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Behavioral economics research and the foundations of economics☆

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Abstract

Five propositions on which economists and psychologists including behavioral economists are in agreement are presented, leading to a discussion about two kinds of rationality. After some comments on methodology and on concepts of fairness, I will discuss the question of wealth maximization versus the economics of survival, and their different implications for behavior.

I want to begin with five propositions on which economists and psychologists including behavioral economists are in agreement.

This will lead me to talk about two kinds of rationality.

After some comments on methodology and on concepts of fairness, I will close on the question of wealth maximization versus the economics of survival, and their different implications for behavior.

Keywords: Behavioral economics; Economist; Rationality

1. Propositions

Curiously, the image of economists and psychologists as protagonists obscures their underlying agreement on foundations. Both rely upon the same underlying interpretation of economic rationality as follows:

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(1) *To the extent that markets are rational, as in controlled S & D experiments or irrational, as in experimental asset bubble markets, this derives directly from the rationality or irrationality of agents*.

Thus, even a ". . . monopolist . . . has to have a full general equilibrium model of the economy" (Arrow, 1987, p. 207). The default explanation for market rationality is assumed to derive entirely from individual rationality. Markets cannot be rational if agents are not fully rational in the particular sense in which theorists have modeled it, and this proposition has been uncritically accepted by psychologists and behavioral economists.

(2) *Individual rationality is a self-aware, calculating process of maximization*.

Here is a particularly clear statement of decision as deliberate intentional action: "Incentives do not operate by magic: they work by focusing attention and by prolonged deliberation" (Tversky and Kahenman, 1987, p. 90). But this is an inference based on how the theorist conceptualizes and models decision problems, not the subject. The inference ignores the demonstrated capacity of motivated subjects to find equilibrium outcomes by repeat interaction in market experiments without cognitive awareness of this capacity, and their capability of achieving better outcomes in two-person anonymous trust interactions than if they applied traditional game-theoretic principles.

(3) *Predominantly both economists and psychologists are reluctant to allow that naïve and unsophisticated agents can achieve socially optimal ends without a comprehensive understanding of the whole, as well as their individual parts, implemented by deliberate action*.

People are not supposed to achieve desirable outcomes, and not be able to tell you about it. There is no 'agic'; no room for the Gode and Sundar zero intelligence traders who in simple environments reveal that at least some of the rationality in markets is encapsulated in the institution; no room for the results of hundreds of single and multiple commodity market supply and demand experiments in which subjects with private information on values and costs achieve unintended equilibrium predicted outcomes. The phenomenon, not being constructively explicable with the tools of standard theory, is in effect denied: if subjects achieve rational outcomes it is because in effect they think about decision problems as we economists do.

(4) *Consequently, psychologists test the rationality of individual decisions largely by asking for subject responses to choice problems to discover how they 'reason.'*

Rather than challenge this interpretation, economists, subject to the identical vision—how do agents consciously think about economic choice?—are critical of the question–response survey methods used in cognitive psychology: the stakes are zero or too low, and it is claimed without citing the experimental evidence on stakes, that the subjects are too unsophisticated, inexperienced or untrained to allow a serious researcher to find out how 'real agents really think.' Many psychologists appear to find irrationality everywhere, and many economists appear to see the findings as everywhere irrelevant. To both, how agents think indeed exhausts the core of investigation.

(5) *Finally, psychologists and economic theorists have identified rationality almost entirely as expected utility (including expected profit, or wealth) maximization.*

But is this the concept of rationality that best explains either behavior or what agents seek as optimal for themselves? In my closing remarks I will discuss a particularly appealing alternative: the theory of economic survival developed by Roy Radner and his coworkers.

1.1. Opinion surveys

In point of fact opinion surveys can provide important insights. Survey findings can be tested more rigorously with reward motivated choices and some have predictive content, e.g., the choice asymmetry between losses and gains in wealth. But sometimes what people actually do completely contradicts what they say, and sometimes you cannot find out by asking because the agents themselves do not know what they will do or are doing. Here are some examples:

(1) Comparisons of risk preferences under low and high monetary stakes have shown that actual reward levels do have a statistically significant effect on decision, but that the qualitative conclusions from hypothetical choice response surveys are not refuted by studies using very high stakes—the accumulated payoffs average three times subjects' normal monthly living expenses (Kachelmeier and Shehata, 1992; also see Binswanger, 1980 for similar findings). These findings have been cited defensively with the argument that payoffs do not change the conclusions from hypothetical choice studies. Yes, and the scholars in question do not claim otherwise, but until their work, we did not know; now we do; it could have gone the other way for all anyone could say; in fact observed outcomes *are* effected, so indeed payoffs do make a difference, but not enough to rescue expected utility theory from the qualitative results in previous contradictions. In other contexts, payoff levels have sometimes, and sometimes not, created important shifts in observations (Holt and Laury, 2002). The correct conclusion is that you better do some testing using incentive rewards, because it can matter. The wrong conclusion is that the results from unmotivated choice surveys generalize to motivated choice.
(2) Consider the double auction in classroom demonstration experiments: in debriefings afterwards students deny that there is any kind of quantitative model that could predict their market price and exchange volume, or that they were able to maximize their profits; but a participant with a sealed envelope containing the predictions provided in advance, opens it to reveal that this consensus is false. The dispersed private value/cost information is aggregated into prices that are at the equilibrium and each agent is maximizing his or her profit given the behavior of all others. Here there is indeed a kind of 'magic', but only, I think, in not being well-understood or modeled at the game-theoretic level of individual choice. In this sense our bounded rationality as economic theorists is far more constraining on economic science, than the bounded rationality of privately informed agents is constraining on their ability to maximize the gains from exchange in markets.
(3) In asset trading, participant survey responses reflect the disparity between their information on fundamental value and their puzzling experience of a price bubble and crash

generated on the long path to the rational expectations equilibrium in a stochastically stationary environment (Schwartz and Ang, 1989).

(4) Opinion polls administered to the Iowa Electronic Market traders show the same judgment biases that psychologists and political scientists find in public opinion polls, but these biases do not interfere where the market is able to predict the popular vote outcomes (Forsythe et al, 1992).

(5) In preference reversal survey experiments subjects report many inconsistent choices: gamble A is preferred to B but a subject will sell A for less than B. Arbitraging the subjects' cash motivated choices quickly reduces these inconsistencies (Chu and Chu, 1990, p. 906), and it has been shown that the inconsistencies are unbiased random errors under some, but not all, conditions (Cox and Grether, 1996; also see Soper and Gigiolotti, 1993, where choice intransitivity is studied directly and the errors are found to be random). But reducing inconsistency through experimental treatments, while of interest, does not mean that the subjects will generalize that 'learning' to other contexts.

In these examples it is in private information environments, where the market is aggregating information far beyond the reach of what each individual knows, understands and is able to comprehend, that the solicited opinions are so far off the mark. The surveys yield no useful understanding because the subjects have none to relate. In the complete information asset market, subjects are aware of its fundamental value structure, and come to have common expectations through an experiential process of repetition; i.e. initial common information is not sufficient to induce common expectations. They trade myopically and their expressed bafflement ('prices rise without cause') reflects this myopia. These observations suggest that much insight might be obtained from the systematic comparison of the conditions under which survey results are robustly informative and the conditions under which they are not.

2. Two kinds of rationality

I think the agreement between economists and psychologists on the constitution of rationality in the above propositions is wrong. My views have been shaped by the discoveries of experimental economists in both impersonal market exchange and personal social exchange. I want to argue that the key to understanding our data is to be found in recognizing two distinct conceptions and sources of rational behavior: constructivist and ecological. This distinction grew out of my experience in the laboratory in which time and again subjects produced results that were not part of their conscious intentions.

2.1. Constructivist rationality

The first concept of a rational order derives from the standard socioeconomic science models (SSSM) going back to the seventeenth century. The SSSM is an example of what Hayek has called 'constructivism', which stems particularly from Descartes and others, who believed and argued that all worthwhile social institutions were and should be created

by conscious deductive processes of human reason. In the 19th century Bentham and John Stuart Mill were among the leading constructivists.

As Hayek put it, the constructivist view of rationality "... gives us a sense of unlimited power to realize our wishes ... (and) ... holds that human institutions will serve human purposes only if they have been deliberately designed for these purposes ... (moreover, an institution's existence) ... is evidence of its having been created for a purpose, and always that we should so re-design society and its institutions that all our actions will be guided by known purposes ... This view is rooted ... in a ... propensity of primitive thought to interpret all regularity ... as the result of the design of a thinking mind ..." (Hayek, 1973, pp. 8–9).

