Journal of Economic Perspectives—Volume 14, Number 1—Winter 2000—Pages 133–141

From Homo Economicus to Homo Sapiens

Richard H. Thaler

Responding to a request for a forecast is especially tricky for someone like me, who specializes in other people's biases. Research in psychology suggests that certain biases are very likely to creep into my forecasts about the future of economics (or anything else).

1. Optimism (and wishful thinking). We all tend to be optimistic about the future. On the first day of my MBA class on decision-making at the University of Chicago, every single student expects to get an above-the-median grade, yet half are inevitably disappointed. This optimism will induce me to predict that economics will become more like I want it to be.

2. Overconfidence. In a related phenomenon, people believe they are better forecasters than they really are. Ask people for 90 percent confidence limits for the estimates of various general knowledge questions and the correct answers will lie within the limits less than 70 percent of the time. Overconfidence will induce me to make forecasts that are bolder than they should be.

3. The False Consensus Effect. We tend to think others are just like us. My colleague, George Wu, asked his students two questions: Do you have a cell phone? What percentage of the class has a cell phone? Cell phone owners thought 65 percent of the class had mobile phones, while the immobile phoners thought only 40 percent did. (The right answer was about halfway in between.) The false consensus effect will trap me into thinking that other economists will agree with me—20 years of contrary evidence notwithstanding.

4. The Curse of Knowledge. Once we know something, we can't imagine ever thinking otherwise. This makes it hard for us to realize that what we know may be less than obvious to others who are less informed. The curse of knowledge will lead

■ *Richard H. Thaler is the Robert P. Gwinn Professor of Economics and Behavioral Science, Graduate School of Business, University of Chicago, Chicago, Illinois.*

me to think that others will have read the same articles I have, and have learned the same lessons from them (lessons I now take for granted), when in fact others have been busy reading entirely different material, and have never even heard of the findings that have so influenced my thinking.

In making some forecasts for the future of economics, it would be embarrassing to commit (in writing) all the mistakes I spend weeks warning my students to avoid. However, the alternatives are not very attractive, either. Rationally, I realize that the forecast most likely to be right is to predict that economics will hardly change at all. (Have I mentioned status quo bias?) Although such a forecast has the virtue of brevity, it would not make very interesting reading (or writing). So, with trepidation, I am going to make six bold predictions about how economics will develop over the next couple decades, forecasts that are guaranteed to contain every bias mentioned above, as well as some others. You have been warned.

Homo Economicus Will Begin Losing IQ, Reversing a 50-year Trend

Economics in the first half of the 20th century was much more of a social science. Writers such as Irving Fisher and John Maynard Keynes stressed psychological factors in their explanations of economic behavior (Loewenstein, 1992). With the mathematical revolution that began to take off in the 1940s with the likes of John Hicks and Paul Samuelson, economic agents began to be more explicitly optimizing. In the 1950s, economists who began formalizing the micro foundations of Keynes developed more rational models; for example, compare Keynes's simple consumption function with the life-cycle hypothesis, and then with the rational expectations hypothesis of Muth, Lucas, and so on. Eventually the models came to include agents that detractors called "hyperrational." The aesthetic in the field became that if the agents in model A are smarter than the agents in Model B, then Model A is better than Model B. The IQ of Homo Economicus became bounded only by the IQ of the smartest economic theorist!

My prediction is that this trend will be reversed in favor of an approach in which the degree of rationality bestowed to the agents depends on the context being studied. To illustrate how this can work in practice, consider the "guess the number" game first studied by Rosemarie Nagel (1995). In this game, contestants are told to guess a number from 0 to 100, with the goal of making their guess as close as possible to two-thirds of the average guess. In a world where all the players are known to be fully rational, in the sense that they will form expectations about the guesses of others can carry out as many levels of deduction as necessary, the equilibrium in this game is zero.

