INTRODUCTION

BEGINNINGS

I am an economist interested in how people make decisions. This book is a collection of papers that try to incorporate the psychology of decision making into economic models of behavior. I am sometimes asked, usually by an economist with a worried look, how I became interested in such matters. After all, I got my Ph. D. in economics at the University of Rochester under Sherwin Rosen's guidance, and no one would call Sherwin a heretic. Afterwards I taught for a while at Rochester's Graduate School of Management, then called a University of Chicago farm club, and hardly a place to get interested in psychology. In fact, while I was there, all the behavioral scientists were fired. Luckily, I was still considered a real economist in those days. So, how and why did I go astray?

I think it all started while I was doing the research for my dissertation on the value of a human life. The idea of my thesis was that the correct measure of the value of a life-saving program is the amount people would be willing to pay to have it. I ended up measuring this by investigating how much more people got paid to work in risky occupations like mining and logging. However, as a diversion from running regressions, I decided to see what would happen if you just asked people some questions. (I don't think I told anyone—especially Sherwin—that I was going to do this.) Anyway, I asked people two questions. First, how much would you be willing to pay to eliminate a one in a thousand risk of immediate death. Second, how much would you have to be paid to willingly accept an extra one chance in a thousand of immediate death. To mitigate the effects of possible wealth and liquidity constraints on the willingness-to-pay question, I told the subjects that they could pay off their
bid over thirty years interest-free. In spite of this, the differences between the
answers to the two questions were astonishing. A typical answer was: "I
wouldn't pay more than $200, but I wouldn't accept an extra risk for $50,000!"
I came to two conclusions about these answers: (1) I better get back to running
regressions if I want to graduate; and (2) The disparity between buying and
selling prices was very interesting.

After I finished my thesis I continued to do traditional economics, but I also
became preoccupied with watching how people made decisions. What I kept
noticing was that people did not seem to behave the way they were supposed
to. I think being at the Rochester business school helped a lot because I was
surrounded by people who really took economic analyses seriously. I was
constantly confronted with the contrast between the models my colleagues
were constructing and the behavior I was so frequently observing. My
observations soon took the form of a list of "anomalous behaviors" that I posted
on a wall in my office. After a while, the list began to have categories: buying
prices much less than selling prices; paying attention to sunk costs; eliminating
options to deal with self control problems, etc. In 1976 I finally had the
courage to write some of these ideas up in a paper to which I gave the low key
title: "Consumer Theory: A Theory of Economists' Behavior." (This would
eventually turn into Chapter 1.) I showed this paper only to close friends and
to colleagues I wanted to annoy.

Though I found the ideas intriguing, I did not think that it was possible to
earn a living thinking about such things. Then I had some luck. In the summer
of 1976 I attended a conference in California on risk (more value of life stuff)
where I met Baruch Fischhoff and Paul Slovic who were then both at an outfit
called Decision Research. I ended up giving Fischhoff a ride from Monterey to
San Francisco and got to learn a bit about what he did for a living. He promised
to send me a few papers, and I promised to send him my consumer theory
paper. Among the papers in the package I got from Fischhoff was a recent
survey article in Science by two Israeli psychologists, Daniel Kahneman and
Amos Tversky. The paper was called "Judgment Under Uncertainty: Heuristics
and Biases." When I read this paper I could hardly contain myself. I still
vividly remember rushing to the library to track down the original papers.
(First I had to find the psychology section of the university library—I had
never been there before.) As I excitedly read all of these (now classic) papers I
kept thinking that somehow there was some way I could use these ideas to help
my joke paper into something serious. Indeed, there was one very important
idea in these papers that was tremendously useful, the idea that the use of
judgmental heuristics (shortcuts) would lead to systematic errors or biases.
This concept, I thought, is what was necessary to make the psychology of
decision making relevant for economics.

Fischhoff also wrote back with some nice comments on my joke paper, and
mentioned that K&T had a new paper they were working on called "Value
Theory” (the name would soon change to prospect theory). Unfortunately, he couldn’t send me a copy, since his was quite preliminary. Somehow, I did manage to get a copy from Howard Kunreuther (who was then a friend of a friend). While I had high hopes about this paper, I could never have anticipated what was in it. The theory of decision making under uncertainty that was proposed there had an S-shaped “value function” that could make sense out of many of the examples I was carrying around in my head. For example, what I was calling the “endowment effect” (buying prices less than selling prices) dropped right out of their value function which was steeper for losses than for gains. At this point I decided to try and take these ideas seriously.

