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The author comments on the article by Ho, Lim, and Camerer (2006), which showcases the potential of the new behavioral economics approach to marketing with six theoretical "case studies." Each case study describes a new model of consumer behavior or competitive marketing interactions. The author raises two general questions about such models. The first question is whether behavioral economics is, in reality, a new form of theoretical psychology, competing with existing psychological models and theories and subject to the same vulnerability to evidence as applies to psychological theory. The author argues that behavioral economists are ambivalent about this point, sometimes claiming psychological realism or plausibility as a necessary modeling requirement and other times introducing assumptions for modeling convenience. The second question is whether the impact of behavioral economics on marketing will be experienced more strongly through the development of new quantitative models or through the exploration of qualitative lessons stimulated by behavioral violations of rationality. It is argued that behavioral economics not only contributes new modeling instruments but, through its documentation of rationality violations or "anomalies," can provide a rich source of intuitions and insights into consumer psychology as well.

Rebuilding the Boat While Staying Afloat: The Modeling Challenge for Behavioral Economics

Few novel academic movements have gained recognition as rapidly as the behavioral approach within economics. Having stormed the economics citadel, the behavioral economics revolution is now moving further afield to other disciplines that use economic modeling, namely, marketing, finance, public policy, law, organizational behavior, and political science. Even to the most trenchant advocates of behavioral economics, such rapid success must seem somewhat surprising in retrospect. Notwithstanding the attention given to prospect theory (Kahneman and Tversky 1979) and to Richard Thaler's essays on mental accounting and finance (Debondt and Thaler 1985; Thaler 1980, 1985), throughout the 1980s, behavioral economics was something

of a curiosum within economics. Psychological evidence that conflicted with rationality assumptions was politely heard (usually), but it was not systematically incorporated into economic theory, and top journals rarely granted space to behavioral pieces.

In the 1990s, there was a sea change that was associated with the work of younger scholars (e.g., David Laibson, Matthew Rabin), whose success legitimized nonstandard behavioral topics as a promising area for doctoral students. Summer institutes in psychology and economics played an important supporting role by introducing economists at the doctoral and early postdoctoral stages to psychological concepts and experiments. A significant, though perhaps unintentional, success factor was that the movement never created an in-house journal. Economists attracted to behavioral research had to "publish or perish" in the inhospitable mainstream environment. They had to convince skeptical reviewers and editors of the value of the new approach and, in doing so, convert them to the new point of view. Whether adopted deliberately or by chance, this high-risk proselytizing strategy proved highly successful. When the mainstream

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journals opened the gates, the rest of the discipline had to follow.

Broadly speaking, behavioral economics combines two research modes, which are often featured in the same article. The first, the “destructive” mode, highlights an anomaly that conflicts with or, better yet, positively embarrasses the rational model under consideration. Anomalies come in several flavors. The anomaly can strike at the heart of a theory by casting doubt on a key supporting axiom; for example, compound discounting implies that if a person prefers \$100 now to \$110 tomorrow, he or she should also prefer \$100 in 365 days to \$110 in 366 days (Thaler 1981). The anomaly can be a real-world phenomenon that is at odds with rational theory; for example, people vote even though they have virtually no chance of affecting the outcome of a major election, and they act against manifest self-interest by consuming addictive drugs. It can be an experimental finding that is difficult to square with rationality; for example, asking a person whether he or she would purchase a product for a randomly selected “anchoring” price strongly affects that person’s subsequent valuation of the product (Ariely, Loewenstein, and Prelec 2003). Finally, the anomaly may be a previously unrecognized implication of the rational model—for example, Rabin’s (2000) deduction that an expected utility maximizer who at any wealth level would reject a 50-50 gamble between losing \$100 and gaining \$110 would also reject a 50-50 gamble between losing \$1,000 and gaining any amount whatsoever. In each case, the anomaly is exploited for theoretical leverage, that is, for its ability to force a rethinking of some aspect of the standard rational model.

The second, “constructive” mode of behavioral economics research is the creation of new theory that accommodates the anomaly, matches the standard theory in precision, and includes the rational model as a special case. Developing such models is difficult theoretical repair work and involves a great deal of judgment and technical craft. Ideally, the departure from the standard model would be captured by a single parameter that isolates the essence of the theoretical adjustment and facilitates empirical testing. Much of the craft involves finding the right parametric distillate of what would otherwise be a complex and unwieldy revision of standard theory.