In any other setting, however, guessing zero is not a good strategy. Recently, I had the opportunity to play this game for quite large stakes (Thaler, 1997). At my request, the *Financial Times* ran a "guess the number" game contest using the rules

described above and offered two business class tickets from London to the United States as a prize (worth over $10,000). Only integer guesses were permitted. Although many contestants did guess zero or one, the most popular guesses were 33 (the right guess if everyone else chooses a number at random) and 22 (the right guess if everyone else picks 33). The average guess was 18.91 and thus the winning guess was 13. Although modeling how this game is actually played is not easy, some lessons are clear enough. An appropriate model would have to allow for two kinds of heterogeneity in sophistication. First, agents differ in how many levels of processing they engage in (33 is one level, 22 is two levels, and so on). Second, there is heterogeneity in how much agents think about the behavior of other agents. Agents who guess zero are sophisticated on the first dimension and naïve on the second. Many economists fall into this category (due in part to the False Consensus Effect and the Curse of Knowledge!) Sophisticated economic models will have agents that are both more and less sophisticated than the agents we are used to modeling. I predict this sort of modeling will be the norm in the future.

Homo Economicus Will Become a Slower Learner

Most economic models have no reason to introduce learning because agents are assumed to solve the relevant problem correctly on trial one. When learning is explicitly introduced, Homo Economicus (hereafter abbreviated HE, with no gender inference intended) is typically taken to be a quick study. If, perchance, HE makes an error, HE quickly learns to correct it. However, the students I have taught over the years, even at our best universities such as Cornell, MIT, and Chicago, are a little slower on the uptake. Even after hearing what is, to my unbiased view, a completely clear explanation, they still often make a mistake in applying a concept if the context is slightly disguised. This is why putting a question about the first part of the course on an exam covering the later part of the course is considered so unfair by the students.

The problem with many economic models of learning is that they seem to apply to a very static environment. In fact, such models seem to be directly applicable only to the situation in which Bill Murray finds himself in the movie *Ground Hog Day*.[1] In that movie, Bill Murray is a TV weatherman sent to report on whether the groundhog sees his shadow on Feb. 2. Murray's character ends up reliving the same day over and over again. Although he is a slow learner, the opportunity to rerun the same day repeatedly, and to learn from the consequences of his actions each time, creates a controlled experiment in which he is able to learn many things eventually, from how to prevent accidents to how to play the piano. Alas, life is not like *Ground Hog Day*. In life, each day is different, and the

[1] The idea that economic models of learning are similar to this movie evolved during a conversation I had with Colin Camerer during a Russell Sage Foundation summer institute on behavioral economics. It is a safe bet that we each think it was our idea.

most important of life's decisions, such as choosing a career or spouse, offer only a few chances for learning! I predict that economic models of learning will become more sophisticated by making their agents less sophisticated and giving greater weight to the role of environmental factors, such as the difficulty of the task and the frequency of feedback, in determining the speed of learning. This means that models of saving for retirement (a hard problem with few opportunities for learning) should be very different from models of frequency of milk purchases (easier, with many learning chances).

The Species Populating Economics Models Will Become More Heterogeneous

Although you can get the wrong impression reading economics textbooks and journal articles, most economists are happy to admit they know many people whose reasoning is quite flawed: their spouses, children, students, colleagues, deans, college presidents, and so on. When pressed on why it is reasonable to base economic models exclusively on rational representative agents, while at the same time thinking that most of the people they interact with are at least occasionally bozos, typically some kind of evolution plus markets argument is offered. The argument proceeds something like this. Suppose there were some less-than-fully-rational agents. I like to call them "quasi-rational," meaning trying hard but subject to systematic error. Once these quasi-rationals started interacting with rational types, the rationals would quickly take all their money, after which the quasi's would either learn or would be rendered economically irrelevant. Rarely is this argument spelled out carefully, and for good reason: It is false!