I soon learned that Kahneman and Tversky were going to be visiting Stanford the following academic year. I had been planning to go to the National Bureau of Economic Research at Stanford for the summer of 1977 to work with Rosen, so I tried to arrange a way to stay longer and meet them. Thanks to the generosity of Victor Fuchs, I actually ended up staying there for fifteen months, and tried to learn as much as I could from Kahneman and Tversky. The papers in this book are all, in some ways, derived from that year in Stanford.

QUASI RATIONAL ECONOMICS

There are five basic themes that have emerged from the research activity I have been engaged in since that year in Stanford. The papers in this book are grouped according to these themes. What I will try to do here is give some indication as to why these might be considered interesting topics to investigate.

Mental Accounting and Consumer Choice

The first two sections of the book are about spending and saving, two of the three most important economic decisions individuals must make (the other being career selection). The economic theory of consumer behavior is frustratingly stark and very difficult to test. Although all economic models of consumer choice are based on rational behavior, most empirical work boils down to testing whether demand curves slope down, or more precisely, whether the sign of the substitution effect is negative. As Becker has shown, in the aggregate demand curves will slope down even if people choose at random, so long as they have binding budget constraints. What then is the economic theory of the consumer? Testing whether people equate price ratios to marginal rates of substitution is, of course, impossible, since we have no direct way of measuring preferences. Even testing for consistency, via revealed
preference theory, is not really possible since if someone did buy something they had previously turned down they might be expressing a taste for variety. The trick, then, for testing the theory is to find some prediction about behavior that is independent of preferences. Chapter 1 focuses on these types of predictions.

One example of such a prediction is derived from the economics dictum: Thou shalt ignore sunk costs. Suppose someone sits through an entire 42-3 football game in a 40 degree rainstorm simply because he paid $37 for a ticket and didn't want to waste the money. At least in this case there is an appropriate thought experiment to do. (Would he have sat through the game if he had gotten the ticket for free?) That is some progress. It is also possible, though not easy, to design an experiment to test this proposition. Take a random sample of people who agreed to pay full price for the ticket and give them back their money. Then see if they are less likely to watch the game to the end.\(^1\) But in some ways we don't really have to run such experiments. We know, just from watching people and from trying to teach our students, that the principle of ignoring sunk costs is not immediately obvious. Indeed, some have argued that the failure to ignore sunk costs substantially prolonged the Vietnam War.\(^2\)

But why do people have so much trouble ignoring sunk costs? To understand why it is necessary to introduce the concept of mental accounting. Consider this example: You have paid $120 for a fancy dinner for two at a local restaurant. You had to pay in advance, and there are no refunds. At the last minute you are invited to a friend's house for dinner where there will be an out-of-town visitor whom you would dearly love to see. If you had to choose between these two events for free you would choose the dinner with friends. Do you go to the restaurant? Most people (even economists?) would have trouble ignoring the sunk cost here. They feel that $120 is a lot to pay to eat dinner, and it is even more to pay not to eat dinner. Skipping the restaurant meal feels like wasting money, and if you go to your friend's house you will feel that you paid $120 for nothing. It is almost as if you have a debt to yourself that will linger around for a considerable time if you don't pay it off by eating the dinner. This is mental accounting at work. Chapter 2 discusses this concept in some detail.\(^3\)

Mental accounting is applied to the problem of decision making under

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\(^1\)An experiment of this type is described in footnote 8 of Chapter 1. A series of such experiments were subsequently conducted by Arkes and Blumer (1985). (Hal R. Arkes, and C. Blumer. "The Psychology of Sunk Costs." *Organizational Behavior and Human Performance* 35:124–140.)


