2012). A more important determinant of giving in dictator games is whether income is earned or not. Giving in dictator game experiments falls to about 4.3% of an endowment of $10, when income is earned, relative to about 15% of the endowment in the case of unearned income (Cherry et al., 2002); the figure of 4.3% is closer to the corresponding field benchmark of charitable giving in the US, which stands at about 1% of income (Camerer, 2015).

A commonly heard critique of behavioral models of social preferences is that if experimentally observed social preferences are so important, then, putting it rather starkly, why do we not observe people giving envelopes stuffed with money to others (Bardsley et al., 2010, p. 53)? When dictators in experiments give out of earned income, then the extent of giving is not too far off from the rate of charitable giving (4.3% versus 1% for the case of US; see above). In the real world, subjects give money for charitable and other good causes out of after-tax income, which is not the case in the lab. So imagine that in dictator games in the lab with earned income, the dictator was told: "Here is your endowment of $10, which you have earned. We are taking 30% off as taxes, which we will partly use for redistributive purposes to the recipient in the experiment. How much of the rest will you offer to the recipient?" It would be surprising if the 4.3% giving in lab dictator games does not get closer to the 1% figure for charitable giving in the field. Similar observations apply to proposer offers and responder rejections in lab experiments that do not include a tax redistributive component. If this is the case, then giving in experiments may also be tapping into the innate human desire to redistribute to others, that is, at least partly, codified institutionally in the social welfare state.

3.2 *Subject pools used in lab experiments*

It is not unusual in many quarters to dismiss experiments conducted on students, the typical lab subject pool, as having limited or no relevance to testing economic theories. There are several objections to this claim that we now outline.

Economic theory does not specify the subject pool on which its predictions are to be tested. Gilboa et al. (2014, F. 516) write "the economic theorist is typically not required to clearly specify where his model might be applicable and how." Clearly, one cannot have it both ways by not specifying a subject pool and then objecting to a particular subject pool. This view has been popularized in Vernon Smith's *blame the theory argument*. Writing in the context of incentives in experiments, Smith (2001) writes in his abstract: "The rhetoric of hypothesis testing implies that game theory is not testable if a negative result is blamed on any auxiliary hypothesis such as 'rewards are inadequate.' This is because either the theory is not falsifiable (since a larger payoff can be imagined, one can always conclude that payoffs were inadequate) or it has no predictive content (the appropriate payoff cannot be prespecified)."

One concern with the student subject population is that students might not have the necessary and relevant experience to conform to the predictions of the theory. However, one can allow lab subjects to gain experience in the lab by repeatedly making decisions; indeed, many lab experiments examine such learning effects and the effects of experience. We postpone a fuller discussion of these issues to Section 3.4, where we consider the external validity of lab experiments.

Students possess higher than average education and intelligence, which should be rather favorable to tests of neoclassical economic theory that requires economic agents to possess high levels of cognitive ability. It often comes as a surprise to the critics, but student subjects are much less prosocial relative to non-student subject pools (Falk et al., 2013; Carpenter and Seki, 2011; Anderson et al., 2013).[12] In a review of 13 studies that satisfy stringent tests of comparability, Fréchette (2015) finds that either there was very little difference between the behavior of students and professionals, or students were actually closer to the predictions of neoclassical theory. CEOs are often more trusting as compared to the student population (Fehr and List, 2004). More prosocial students do not self-select themselves as subjects in experiment (Cleave et al., 2012). Students who self-select themselves into experiments are motivated by monetary rewards (Abeler and Nosenzo, 2015), or interest in experimental lab tasks (Slonim et al., 2013). This evidence stands in contrast to the characterization of students in Levitt and List (2007) (based on two studies conducted in the 1960s) as scientific do-gooders who cooperate with experimenters to seek social approval.

3.3 *Stake sizes in experiments*

Economic theory does not specify the size of the stakes for which its predictions hold. Experimental economics is typically criticized for its low stakes. The evidence on stake size effects is mixed. However, many experimental results continue to hold, at least qualitatively, even with higher stakes (Slonim and Roth, 1998; Cameron, 1999). The most prominent effect of stakes arises when one moves from hypothetical payoffs to some strictly positive incentives. However, there is much less difference between moderate and high stakes; in particular, the main effect is a reduction in the variance of responses (Camerer and Hogarth, 1999).

There are two issues with high stakes, which are understated in many critiques of experimental economics. First, the vast majority of decisions that we make in real life are low stake decisions. How many times do we buy a car, a house, or a consumer durable such as a TV/laptop? Second, the main evidence for stake effects comes from experiments themselves. Third, as Thaler (2015) notes, the insistence on high stakes arises presumably because we are supposed to pay greater attention to economic decisions involving high stakes. But our success and expertise in making economic decisions is as much a matter of practice and learning. Since high stakes decisions are rare, we get limited opportunities to learn and make optimal decisions; the converse is true of low stakes decisions. Hence, there is no supposition that high stakes decisions should be closer to the predicted outcomes in neoclassical economics. So, he argues, correctly, that economists need to make up their minds whether they wish to insist on high stakes or low stakes as the appropriate test of their theories. Either way, experiments still offer the most natural environment to test the effect of stakes, which is an argument for more, not fewer, experiments.