When rational agents interact with quasi-rational agents, the rational agents cannot be expected either to take all the quasi's money, or to set prices unilaterally. Indeed, careful analyses of such situations in financial markets, such as those by De Long et al. (1990), show that it is possible for the quasi's—called "noise traders" in finance circles—to end up richer than their rational counterparts (by inadvertently bearing more risk). Although papers mixing rational and quasi-rational agents have become popular over the past decade or so, it is still considered a novelty to have some quasi's in the model. In workshops, presenters of such models still feel compelled to explain why they need to have these quasi's mucking things up. My prediction is that in future seminars presenters will have to explain why they are using a model with *only* rational agents (unless the paper is on the history of economic thought). After all, analyses of market interactions between agents of various types is exactly what differentiates economics from other social sciences. Psychologists, sociologists and anthropologists might help us improve our characterizations of economic behavior, but economists are the only social scientists with the tools to analyze what happens in market contexts.

Note that I am not predicting that HE will disappear from economics research.

At least two roles should remain. First, many aspects of the standard HE model are useful as theoretical special cases, much as perfect competition is used today. Second, when a few highly trained special agents can influence markets, as in financial markets, they can be usefully modeled as HEs, especially in models with heterogeneous agents.

Economists Will Study Human Cognition

One way of modeling bounded rationality is to reduce the information-processing capabilities of the agents; for example, in the case of the numbers game discussed earlier, by assuming that individuals will only do two steps of backward induction rather than infinite steps. This is a sensible initial approach, but we can do more that make HE dumber. A more interesting research agenda is to attempt richer characterizations of economic agents via a better understanding of human cognition. This, I predict, will be a major area of effort over the next two decades. Some successful examples published in the last 20 years prove that this kind of work is both feasible and useful.

The most significant exemplar is the "prospect theory" of Daniel Kahneman and Amos Tversky (1979). This positive theory of decision-making under uncertainty manages to capture an enormous amount of psychological wisdom in its S-shaped "value function." The value function shows changes in material well-being on the horizontal axis, rather than levels as in expected utility theory, because humans (and other species) have a strong tendency to adapt to their environment and react only to perceived changes. The vertical axis shows happiness resulting from these changes. The S-shape displays diminishing marginal sensitivity to both gains and losses, a basic finding in the psychology of perception (psychophysics). Finally, the loss function is steeper than the gain function, a property that has come to be known as loss aversion. Losses hurt about twice as much as gains make us feel good. These three psychological concepts yield plenty of explanatory power, having been used to explain as diverse phenomena as consumers reaction to price changes in the supermarket to the labor supply behavior of cab drivers (Camerer, forthcoming).

There are an enormous number of exciting ways in which a better understanding of human cognition could help us do better economics. I'll suggest two here. First, there is a problem with prospect theory that cognitive psychology might help us fix; namely, the theory is incomplete. Prospect theory tells us that choices depend on the framing of a problem, but does not tell us how people will spontaneously create their own frames. By directly studying how people attack decision-making problems, we may learn more about this problem-editing process.[2] Second, though we have given considerable attention in recent years to the impli-

[2] Some of what we know about this problem falls into the category of "mental accounting." For a current review of this literature, see Thaler (1999).

cations of bounded rationality, we have spent less time studying the impact of bounded memories. A simple example is "hindsight bias": after the fact, events that happen are thought to have been predictable. For example, one year in my class I asked my students on the first day of class (in late January) to make predictions about stock market returns for the next two months. Their forecasts were bearish: they thought it was more likely that the market would go down than up. Two months later I asked them to try to recall their earlier forecast. They remembered being bullish. Needless to say, the market rose sharply over this two month period.

This phenomenon (related to the curse of knowledge mentioned earlier) is both strong and robust, and has powerful implications for economics. Consider, for example, the role of hindsight bias in agency problems. A principal with a biased memory (that is, any real world principal) will find it very difficult to distinguish between a bad decision and a bad outcome, since an unlucky exogenous event will be thought, in hindsight, to have been predictable. Agency theory with absent-minded principals (and agents) would be an exciting field of inquiry.[3]

Economists Will Distinguish Between Normative and Descriptive Theories

Psychologists distinguish between two kinds of theories: normative and descriptive. To them, normative theories characterize rational choice: examples would include the axioms of expected utility theory and Bayes' rule. Descriptive theories try to characterize actual choices. Prospect theory is an example of a descriptive theory. Agents who choose according to prospect theory violate fundamental axioms of rational choice; for example, under certain circumstances they will choose option A over B even when B dominates A, as long as the dominance is not too obvious.