In economics the SSSM leads to rational predictive models of decision that motivate research hypotheses that experimentalists have been testing in the laboratory since mid-twentieth century. Although the test results tend to be confirming in impersonal market exchange, the results are famously and recalcitrantly mixed in personal exchange, notably in a great variety of two person extensive form games where some half of the people attempt and frequently succeed when risking cooperation, even when anonymously paired.

Behavioral economics has made a cottage industry of showing that the SSSM assumptions seem to apply almost nowhere to decision making. This is because the methodology of behavioral economists unabashedly champions a deliberate search for "identifying the ways in which behavior differs from the standard model ..." (Mullainathan and Thaler, 2001). This search must necessarily succeed for it includes search in the tails of distributions, and this inappropriate methodology explains why there is so much variation in the degree of similarity in the findings of experimental and behavioral economists.

Thus, there is agreement between EE and BE that in certain standard contexts the SSSM fails to predict the high levels of cooperation in ultimatum and dictator games. But in exploring and trying to understand such behavior EE has discovered conditions under which cooperation in these games are greatly reduced or become inconsequential. For example, ultimatum game proposals vary from 44% to 28% depending upon whether the task is formulated as a market exchange, and whether a pre game exercise is used to earn the first mover position.

Similarly, in dictator games the percentage of the pie given by dictators falls from 23% to 10.5% when conducted double blind, and further to only 3% when dictators must first earn their stakes of $40 (essentially, the $40 becomes their "own money" before it can be given—a condition believed by some behavioral economists to be important). Alternatively the percentage given rises from 23% to 52% as in the two stage investment trust game, with gains from exchange—the amounts sent are tripled and any amount can be returned. We now understand that giving can be very self-interested as in the SSSM if social distance is great and subjects have to earn their stakes perhaps creating an "endowment effect" (Smith, 2004, Tables 4 and 5).

The results from some of these games have motivated constructivist extensions of game theory based on other-regarding, in addition to own-regarding, preferences (e.g. Bolton, 1991; Rabin, 1993), but these models perform poorly in competition with exchange models when applied to wider classes of games and are inconsistent with the above very large variation in context, stakes and anonymity.

Moreover there now exists a large data set from trust games in which subjects achieve cooperative outcomes in single play, and make more money than if they applied game-theoretic principles, contrary to both the predictions of game theory and the behavioral economics search for irrational behavior.

An alternative explanation of some of the contradictions to theory is that people may use ancient social-grown norms of reciprocity[7] (including equity, meaning to each according to his justly earned desert; i.e., equality of opportunity, not outcome) to achieve cooperative states superior to individually rational defection outcomes. Technically, the issue can be posed as one of asking how most productively to model agent 'types' by extending game theory so that types are an integral part of its *predictive* content, rather than merely imported as an ex post technical explanation of experimental results. For example, moves can signal types, and effect decision, which explains why game-form matters, and why opportunity cost—payoffs available, but foregone—can effect outcomes. These considerations must be part of the internal structure of the theory such that outcomes become predictions conditional on the elementary characteristics of players who read each other's intentions. If such a program were successful, many of the basic results in game theory would become irrelevant or special cases of the extended theory.

In market experiments—where cooperation can occur through the coordination function of prices produced by, but simultaneously resulting from, interaction with individual choice behavior—the results are more commonly in accord with standard competitive models that maximize group welfare. This cannot be claimed as a professional economic victory, however, because of the complete failure of the SSSM to predict the 'surprisingly' weak conditions under which the results obtain.

Thus, for tractability, Cartesian rationalism provisionally assumes or 'requires' agents to possess complete payoff and other information—far more than could ever be given to one mind. In economic analysis the resulting exercises are believed to sharpen economic thinking, as if-then parables. Yet, these assumptions about the economic environment are unlikely to approximate the level of ignorance that has conditioned either individual behavior, or our evolved institutions as abstract norms or rules independent of particular parameters, which have survived as part of the world of experience. The temptation is to ignore this reality because it is poorly understood, and does not yield to our familiar but inadequate modeling tools, and to proceed in the implicit belief that our parables capture what is most essential about what we observe. Having sharpened our understanding on Cartesian complete information parables we carry these tools into the world for application without all the necessary caveats that reflect the tractability constraints imposed by our bounded professional cognitive capacities as theorists.