3.4 *The issue of the external validity of lab findings*

Camerer (2015) distinguishes between the *policy view* and the *scientific view*. In the policy view, generalizability of lab findings to the field, or *external validity*, is essential. In the scientific view, all properly gathered evidence, including lab and field evidence, serves to enhance our understanding of human behavior. In this view, there is no hierarchical relation between lab and field evidence, and it is a mistake to pose the issue as if one had to make a choice between the two kinds of evidence. Camerer (2015, p. 251) explains cogently: "In this view, since the goal is to understand general principles, whether the 'lab generalizes to the field' (sometimes

[12] However, student subjects might be more prosocial when it comes to volunteering time (Slonim et al., 2013).

called 'external validity' of an experiment) is distracting, difficult to know (since there is no single 'external' target setting), and is no more useful than asking whether 'the field generalizes to the lab.' "

To understand Camerer's argument more fully, consider the following simple formalization in Falk and Heckman (2009). Suppose that we are interested in some variable Y that can be explained fully by the variables $X_1, X_2, \ldots, X_n$ and the "true" functional relation between them is given by $Y = g(X_1, X_2, \ldots, X_n)$, which is sometimes known as an *all causes model*. A researcher may be interested in examining the causal effect of X_1 on Y, holding fixed all other variables $\widehat{X} = (X_2, X_3, \ldots, X_n)$. For instance, in gift exchange experiments, Y is the level of effort of a worker and X_1 is the level of wage paid by the firm. The all causes model will typically include many factors in the vector $\widehat{X}$, such as the number of firms and workers, choice sets, payoff functions, incentives, demographic characteristics, regulatory environment, and moral and social characteristics of the parties involved.

When the relevant hypothesis is tested in the lab, the researcher estimates a model of the form $Y = f(X_1, X^L)$, rather than the all causes model $Y = f(X_1, \widehat{X})$, where $X^L \neq \widehat{X}$; X^L includes variables such as incentives given in the experiment, the endowments of subjects, the subject pool, context, and the structure of payoffs. One may also conduct field experiments in which one estimates a model of the form $Y = f(X_1, X^{F_1})$, where $X^{F_1} \neq \widehat{X}$, and typically $X^{F_1} \neq X^L$. Field experiments are conducted with a particular subject pool, such as sports card traders in List (2006).

The typical claim by critics of the experimental method is that $f(X_1, X^L)$ does not satisfy external validity, but $f(X_1, X^{F_1})$ does satisfy it. Now suppose that we are interested in examining the gift exchange relation in yet another population of subjects in the field, say, part time employees at General Motors. This gives rise to yet another estimated relation $Y = f(X_1, X^{F_2})$, where X^{F_2} reflects the set of variables and their characteristics in this field experiment. Is there any particular reason why the results based on the model $Y = f(X_1, X^{F_1})$ are more relevant, as compared to $Y = f(X_1, X^L)$, for predicting the causal effects of X_1 in the relation $Y = f(X_1, X^{F_2})$? Camerer (2015, p. 256) offers his assessment (expressed in our notation): "If the litmus test of 'external validity' is accurate extrapolation to X^{F_2}, is the lab X^L necessarily less externally valid than the field setting X^{F_1}? How should this even be judged?" Falk and Heckman (2009, p. 536) go slightly further: "The general quest for running experiments in the field to obtain more realistic data is therefore misguided. In fact, the key issue is what is the best way to isolate the effect of X_1 while holding constant $\widehat{X}$."

Since the criterion for external validity is unclear, it is best to treat lab and field evidence as complementary. Lab evidence allows for much tighter control of the variables in $\widehat{X}$. Field experiments allow for a larger variation in some aspects of $\widehat{X}$ (e.g., different subject pools with different demographic and social characteristics) while lab experiments allow for larger variation in other aspects of $\widehat{X}$ (e.g., exploration of the parameter space for values that can be hard or rare to find in the field). Lab experiments allow for greater replication because they are less costly and the economic environment in the lab can be more tightly controlled, while any specific field environment could be fairly unique.

We review the evidence for the generalizability of lab evidence to the field in many parts of this book. We end this section with the following bold claim from Camerer (2015, p. 277) made from studies where the lab and field evidence can be well matched: "There is no replicated evidence that experimental economics lab data fail to generalize to central empirical features of field data (when the lab features are deliberately closely matched to field features)." Readers interested in these issues can further consult Camerer (2015, pp. 281–5) for a list of studies that show a good