I would not want to call such choices rational, but since people do choose them in real life, high stakes situations, it is important that economists develop models that predict such behavior. Economists have traditionally used one theory to serve both the normative and descriptive purposes. Expected utility theory and the life-cycle theory of saving are rational (normative) models that economists have used also as descriptive models. Occasionally economists have proposed explicitly descriptive theories, such as William Baumol's (1967) theory of the firm in which managers maximize sales subject to a profit constraint. However, such descriptive theories have not won great acceptance. Part of the resistance to such theories, I think, has been based on a misunderstanding of the issues raised above regarding how competition will force changes in quasi-rational behavior. For example, Baumol often heard the critique that firms that maximized sales would inevitably lose market share to competing firms that were trying to maximize profits. The self-

[3] For a clever example of what absent-minded economics might look like, see Mullainathon (1999).

contradictory nature of this critique—that maximizing sales would cause lower market share—did not seem to bother (or occur to) its adherents. There is, of course, a perfectly good equilibrium in which some firms are willing to accept lower profits in order to be bigger, and such firms take market share away from profit-maximizing competitors, not vice versa. Similarly, if a baseball team owner chooses to buy a World Series victory at the expense of profits, the profit-maximizing owners of other teams can do little except become even richer losers.

One additional point about descriptive theories: they are, of necessity, driven by data. Baumol's sales maximization hypothesis was suggested to him by conversations with managers. Kahneman and Tversky's prospect theory was derived by examining hundreds of choices between pairs of gambles. Some economists seem to feel that data-driven theory is, somehow, unscientific. Of course, just the opposite is true. Copernicus watched the movements of the planets before devising his theory that the planets orbit the sun. What makes for a good descriptive theory is out-of-sample tests; for example, the prediction that Pluto would be discovered before telescopes were good enough to see it. So, this prediction leads to an auxiliary prediction that more theorists will pay attention to data.

Homo Economicus Will Become More Emotional

The predictions I have made so far, though fraught with the biases I identified early on, are still somewhat conservative in the sense that lots of good work is already going on in the directions I suggest that the field will be headed. So, it seems right to offer the slightly more courageous prediction that Homo Economicus will become more emotional, by which I mean that economists will devote more attention to the study of emotions.

To get a sense of what the study of emotions entails, I refer readers to Jon Elster's (1998) recent article. Although Elster does not define emotions explicitly, he does offer a list of states that he says are unambiguously emotions, of which a subset are: anger, hatred, guilt, shame, pride, liking, regret, joy, grief, envy, malice, indignation, jealousy, contempt, disgust, fear, and, oh yes, love. Elster distinguishes this list from other "visceral factors" (a more general term, see Loewenstein, 1996) such as pain, hunger, and drowsiness, in that they are triggered by beliefs. Many of these emotions are often accompanied by states of physiological arousal, like fear.

How can emotions be incorporated into economic analyses? The ultimatum game offers one simple example. In the ultimatum game one player, the Proposer, is given a sum of money, say $10, and makes an offer of some portion of the money, x, to the other player, the Responder. The Responder can either accept the offer, in which case the Responder gets x and the Proposer gets $10-x, or reject the offer in which case both players get nothing. Experimental results reveal that very low offers (less than 20 percent of the pie) are often rejected. Speaking very generally, one can say that Responders react emotionally to very low offers. We might get more specific and say they react indignantly. What is certain is that Responders do

not act to maximize their own payoffs, since they turn down offers in which they receive a small share of the pie and take zero instead. Matthew Rabin's (1993) model of fairness, which is an attempt to explain such behavior (specifically, the resisting of unfair offers) is based partly on emotions.