In summary, constructivism uses reason to deliberately create rules of action, and design human socioeconomic institutions that yield outcomes deemed preferable, given particular circumstances, to those produced by alternative arrangements. Although constructivism is one of the crowning achievements of the human intellect, it is important to remain sensitive to the fact that human institutions and most decision making is not guided only or even primarily by constructivism. Emergent arrangements, even if initially constructivist in form must have survival properties that take account of opportunity costs and environmental challenges invisible to our modeling efforts.

2.2. Limitations and distractions of constructivist rationality

Since our theories and thought processes about social systems involve the conscious and deliberate use of reason, it is necessary to constantly remind ourselves that human activity is diffused and dominated by unconscious, autonomic, neuropsychological systems that enable people to function effectively without always calling upon the brain's scarcest resource—attention and reasoning circuitry. This is an important economizing property of how the brain works. If it were otherwise, no one could get through the day under the burden of the self-conscious monitoring and planning of every trivial action in detail. Thus, "If we stopped doing everything for which we do not know the reason, or for which we cannot provide a justification . . . we would probably soon be dead" Hayek (1988, p. 68). No one can express in thoughts, let alone words, all that he or she knows, and does not know but might call upon, or need to discover, for some purposive action. Imagine the strain on the brain's resources if at the supermarket a shopper were required to explicitly evaluate his preferences for every combination of the tens of thousands of grocery items that are feasible for a given budget.

Such mental processes are enormously opportunity-costly and implicitly our brain knows, if our conscious, planning, modeling mind does not know, that we must avoid incurring opportunity costs that are not worth the benefit. The challenge of any unfamiliar action or problem appears first to trigger a search by the brain to bring to the conscious mind what one knows that is related to the decision context. Context triggers autobiographic experiential memory, which explains why context surfaces as a nontrivial treatment, particularly in small group experiments.

We fail utterly to possess natural mechanisms for reminding ourselves of the brain's offline activities and accomplishments. This important proposition has led Gazzaniga (1998) to ask why the brain fools the mind into believing it is in control: "By the time we think we know something—it is part of our conscious experience—the brain has already done its work. It is old news to the brain, but fresh to 'us.' Systems built into the brain do their work automatically and largely outside of our conscious awareness. The brain finishes the work half a second before the information it processes reaches our consciousness . . . We are clueless about how all this works and gets effected. We don't plan or articulate these actions. We simply observe the output . . . The brain begins to cover for this 'done deal' aspect of its functioning by creating in us the illusion that the events we are experiencing are happening in real time—not before our conscious experience of deciding to do something" (Gazzaniga, 1998, pp. 63–64).

As stated by Benjamin Libet, whose pioneering contributions to the study of brain function in conscious mental activity dates from the 1960s, ". . . experimental evidence does show that certain stimuli in the sensory pathway, even when inadequate to produce any conscious experience, can nevertheless be usefully detected by the human subject. The important inference is, then, that neural activities inadequate to produce any subjective awareness can nevertheless help to mediate functions without awareness. Indeed much of our brain activities are of that nature" (Libet, 2004, p. 28).

And to Hayek, who had a thorough grasp of these propositions, but without the advantage afforded by recent neuroscience understanding, what was the 'fatal conceit'? "The idea that the ability to acquire skills stems from reason." The constructivist mind makes a fatal

'error,' blinding itself to understanding, as we are warned, "one should never suppose that our reason is in the higher critical position and that only those moral rules are valid that reason endorses" (Hayek, 1988, p. 21).

That the brain is capable of off-line subconscious learning is shown by experiments with amnesiacs who are taught a new task. They learn to perform well, but memory of having learned the task escapes them (Knowlton et al., 1996).

Also, there are numerous anecdotal reports of people seeking a solution to a problem, who abandon it unsolved, but awaken in the morning with a solution. Such reports involve famous composers, poets and many scientists (Mazzarello, 2000). For example, Mendeleyev reported that the critical rule underlying his periodic table of the elements came in a dream after unsuccessful efforts to gain insight while awake. Poincaré (1913) reports having given up on solving a particularly difficult mathematical problem only to have the solution suddenly appear to him on a trip to Lyon.

It is known from many experimental studies that neuronal patterns associated with task stimulus-responses when awake are reactivated subsequently during sleep (Maquet, 2001). It's like the brain is "practicing" while asleep. Sleep appears to have a consolidating effect on memory, but may also restructure previous representations and yield insight. Thus, Wagner et al. (2004) provide evidence that sleep has a facilitating role in subjects' discovery of a hidden abstract rule that abruptly improved their response speed in a mental task using two simple explicit rules. More than twice as many subjects experienced this "insight" following sleep as after wakefulness independent of time of day.