\(^3\)The concept was introduced first in Chapter 1, but I referred to it as "psychological accounting." Kahneman and Tversky suggested the better term "mental accounting" in their 1981 paper. (Amos Tversky and Daniel Kahneman. "The Framing of Decisions and the Psychology of Choice." *Science* 211:453–463.)
uncertainty in Chapter 3. This chapter was prompted by many years of playing low stakes poker with a group of economists and business school faculty members. What I noticed over the years was that though my colleagues knew better, their betting behavior was definitely influenced by how they were doing in the poker game that evening. Players who were ahead would tend to become more reckless, feeling perhaps that they could only lose the money that they had just won. In casinos gamblers call these winnings "house money" (as opposed to "real money"). The behavior of players behind was more difficult to describe. They would become cautious about losing substantially more money (especially those who were married), but would find a bet that gave them some chance of getting back to break even very attractive. Chapter 3 documents these house money and break-even effects in more carefully controlled (but less realistic) settings.

Intertemporal Choice and Self-Control

Chapters 4 and 5 continue a theme started in Chapter 1, the economics of self-control. My interest in this issue was prompted by an incident described in Chapter 1. A group of economists had gathered at my house for dinner. While we were waiting for the food in the oven to finish cooking, I brought a large bowl of cashew nuts into the living room where people were having cocktails. In a few minutes, half the bowl of nuts was gone, and I could see that our appetites were in danger. Quickly, I seized the bowl of nuts and put it back in the kitchen (eating a few more nuts along the way, of course). When I returned, my fellow economists generally applauded my quick action, but then we followed our natural inclinations which was to try to analyze the situation to death. The burning question was: how could removing an option possibly have made us better off? After all, if we wanted to stop eating cashews, we could have done that at any time.

When I began to consider this issue more seriously, I realized that almost all the instances I could think of where people intentionally restrict their options involve intertemporal choice: choosing over time. Specifically, people restrict their current choices when they think that they will later regret them. The domains where this behavior is common include dieting (don’t keep desserts in the house), smoking (buying cigarettes by the pack), and, most important for economics, saving. Indeed, almost all the individual saving in the United States is accomplished through the use of so-called forced saving vehicles: pensions, whole life insurance, home equity, and especially social security. In Chapter 1, I suggested that to model self-control behavior it was necessary to think of the individual as having two components, a far-sighted planner, and a myopic doer. Self-control is observed when the planner somehow restricts the actions of the doer. Naturally, it seemed incongruous to work on this kind of idea alone, so I coned Hersh Shefrin, a mathematical economist who had
heretofore shown no deviant tendencies (at least as an economist), to join me. (Hersh was a quick convert. He has since written an interesting series of papers on behavioral finance with Meir Statman.) Chapter 4 represents our first attempt to model the self-control problem. Chapter 5, written several years later, makes much more use of mental accounting, and offers considerable empirical evidence to support what we call the behavioral life-cycle hypothesis.

Chapter 6 is a short piece on intertemporal choice from a different perspective. The question addressed here is whether people choose over time as if they were using a constant exponential discount function, as is normally assumed in economic analysis. I found three important deviations from a constant discount rate: (1) The discount rate is inversely related to the size of the amount being discounted (the bigger the prize, the lower the discount rate); (2) The discount rate is inversely related to the length of time being waited (the longer the wait, the lower the rate); and (3) Discount rates were much lower for negative amounts than for positive amounts. In fact, some subjects would not be willing to pay anything extra to postpone the payment of a fine.4

Experimental Economics

Demonstrating that the predictions of economic theory are false is a daunting task, especially if the usual economic data sets are used. No matter how strange a particular economic action might seem to be, some economist can usually construct a rational explanation for it. For this reason, experiments are often the most attractive domain for theory testing. Chapter 7 is a survey article written for a conference organized by Al Roth, one of the most thoughtful and creative practitioners of the experimental art. The article surveys the experimental work done to test the basic tenets of rational choice. Much of the work I survey was conducted by Kahneman and Tversky. This chapter could serve as an introduction to the more psychological literature for the uninitiated.