Rejecting a positive offer in the ultimatum game is spiteful; it hurts both parties. Unfortunately, such behavior is more common than economic theorizing would lead us to expect. I need only mention the word "divorce" to bring to mind all-too-many familiar examples. Spite is not confined to ex-spouses. The Coase theorem prediction that the allocation of resources is independent of the assignment of property rights depends on the willingness of the parties to a lawsuit to recontract. However, recontracting requires interaction that can be made difficult by spite. In a recent study of this issue, Ward Farnsworth (1999) interviewed attorneys from over 20 nuisance cases in which injunctive relief was sought and either granted or denied after full litigation before a judge. In not a single case did the parties even attempt to contract around the court order.

Conclusion

My predictions can be summarized quite easily: I am predicting that Homo Economicus will evolve into Homo Sapiens. This prediction shouldn't be an outlandish one. It seems logical that basing descriptive economic models on more realistic conceptions of economic agents is bound to increase the explanatory power of the models. Still, a conservative economist might (emotionally) scoff: "If this were a better way of doing economics, we would already be doing it that way!" Why aren't all my predictions already true? And why should I expect things to change?

One reason economics did not start out this way is that behavioral models are harder than traditional models. Building models of rational, unemotional agents is easier than building models of quasi-rational emotional humans. Nonetheless, each generation of scientists builds on the work of the previous generation. Theorems too hard to prove 20 years ago are found in graduate student problem sets today. As economists become more sophisticated, their ability to incorporate the findings of other disciplines such as psychology improves. Simultaneously, we can hope that new scholars in other disciplines can do for economics what cognitive psychologists such as Kahneman and Tversky have already done: offer us useful findings and theories that are relatively easy to incorporate into economic models.

I will close with a very safe prediction. If some of my predictions about the future of economics come true, young economists will have done the work. (Old economists, like me, can't learn new tricks any better than dogs.) A few of these young economists are already on the horizon. Others will follow.

■ *The author would like to thank Colin Camerer, Peter Diamond, George Loewenstein, Jesus Santos, Andrei Shleifer, Cass Sunstein and all the editors for helpful comments.*

References

Baumol, William. 1967. *Business Behavior, Value and Growth.* New York: Harcourt Brace.

Camerer, Colin. forthcoming. "Prospect Theory in the Wild." In *Choices, Values, Frames.* Kahneman, Daniel and Amos Tversky, eds. New York: Cambridge University Press.

De Long, J. Bradford, Andrei Shleifer, Lawrence Summers, and Robert Waldmann. 1990. "Noise Trader Risk in Financial Markets." *Journal of Political Economy.* 98, pp. 703–38.

Elster, Jon. 1998. "Emotions and Economic Theory." *Journal of Economic Literature.* March, XXXVI, pp. 47–74.

Farnsworth, Ward. 1999. "Do Parties to Nuisance Cases Bargain After Judgement? A Glimpse of the Cathedral." *University of Chicago Law Review*, 66, pp. 373–436.

Kahneman, Daniel and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica.* 47:2, pp. 263–91.

Loewenstein, George. 1992. "The Fall and Rise of Psychological Explanations in the Economics of Intertemporal Choice," in *Choice Over Time.* Loewenstein, George and Jon Elster, ed. New York: Russell Sage Foundation. pp. 3–34.

Loewenstein, George. 1996. "Out of Control: Visceral Influences on Behavior." *Organizational Behavior and Human Decision Processes.* March, 65:3, pp. 805–24.

Mullainathon, Sendhil. 1999. "A Memory Based Model of Bounded Rationality," working paper, MIT Economics Department.

Nagel, Rosemarie. 1995. "Unraveling in Guessing Games: An Experimental Study." *American Economic Review.* 85:5, pp. 1313–1326.

Rabin, Matthew. 1993. "Incorporating Fairness into Game Theory and Economics." *American Economic Review.* December, 83, pp. 1281–1302.

Thaler, Richard H. 1997. "Giving Markets a Human Dimension." *Financial Times.* 6, June 16, 1997.

Thaler, Richard H. 1999. "Mental Accounting Matters." *Journal of Behavioral Decision-making.* 12, pp. 183–206.