Perhaps almost all of our individual mental activities and accomplishments are inaccessible to our conscious awareness. Similarly we are not aware of a great range of socioeconomic phenomena, such as the productivity of social exchange systems and the external order of markets that underlie the creation of social and economic wealth.

2.3. Ecological rationality

These considerations lead to the second concept of a rational social order: an ecological system, designed by no one mind, which emerges out of cultural and biological evolutionary processes: home grown principles of action, norms, traditions, and 'morality'.

Constructivism, however, can apply reason by rational reconstruction to examine the behavior of individuals based on their experience and folk knowledge, who are 'naïve' in their ability to apply constructivist tools to the decisions they make; to understand the emergent order in human cultures; to discover the possible intelligence embodied in the rules, norms and institutions of our cultural and biological heritage that are created from human interactions but not by deliberate human design. People follow rules without being able to articulate them, but they may nevertheless be discoverable.

This is the intellectual heritage of the Scottish philosophers and Hayek, who described and interpreted the social and economic order they observed, and its ability to achieve desirable outcomes. The experimental laboratory provides a tool for testing hypotheses derived from models of emergent order.

'David Hume was famously concerned with the limits of reason, the bounds on human understanding, and with scaling back the exaggerated claims of Cartesian constructivism. To Hume, rationality was phenomena that reason discovers in emergent institutions. Thus,

"the rules of morality . . . are not conclusions of (our) reason" (Hume, 1739/1985, II, p. 235).

Adam Smith developed the idea of emergent order for economics. Truth is discovered in the form of the intelligence embodied in rules and traditions that have formed, inscrutably, out of the ancient history of human social interactions. This is the antithesis of the anthropocentric belief that if an observed social mechanism is functional, somebody, somewhere, somehow in the unrecorded past surely must have used reason consciously to create it to serve its perceived intended purposes. This is the default folk belief in the historic origins of any legacy that is functional.

But in cultural and biological evolution, order arises from mechanisms for generating variation to which is applied mechanisms for selection. Reason is good at providing variation, but poor at selection. Constructivism is a powerful engine for generating variation, but it is far too limited and inflexible in its ability to comprehend and apply all the relevant facts in order to serve the process of selection, which is better left to ecological processes.

In experimental economics the eighteenth century Scottish tradition is revealed in the observation of emergent order in numerous studies of existing market institutions such as the continuous double auction (CDA). To paraphrase Adam Smith, people in these experiments are led to promote group welfare enhancing social ends that are not part of their intention. This principle is supported by hundreds of experiments whose environments and institutions (sealed bid, posted offer and others besides CDA) may exceed the capacity of formal game-theoretic analysis to articulate predictive models. But they do not exceed the functional capacity of collectives of incompletely informed human decision makers, whose autonomic mental algorithms coordinate behavior through the rules of the institution—social algorithms—to generate high levels of measured performance.

Both kinds of rationality have influenced the design and interpretation of experiments in economics. Thus, if people in certain contexts make choices that contradict our formal theory of rationality, rather than conclude that they are irrational, some ask why, reexamine maintained hypotheses including all aspects of the experiments—procedures, payoffs, context, instructions, etc.—and inquire as to what new concepts and experimental designs can help us to better understand the behavior. What is the subject's perception of the problem that he/she is trying to solve?

Finally, understanding decision requires knowledge beyond the traditional bounds of economics, a challenge to which Hume and Smith were not strangers. Thus, for Hayek, "an economist who is only an economist cannot be a good economist." This is manifest in the recent studies of the neural correlates of strategic interaction (Kevin McCabe calls it neuroeconomics) using fMRI and other brain imaging technologies. This research explores the neuro-correlates of intentions or "mind reading," and other hypotheses about information, choice, and own versus other payoffs in determining interactive behavior.

2.4. How are the two concepts of a rational order related?

Constructivism takes as given the social structures generated by emergent institutions that we observe in the world, and proceeds to model them formally. Thus, constructivist models need not ask why or how an auction institution arose; or what were the ecological conditions that created it; or why there are so many distinct auction institutions. In some

cases it is the other way around. Thus, revenue equivalence theorems in auction theory show that the standard auctions generate identical expected outcomes, which, if taken literally, leave no modeled economic reason for choosing between them. But society chooses between them in particular applications. It is important to ask how and why, and to avoid dismissing such learning as irrational because of revenue equivalence.