Ho, Lim, and Camerer’s (2006; hereinafter HLC) article is a superb introduction to the state of the art in constructive behavioral economics modeling, with the twin qualities—judgment and craft—abundantly on display. The ideal reader of the article is someone who creates or uses economic models in research and who wants to add concepts and techniques to the theoretical tool kit. Ho, Lim, and Camerer present six theoretical case studies, each of which describes a specific model that extends standard theory by adding one or several parameters that capture a psychological dimension missing from rational theory. The authors present the models in finished, ready-to-use form, which conceals the modeling birth pangs and perhaps understates the level of achievement, and then productively apply each to a marketing problem. Three models cover individual behavior, expanding utility theory to include loss aversion, social preferences, and hyperbolic discounting, and three models cover competitive interactions, enriching game theory with stochastic choice, finite reasoning ability, and

learning. The cumulative effect is striking: Thorny puzzles are smoothly resolved by user-friendly models and are put to good work explaining marketing phenomena.

Ho, Lim, and Camerer (2006, p. 308) define the goals for any model as follows:

Our view is that models should be judged according to whether they have four desirable properties: generality, precision, empirical accuracy, and psychological plausibility. The first two properties, generality and precision, are prized in formal economic models.... In general, the third and fourth desirable properties that models should have, empirical accuracy and psychological plausibility, have been given more weight in psychology than in economics, that is, until behavioral economics came along.

The conflict between generality and precision on the one hand and between empirical accuracy and psychological plausibility on the other hand is only apparent. It *is* possible to have it all:

The goal in behavioral economics modeling is to have all four properties, insisting that models have both the generality and the precision of formal economic models (using mathematics) *and* be consistent with psychological intuition and experimental regularity. (p. 308, emphasis in original)

This is a refreshingly unabashed statement of the ambitions of behavioral economics. In the remainder of this commentary, I raise two general, related questions without attempting any resolution: (1) Are behavioral economics models a new kind of theoretical psychology, competing with other psychological theories and, in principle, answerable to the same evidence as bona fide psychological theories? and (2) Does the long-term influence of behavioral economics on marketing and other applied disciplines hinge on models or on loosely organized, qualitative insights?

A NEW KIND OF PSYCHOLOGY?

Most behavioral economists do not perceive themselves as developing alternative psychological theory. At the same time, they reject the traditional methodological “as-if” doctrine that Milton Friedman (1953) advocates (and that is mentioned in HLC [2006]), which states that marketplace empirical accuracy is the only proper criterion for evaluating economic models. With respect to the as-if view, the assumptions of the economic model, which have psychological substance at least on the surface, are merely a convenient abbreviation for the predictions of the theory and should never be tested directly. The only true test is the market.

By adding psychological plausibility to the list of desiderata, the behavioral economist departs from the as-if stance. However, it is not clear whether the departure is wholesale, in which case the goal must eventually be the development of a truly accurate psychological model that will displace whatever is currently offered in psychology, or whether—and this seems the more likely interpretation—the desire for psychological realism stops somewhere in between, at a convenient point of compromise at which assumptions retain psychological plausibility but the ultimate empirical test is still reserved for marketplace predic-

tions, whether conducted in the lab or in the wild. That HLC (2006) posit “empirical accuracy” and “psychological plausibility” as distinct criteria may be a sign of ambivalence on the issue.

Quasi-Hyperbolic Discounting

Two examples from HLC (2006) illustrate how this tension plays out in the context of specific models. The first is the quasi-hyperbolic, b - d model of intertemporal choice that Laibson (1994, 1997) introduced and O’Donoghue and Rabin (1999, 2001) further enriched. It is arguably the single most productive theoretical innovation associated with behavioral economics, overshadowing prospect theory in the breadth of applications. It is well known that the b - d model begins with the standard discounted utility model, with discount rate d , and then makes a theoretical adjustment that is stunningly simple: The present moment, and only the present, is “overvalued” by a factor of $1/b$. Thus, the quasi-hyperbolic time discount factors follow the progression, bd , bd^2 , bd^3 , bd^4 , and so on, with the “present-bias” parameter b capturing the additional discounting of all future utility relative to the present. In Laibson’s formulation, the quasi-hyperbolic agent fully anticipates that at any future decision point, immediate utility will be overvalued. Thus, the “sophisticated” Laibson agent acts strategically, making choices today to influence or restrict the choices of his or her future self. O’Donoghue and Rabin allow for the additional possibility that the agent is “naive” and falsely believes that there is no present bias. As a result, a naive agent forms chronically incorrect expectations about his or her future choices. In particular, a naive agent may procrastinate on investment projects, in which delayed benefits follow an initial cost, by perpetually shifting the starting point of such an investment for “tomorrow.”