Chapter 8 was written directly as a result of attending Roth’s conference with Kahneman. At the conference, I presented experimental evidence on the disparity between buying and selling prices. Some of the other participants, particularly Vernon Smith and Charlie Plott, raised some objections. They argued that the existence of this effect had not yet been established. They, in effect, challenged us to demonstrate this disparity in an experimental market setting where subjects had both opportunities and incentives to learn. Kahneman and I teamed up with Jack Knetsch (who had been working on this problem for years) to see whether the endowment effect would exist under

such conditions. In fact, we found that the strength of the effect surprised even us, the true believers. Simply putting a Cornell coffee mug on the desk of a student creates an endowment effect. Students endowed with a mug demanded twice as much to sell it as other students who were not given mugs were willing to pay to get one.

Chapter 9 is a set of comments I delivered at another conference organized at the University of Chicago by Robin Hogarth and Mel Reder. My nominal task was to discuss papers by Hillel Einhorn and Robin Hogarth; Kahneman and Tversky; and Herbert Simon. The other discussant was Gary Becker, so there was one pro-rationality discussant, and one con. I chose to base my remarks on a clever article by George Stigler called “The Conference Handbook.” Stigler’s piece, in turn, is based on the old joke about the prisoners who have heard the same jokes told so often that they just call them out by number.  

Stigler points out that many seminar remarks are also a bit worn out, so it would save some time to just call them out by number. In my piece, I offer a few of the most common knee-jerk comments made by economists about the work of psychologists, as well as my own recommended responses. I also recommended the use of Wilem Hofstee’s reputational betting paradigm for settling academic disputes. Indeed, the mugs paper (Chapter 8) is essentially our response to a reputational bet. Unfortunately, I am not sure anyone is keeping score.

**Fairness**

One of the basic principles of economic theory is that, in the absence of government interference or other impediments to efficient markets, prices will adjust to eliminate shortages or surpluses. Yet, as was pointed out in Chapter 2, many markets do fail to clear. Tickets to the Super Bowl and major concerts, and dinner reservations for Saturday night at 8 at Lutece, are all priced too low. The explanation in Chapter 2 was based on the notion of transaction utility. If a customer pays more for a good than is considered normal or fair, she feels “ripped off”; the utility of the transaction per se is negative. Chapters 10 and 11, written in collaboration with Daniel Kahneman and Jack Knetsch, investigate the question of what makes a transaction seem fair or unfair. Chapter 10 concentrates on the results of a large-scale telephone survey conducted in Toronto and Vancouver. (Most of the research was conducted during the glorious year I spent visiting Kahneman at the University of British Columbia in Vancouver.) For several months we had access to a telephone polling bureau, so we ended up asking hundreds of fairness questions to groups of 100–150 respondents. There were two reasons why we

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5The punchline of the joke is this: A newcomer has the system explained to him and decides to try it himself. He calls out “221,” but no one laughs. When he asks his roommate why, his roommate tells him, “Well, I guess you just don’t know how to tell a joke.”
asked so many different questions: (1) Some theories we had about what determines the fairness of a transaction were wrong; and (2) Some questions (most actually) had alternative interpretations which could be eliminated only by asking many variations. The questions we ultimately reported were those that were most robust, which illustrated the key determinants of fairness.

Chapter 11 reports some additional results for the telephone poll plus some experimental results using a paradigm called the “ultimatum game.” One subject (the allocator) makes an offer to divide a sum of money (say, $10) between herself and another subject, the recipient. The recipient can then either accept the offer (and take what he was offered) or reject the offer, in which case both players get nothing. Our interest in this game (which had been first investigated by Werner Guth) was in the behavior of the recipients. How much money would they be willing to turn down in order to punish an allocator who was too greedy. We felt that this game captures the essence of the enforcement mechanism that induces firms to behave fairly. That is, firms that behave unfairly will be punished by their customers, who will be willing to pay a little more to take their business elsewhere.

Financial Markets

The title of this book is taken from Chapter 12, written with my long-time friend Tom Russell. In this article we investigate the conditions in which less than fully rational behavior, or quasi rational behavior as we call it, matters in competitive markets. Is it true that in competitive markets, quasi rational behavior is eliminated or rendered irrelevant? We begin the article with a Shel Silverstein poem, surely the best feature of this book, which illustrates the concept of quasi rationality. In it, a boy makes a series of trades, a dollar for two quarters, then a quarter for three dimes, etc., all based on the premise that more is better than less. Notice that such behavior is systematic, and therefore predictable. We find that the conditions for markets to eliminate quasi rational behavior of this sort are rarely observed. Indeed, even financial markets, thought by most economists to be the most efficient, leave room for quasi rational behavior to persist.