More generally, using rational theory, one represents an observed socioeconomic situation with an abstract interactive game tree. Contrarily, the ecological concept of rationality asks certain prior questions: from whence came the structure captured by the tree? Why this social practice, from which we can abstract a particular game, and not another? Were there other practices and associated game trees that lacked survival properties and were successfully invaded by what we observe? There is a sense in which ecological systems, whether cultural or biological, must necessarily be, if not already, in the process of becoming rational: they serve the fitness needs of those who unintentionally created them through their interactions.

To understand what is—the surviving tip of the 'can-do' knowledge iceberg—requires understanding of a great deal *more* that is *not*. This is because of the rich variety of alternatives that society may have tried, but that have failed. Nor is there any assurance that arrangements fit for one economic and social environment may be fit for another. In the laboratory we can not only rationally reconstruct counterfactuals, as in economic history, but also use experiments to test and examine their properties.

3. Behavioral methodology

As I have indicated researchers in psychology and behavioral economics almost uniformly report results contrary to accepted rational theory (Hogarth and Reder, 1987). It was not always so, but the focus on what are called 'anomalies', beginning in the 1970s, converted an emerging discovery enterprise into a search for contradictions between reports of behavior and standard decision theory. This has been documented by Lopes:

> "Prior to 1970 or so, most researchers in judgment and decision-making believed that people are pretty good decision–makers . . . Since then, however, opinion has taken a decided turn for the worse, though the decline was not in any sense demanded by experimental results. Subjects did not suddenly become any less adept at experimental tasks nor did experimentalists begin to grade their performance against a tougher standard. Instead, researchers began selectively to emphasize some results at the expense of others . . . The view that people are irrational is real in the sense that people hold it to be true. But the reality is mostly in the rhetoric" (Lopes, 1991, pp. 66, 80).

Well before the 1970s, work in decision theory under uncertainty had been challenged by some economists as a theory that was unable to account for certain observations, such as individual purchases of lotteries and insurance, requiring explanation. They sought to redefine the domain of the utility function to get a better representation (Markowitz, 1952; Friedman and Savage, 1948). Also, externalities in choice had long been recognized as potentially important qualifications of standard theory. But it is the neoclassical hypothesis of a particular definition of self-interested agents that led to some of the most productive results and therefore was a prominent and easy target of criticism.

Kahneman clearly does not see people as "irrational" except in the narrow constructivist context used in economic modeling based on dominant choice and arising from the abstract mental concept of the context-free one shot isolated decision or game. In fact he describes his empirical findings contradicting rationality, in this particular representation of the SSSM, as having been easy, thanks to the implausibility to any psychologist of this rational model.

(See the interview of Kahneman and Smith on the Nobel web site.) Unfortunately, the popular press—by focusing on the newsworthy 'rhetoric' of behavioral economic studies—has often interpreted the contributions of Kahneman as proving that people are "irrational," in the popular sense of stupid, so elementary and transparent are their "errors," in the popular sense of "mistakes." The pejorative meaning attached to words that define theoretical concepts and hypotheses has been all too prominent in professional interactions, and this allows easy public access to the muck raking fray in public discussions. Thus, "errors" in the sense of deviations from the SSSM predictions, are referred to in the psychology and behavioral science literature as "cognitive errors," meaning mistakes as deviations from what 'should be' observed. This description implicitly accepts the undoubted and un-doubtable "truth value" of the SSSM as a representation of optimality, and therefore those subjects are indeed making mistakes transparently contrary to their own rational best interest. In science, however, observed deviations from theoretical predictions normally mean that either the theory is "wrong"—i.e. needs modification as a predictive theory, which may mean altering the conception of optimality—or, as in tests of decision theory, human decision making is flawed. Since, as Kahneman has noted, if human decision making is as flawed as indicated by these representations it is hard to understand our species success over the last 2 million years of survival and occupation of the planet. Kahneman here raises a natural question: to what kind of "optimal" decision making process have human beings become adapted?