The example of gym membership plans, taken from the work of Della Vigna and Malmendier (2004), demonstrates how the presence of sophisticated and naive agents, separately and for entirely different reasons, contributes to lower-than-marginal cost pricing of per-use fees at gyms. Sophisticated customers prefer a higher flat membership fee and a zero per-use fee because that arrangement promotes actual future usage of club facilities and brings it more in line with current preferences. The zero per-use fee caters to their desire to precommit. Conversely, naïfs overestimate their own future usage of the club and thus overestimate the total costs of usage fees relative to the fixed membership fee. The zero per-use fee exploits their lack of self-knowledge.

As an explanation of anomalous pricing, the story is compelling for both sophisticated and naive consumers. The model does indeed work, but when examined closely, its assumptions are psychologically unrealistic. The difficulty resides not in the b - d idealization of hyperbolic discounting but rather in the conceptualization of the naive agent as someone “who is totally unaware that he or she is a hyperbolic discounter and believes that he or she discounts exponentially” (HLC 2006, p. 316). If this statement is taken literally, it is highly improbable that an agent of such description exists. Specifically, it is highly improbable that a person (1) believes that his or her decisions conform to a compound discount rate, (2) exhibits behavior consistent with hyperbolic discounting, and (3) does not acknowledge

procrastination and so forth in his or her own behavior. In lectures to thoroughly naive audiences, I can attest that what generates surprise is not the claim that people are dynamically inconsistent (which immediately elicits smiles of recognition) but rather the claim that intertemporal rationality requires compound discounting; that is, to “stay out of trouble,” people should apply a constant discount factor to each successive period. The actually naive person seems more plausibly characterized by (1*) a belief in hyperbolic discounting, which may be implicit but can easily be made explicit with a time preference survey; (2*) an unawareness that there is anything problematic about hyperbolic discounting per se; and (3*) a rueful recognition that, indeed, his or her own behavior falls short of the ideal, exhibiting self-control problems, procrastination, and so on. According to this characterization, the naive person simply does not draw a connection between 1* and 3*.

In response, it could be argued that the theoretical definition of the naive quasi-hyperbolic agent is just a modeling device that “delivers the goods,” meaning, in this case, that predictions about a person’s own future choices will be off the mark in certain orderly ways. The root cause could be something else, such as an inability to compute the implications of hyperbolic discounting or wishful thinking about self-control abilities, but what really matters for economic applications is just the simple notion that misprediction occurs. As a matter of technique, it is easier and more consistent with current modeling practice to trace mispredictions to incorrect beliefs about a personal discount parameter than, for example, to computational blindness. This line of argument comes close to Friedman’s (1953) as-if position, in which the theory is evaluated on the basis of predictions, not the realism of assumptions.

The Cognitive Hierarchy Model

The second example is the cognitive hierarchy (CH) model (Camerer, Ho, and Chong 2004), which is one of the most interesting inventions to emerge from the behavioral economics workshop. Although the notion of bounded rationality has been around for more than 50 years, it has proved resistant to modeling (but see Rubinstein 1988). The CH model not only demonstrates the possibility of combining bounded rationality with standard theory but also provides guidance for behavior in situations in which normative theory is essentially helpless.

The CH model grants insight into two otherwise puzzling games. The first is the p -beauty contest game, in which players pick numbers from 0 to 100, and the winning player is the one whose number is closest to two-thirds of the group average. The “scandal” with this game is the Nash equilibrium prediction, which rules out all nonzero bids, for the simple reason that if any particular number is an equilibrium played by all, to be the best response, it must equal two-thirds of itself, which holds for 0 only. In experiments, however, winning numbers typically range from 10 to 25. Equilibrium analysis would seriously mislead anyone seeking instruction on how to play this game for real.

The second, and perhaps even more mysterious, example is the market-entry game. Here, players (i.e., firms) independently decide whether to enter a market. Entrants receive a profit of +1 if the fraction of entrants is smaller than a specified fraction d ; they sustain a loss of -1 if the

fraction is greater than d . Nonentrants receive a profit of 0. In this setting, Nash equilibrium makes a striking prediction, namely, that even though the firms cannot communicate, they will each enter with the correct probability d ; thus, even in a one-shot game, approximately the correct proportion of entrants will be observed. Both entrants and nonentrants will presumably be led to different actions by pure reflection on the payoff structure of the game, which is symmetric across all firms. Notably, the experimental data are close to equilibrium, even in a one-shot game.

On the surface, the behavior patterns elicited by these two games have little in common: With the beauty contest game, the equilibrium logic is transparent, yet actual players do not come anywhere close to it. With the market-entry game, equilibrium assigns different actions to otherwise identical players in just the right proportions without communication; here, the hand of the market is truly invisible, yet actual players somehow instantly know how to follow it.