The rest of this section of the book is a series of chapters about financial markets and quasi rational behavior. I was drawn to the study of financial markets for two reasons. First, because these markets are thought by many to be so efficient, finding evidence of quasi rational behavior in this domain would be particularly telling. The other reason why financial markets are attractive to study is that the data are so good. Stock price data are available on computer tape going back to the 1920s. Actually, there is an important third

*A Rochester colleague of mine, Mike Jensen, now at the Harvard Business School, once wrote that the efficient market hypothesis was the best documented fact in social science.*
reason: Werner De Bondt, Werner was a doctoral student of mine at Cornell (he is now at the University of Wisconsin), who shared my interest in behavioral decision research and who also knew the financial economics literature. Werner soon learned how to spin tapes, so we were off and running.

Chapter 13 is derived from Werner’s thesis. It has the following history. There is a well-known anomaly in finance called the P/E effect (price/earnings). The anomaly is that for many years, going back at least to the 1930s, stocks with low price/earnings ratios have had higher rates of return than stocks with high price/earnings ratios. David Dreman, in a popular book called the New Contrarian Investment Strategy, offered an interesting theory of the P/E effect based on behavioral decision theory. Essentially, he argued that the market overreacted to the bad events that produce a low P/E and to the good events that lead to a high P/E. He cited the work of Kahneman and Tversky to support his theory. De Bondt and I found his theory plausible, and we felt that if he were right then we should be able to predict a new anomaly, namely that stocks with extreme past price performance should display mean reverting prices in the future. That is, big losers should outperform the market, and big winners should underperform the market. With considerable anticipation, Werner set about to test his idea, and to our great relief it actually worked. A portfolio of 35 big losers outperforms a portfolio of big winners by almost 40 percent over five years.

These results supporting overreaction were greeted with considerable skepticism by the financial economic community. Initially, the leading alternative hypothesis was that we had made a programming error. Fortunately, however, Werner did all the programming, and there were no mistakes discovered. At this point, our critics turned their attention to our interpretation of the results. Chapter 14 is our response to this criticism. In this paper we investigate whether the excess returns to the losers is actually a manifestation of another anomaly (equally mysterious) called the size effect. Over long periods of time, (though not in the late 80s) small firms have outperformed large firms. Since big losers have lost much of their market value in the process of becoming big losers, they have, by the standard measure of firm size, market value of equity, become smaller. Therefore, some argued, our losing firm effect was simply the size effect. We did find that the two effects are related. The small firm effect is helped by the fact that many small firms are prior losers, and losing firms are indeed smaller than average, but we concluded that both effects are present. We also investigated whether the losing firm effect can be explained by risk. Do the excess returns to the losers occur because the losing firms are especially risky? Here we found a very striking result. In the test period, after the portfolios are formed, the losing firms have high betas (the risk measure used in the standard capital asset pricing model) only in periods when the market is going up! When the market falls, the losers have betas less than one. This means that the losers go up faster
than the market when the market rises, and fall more slowly than the market when it falls. At least to us, this does not seem too risky.

Chapter 13, also written with Werner De Bondt, asks a simple question: Is there any evidence that security analysts overreact in making their predictions of earnings? We test for this by regressing the actual change in earnings for a given firm on the average change forecasted by security analysts. We find strong evidence of overreaction. For one-year forecasts, actual changes are only 65 percent of the predicted change. For two-year forecasts, there is even more overreaction.

The final paper on financial markets concerns the curious institution of closed-end mutual funds. In the case of the more common open-end fund, an investor can withdraw money from the fund at any time just by asking the fund to sell his or her shares at market value. In contrast, investors in a closed-end fund are issued shares in the fund that are traded on the exchanges. An investor who wants to sell her shares must sell them on the market at the going price. This means that the price of the shares of the fund can diverge from the value of the assets that the fund owns. Not only can the prices diverge, they do. The prices of closed-end fund shares typically sell at a discount, compared to the value of the underlying securities, though sometimes premia are also observed. I have found the closed-end fund anomaly interesting for a long time. A student at Rochester, Rex Thompson, wrote an interesting thesis on this topic while I was there. Closed end funds are interesting because they represent the only case in which it is possible to test the efficient market hypothesis prediction that prices should be equal to the intrinsic value of the security. (It is testable in this case because intrinsic value is observable, namely, the value of the assets held by the fund.)