But looking beneath the rhetoric, many psychologists to their credit, have maintained an intensive program examining the contradictions between observed behavior and the predictions of classical models of choice, bargaining and competition, and have moved the argument from an account of theoretical possibilities and anomalies to deeper empirical investigations. For example Siegel (1959) and Fouraker and Siegel (1963) reported both confirmations and contradictions, and used the pattern to propose improved models. These were among the important contributions defining work "prior to the 1970s" and described by Lopes above, but which were ignored in the rush to produce anomalies and to revise earlier work in the discredited "behaviorist tradition." Similarly, in prospect theory Kahneman and Tversky (1979) have proposed modifications in both the utility and probability weighting functions of standard expected utility theory, and thus revised the specification of optimality in expected utility theory. Research strategies that focus on the study of errors—as "mistakes", rather than deviations from predictions—can distort professional beliefs, to say nothing of popular representations, if the rhetorical emphasis is on the failures, to the

exclusion of the predictive successes, of the theory. New theory needs to be able to embrace the old theory where it is accurate, improve its performance where it is inaccurate, and have new predictive consequences.

As I see it experimental market economics and behavioral economics are in principle complementary. Experimental economists study market performance given individual valuations, while cognitive psychologists study the performance consistency of individual decision making. If the objects traded are prospects the appropriate valuations are their "cash values," whether based on expected utility, prospect theory (Kahneman and Tversky, 1979), or some other representation. Thus, Plott and Agha (1983) study experimental markets in which speculators have the capacity to buy in a market with certainty of demand and supply and resell in a second market with demand uncertainty—in effect the resale values were simple two-outcome gambles in the second market. They report convergence in the second market to a CE defined by demand and supply based on the expected values of the gambles (also see Plott and Turocy, 1996). But the connective interface between rationality at the individual and at the market level and how institutions modulate the interface is yet to be fully explored. It appears that markets may yield equilibrium outcomes given induced cash values, whatever are the cash values—rational, irrational, or nonrational—that are provided by individuals. If those cash values are not individually stable or consistent this may just mean that market equilibrium predicted outcomes are subject to uncertainty.

But there is a more serious problem with the narrow focus on the alleged rationality or irrationality of individual choice. It's not a failure to see the forest for the trees; it's a failure to see either the forest or the trees for the leaves. Individual choice either in the sense of commodities and services based on preference ordering, or under uncertainty is not where the action is in understanding economic performance. The fundamental theorem in economics is that wealth is created by task specialization across individuals, groups, populations, regions and climates; and specialization is determined by the depth and breadth of the market—personal or impersonal exchange systems. What are important about individual choices are the decisions that cause people across time and generations to change tasks, locations, and directions in an effort to better themselves in response to market prices. This does not require individuals to use their gains from specialization and trade for self-interested economic ends in the sense of always choosing dominant outcomes.

To focus on the "rationality" of choice in the narrow frames defined to give it precision does not provide guidance toward a better understanding of this specialization process. People can make a lot of cognitive errors on the way to creating a new market—say in off-the-shelf software programs that allowing new firms to specialize and create much new wealth. All the work of economics is in specialization and the exchange systems that make possible the wealth they create. That work is beyond the field of vision of the individual, but it should not be beyond the vision of the scientist.

4. Fairness

What is fairness? The descriptor 'fairness' has so many meanings in different contexts that I believe it is best to avoid the term entirely in experimental science except where

it is explicitly modeled and the model tested in environments where subjects make decisions on the basis of the defining parameters of the proffered 'fairness' model; then the descriptor 'fair' and its ambiguity can be avoided altogether. This is the way it was used in the utilitarian definitions by Franciosi et al. (1995), then by Fehr and Schmidt (1999), and Bolton and Ockenfels (2000). Of course it is appropriate to use the descriptor if the purpose is to see how its instructional use might have an emotive affect on behavior, but this is not what is done in Kahneman, Knetch and Thaler (KKT) (1986). The emotive content of 'fairness' is clear in the important work of Zajac (1995), who has also examined the rhetoric of fairness arguments as self-interest serving in the Florida, 2000, election controversy where each side charged the other with gross violation of "fairness" (Zajac, 2002).