Remarkably, the CH model provides an account of both phenomena, in one case, the deviation from equilibrium and, in the other case, the mysterious consistency with it. The reader is encouraged to work through the model details, but as with quasi-hyperbolic discounting, the core ideas are simple enough. Players go through a finite number of reasoning iterations about other players' reasoning. The cycle "I think that you think that I think ..." eventually stops at some number of iterations, which is different for different people. However, and this is the critical assumption, each player believes that other players use fewer steps of reasoning. The belief of any player about the levels of reasoning that other players engage in is set by a discrete Poisson distribution but truncated at the level of reasoning for that player. Thus, the model is fully specified by the single parameter of the Poisson distribution.

It is a modeling tour de force, but as with quasi-hyperbolic discounting, the psychological reality of the reasoning steps, especially in the case of the market-entry game, is questionable. The assumption that players are "overconfident" about their depth of reasoning relative to others might seem to draw support from the social psychology literature on overestimation of personal abilities, but here, the overestimation is taken to an extreme level: The model insists that every participant in the game believes that others' reasoning is more shallow. Surely, there must be people who feel inferior in their reasoning to their peers. What will they do?

The superiority assumption has more of a feel of a bounded rationality constraint than of a statement about overconfidence. It is virtually a requirement of the p -beauty contest game that a person must believe that his or her choice is more clever than that of others; if this person did not hold such a belief, he or she would choose a different number. The logic of the game yields three unpalatable options: (1) select zero, the implausible Nash equilibrium prediction; (2) select a positive number and justify it "egocentrically" by deeper reasoning ability; or (3) select a positive number without any justification whatsoever (in effect, this is what the zero-step reasoners do in the CH model).

These questions arise only if the model is treated as a genuine psychological hypothesis, which, again, may not be HLC's (2006) full intention. I believe that the ambitious goals that HLC outline should be made more ambitious still

and that this and other behavioral economics models may be interpreted as contributions to a future theoretical psychology, one that will appear uncomfortably rigorous from the current psychological perspective and uncomfortably vulnerable to empirical refutation from the current economic perspective.

ANOMALIES AS SOURCES OF INSIGHT

Treating behavioral economics as substantive psychology casts a somewhat different light on the likely impact of the new discipline on marketing and on the pedagogical role of anomalies. As I noted previously, a key objective of behavioral economic modeling is to retain the precision and generality of standard economic theory, while enhancing its empirical accuracy and plausibility. The virtues of precision and generality are easy to defend: Precise models give exact predictions that can be tested; general models can be applied indifferently across domains. Nowhere are these virtues more on display than in game theory.

Yet game theory cannot be viewed merely as a calculus for deriving precise predictions (i.e., for computing equilibriums). After all, actual applications of the theory that involve computation of equilibriums are relatively rare and certainly can neither explain the appeal of game theory throughout the social sciences nor justify its presence in most business school curricula. The value of game theory resides as much in its ability to provide a qualitative typology of competitive situations: zero-sum, prisoner's dilemma, stag hunt, p -beauty contest; each game has a distinctive strategic personality that is diagnosed by game theory even when its exact predictions are off the mark. The essence of this personality can be absorbed in qualitative terms, provided that there is a grasp of the basic concepts of strategy, best response, equilibrium, and so on.

Something similar holds for the insights provided by behavioral economics, especially the insights generated in the "destructive" anomalies mode. The album of behavioral anomalies provides insights into motivation and reasoning that are neither a part of lay psychology nor something that can be easily extracted from a standard psychology textbook. As HLC (2006) themselves state in their abstract, "[b]ehavioral economics explores the implications of the limits of rationality." The key words here are "explore" and "rationality." The standard theory under attack is not just any theory, but a theory of rational behavior with normative force. Failures of rationality at the individual or collective level are not simply failures of a particular obsolete model but also a deeper challenge to intuitions about human behavior.

The marketing discipline should be especially alert to the limits of rationality. Limited rationality creates opportunity for influencing behavior (for better or worse) and for creating value out of thin air (Thaler and Benartzi 2004). A failure of rationality invites speculation about whether the failure is inevitable or whether it represents a deadweight loss that can be restored by creative development of new products or institutions. Indeed, an ambition of marketing should be the development of qualitative principles (as in architecture or industrial design) that reflect what consumers truly want from goods and services or from payment arrangements, holding constant the economic bottom line (Prelec and Loewenstein 1998). Behavioral economics

is both a novel modeling technology and a source of psychological insight that can contribute to the development of such principles.

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