Two events led to the paper that appears as Chapter 16. First, Charles Lee, then an accounting graduate student at Cornell, now a professor at the University of Michigan, wrote a term paper on closed-end funds for a seminar I was teaching. Then, during the seminar we read a working paper by De Long, Shleifer, Summers, and Waldmann that introduced a new model of financial markets with “noise traders” (quasi rational investors). In their paper, they discussed closed-end funds as an interesting application of the model in which the discounts and premia are determined by changes in the sentiment of noise traders. Charles and I decided to collect a data set on closed-end fund discounts and try to test this model. Soon we had the good sense to ask Andrei Shleifer to join us. (This way, if we rejected the noise trader model, he wouldn’t blame us!) One of the most interesting implications of the noise trader model is that closed-end fund discounts should be related to the prices of other securities in which noise traders are important investors. Indeed, we report in Chapter 16 that changes in the average discount on closed-end funds helps explain the magnitude of the small firm effect mentioned above. Specifically, in months when the average discount on closed-end funds narrow, small firms have higher excess returns. In the paper we also examine
several standard explanations for closed-end fund discounts and premia and find them lacking.

CONCLUSIONS

I would summarize what I have learned so far this way: Quasi rational behavior exists, and it matters. In some well-defined situations, people make decisions that are systematically and substantively different from those predicted by the standard economic model. Quasi rational behavior can be observed under careful laboratory controls and in natural economic settings such as the stock market. Market economies and their institutions are different from the way they would be if everyone were completely rational.

How should economists react to this? First let me suggest what I think is the wrong answer. Once, after a talk I gave at a meeting of economists, I was asked a question by a member of the audience, a respected macroeconomic theorist. He said: “If I take what you say seriously, what am I supposed to do? I only know how to solve optimization problems!” Am I suggesting that economic theorists fold up their tents and go home to make room for psychoeconomists? Just the opposite. I think that there is much to do, and only economists have the tools to do it.

Some of the work that needs to be done is theoretical. What happens in a standard economic model if we relax the assumption that everyone is rational all of the time? Suppose some of the people are rational and some are not, as in Chapter 12. The recent series of papers by De Long, Shleifer, Summers, and Waldmann on “noise traders” is in this spirit. They show that the economists’ intuition that irrational investors will automatically go broke is incorrect. In some situations, the irrational investors actually end up with more wealth. Dumb and rich can happen. A similar thrust is coming in game theory. Experiments have shown that real people do not play games according to the predictions of game theory. In complex games, people use simplifying strategies, and they care about acting fairly and being treated fairly. These factors need to be incorporated into new game theoretic models. Only economists can construct such models. And then there is macroeconomics. Over the postwar period a new criterion emerged as a basis of evaluating macroeconomic models: The more rational the agents in a model are assumed to be, the better. It is time to recognize that there can be too much of a good thing, even rationality. Perhaps we can construct better macroeconomic models by recognizing that the agents in the economy are human. They overwithhold on their income tax in order to get a refund. Then they treat the refund as a windfall. They have positive balances in their savings accounts earning 5 percent and outstanding balances on their credit card for which they pay 18 percent. Where are these people in macroeconomic models?

In many cases, new theory must be done in conjunction with empirical
work, especially experimental research. One of the most important questions to be addressed is learning. I am talking about real learning here, not optimal learning. How do people really learn about the world around them? It is true that most of us do tolerably well at learning how to read and drive a car. Some people can even learn to play the piano or cook a soufflé. But this is all easy compared to choosing a mortgage, much less a career. How do people really search for a job when they are unemployed? Do those who experience frequent spells of unemployment search more optimally? How do firms learn about the elasticity of demand for one of their products? I simply want to make the following claim: It is impossible to build descriptive models of learning without watching people learn.

So, progress has been made, there is lots to do. If you read and enjoy this book, I hope you will pitch in and help. There is more than enough work to go around.