KKT state ". . . the phrase 'it is fair,' is simply an abbreviation for a substantial majority of the population studied thinks it is fair" (KKT, 1986, p. 201). KKT thereby decentralizes the definition to whoever is responding to a question that uses the phrase "it is fair." Response volatility will therefore compound variation in self-definition with variation in individual responses. This is not good instrument design. Here are some alternative concepts of fairness each anchored in SSSM theories or in cultural norms and which therefore may provide a more precise connection with principles—either constructivist or emergent:

(1) A utility for the reward of other(s) as well as self. This is the concept underlying the market tests of the effect of fairness in experiments.
(2) Equality of outcomes. This is the imputation rule found by social psychologists to be preferred by people in situations where they have no means by which they can identify differences in individual merit or in their contributions to the total to be apportioned to individuals.
(3) Equality of opportunity to achieve outcomes: To each in proportion to their merit or contribution to the total to be allocated. This is the *equity* principle, the preferred imputation reported by social psychologists when individual contributions can be identified. It is also the ancient 'first harpoon' principle in Inuit, Arctic and other cultures (see Freuchen, 1960, pp. 53–53).
(4) To each in proportion to his or her contribution to the net surplus of society. This derives from the equilibrium market allocation principle. The achievement of such allocations and the fact that they are unintended, are both confirmed by over four decades of experimental market tests. Indeed, the fact that they are unintended can explain why the perceived fairness of the imputations is denied by people from the same population that confirms 'fairness' in the sense of the equity principle.
(5) Thou shalt not steal, or covet the possessions of thy neighbor. These historical Judaic laws are the property right norms that support markets, and surely underlie the equity principle that emerges in social psychological findings.
(6) Reciprocity: it is 'fair' behavior to return favors and 'fair' to expect others to return yours. This also has historical Judaic-Christian roots in the Golden Rule: Do unto others as you would have others do unto you, which helps to explain its emergence in two person extensive form games in the laboratory, even when subjects are anonymously paired.

5. Expected wealth maximization or survival?

Kahneman and Tversky's (1979) most widely recognized contributions in prospect theory were in choice exercises demonstrating the relevance of two propositions: the idea that the theory applies to changes in wealth (that is, income) relative to the individual's current asset state, and that people choose as if they are risk preferring in losses and risk averse in gains. No one has stated these propositions more eloquently than Adam Smith:

> We suffer more ... when we fall from a better to a worse situation, than we ever enjoy when we rise from a worse to a better. Security, therefore, is the first and the principal object of prudence. It is averse to expose our health, our fortune, our rank, or reputation, to any sort of hazard. It is rather cautious than enterprising, and more anxious to preserve the advantages which we already possess, than forward to prompt us to the acquisition of still greater advantages. (Adam Smith, The Theory of Moral Sentiments, p. 213).

This much is not inconsistent with the axioms of standard expected utility theory, which requires only that the prizes of choice can be ordered, and therefore applies either to wealth or changes in wealth, since either can be ordered. Also, the theory does not preclude risk preferring preferences so long as utility, kinked or smooth, is bounded.

But I intend no defense of the standard theory; quite the opposite: the weakness of the theory is that it places so *little* restriction on choice. Hence, to which prizes the theory is best applied has always seemed to me to be inherently a subject for empirical determination, with the objective of tightening up the wide breadth of the theory. If applied to wealth, the theory starts to infringe on preference theory over time, recognized from the start as especially difficult modeling terrain where tractability strains plausibility, and rejected empirically by Binswanger (1980) in his large-stakes tests using Indian farmers.

These deeper considerations have been obscured by the narrow focus on errors relative to the arbitrary interpretation that utility is necessarily only about wealth, and on the rhetoric of errors as human mistakes, rather than deviations from the predictions of a theory that itself may be the mistake.

This brings me to the economics of survival on which I will close. Is it rational to maximize the expected value of wealth? Work on the economics of survival by Roy Radner and his associates' offers an alternative:

> Suppose the investor or firm is assumed in continuous time to choose from the set of available investments to maximize the probability that she will survive forever. A basic theorem shows that there exists a critical level of wealth below which the investor appears in the short run as if he is risk preferring and above which risk averse. But this prediction has noting to do with risk aversion. Below the critical wealth level the best prospect for avoiding bankruptcy is to take very high variance risks because they offer a small chance of getting back in the game—increasing wealth enough to enable survival.

Moreover it is shown that the investor who chooses to maximize expected profit (discounted total withdrawals) fails in finite time. Moreover, there exist a variety of

non-profit-maximizing behaviors that have a positive probability of never failing. In fact it is shown that firms that maximize profits are the least likely to be the market survivors.

My point is simple: when experimental results are contrary to standard concepts of rationality, assume not just that people are irrational, but that you may not have the right model of rational behavior. *Listen to what your subjects may be trying to tell you*.

Think of it this way. If you could choose your ancestors, would you want them to be survivalists or to be expected wealth maximizers?

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