On Modeling In Behavioral Accounting Research

William B. Joyce, (E-mail: wbjoyce@eiu.edu), Eastern Illinois University

ABSTRACT

There is a sufficiently significant consistent body of knowledge to be modeled. This research seeks to provide a more complete foundation to the current models while also providing a more unified explanation to a wider range of empirical results.

INTRODUCTION

In recent years, behavioral accounting has changed from a niche topic to one that is well represented in all of the major journals. For accountants who are wondering what all the fuss is about, Advances in Behavioral Economics (edited by Camerer, Loewenstein, and Rabin and published by the Russell Sage Foundation and Princeton University Press in 2004) is an excellent introduction to the field. Advances reprints a selection of the "greatest behavioral hits" of the 1990s, eighteen papers that could be characterize as new research and seven surveys that were previously published in books or in review journals. In addition, the editors contributed a substantial overview chapter, "Behavioral Economics: Past, Present, and Future," which outlines the contents of the book, argues for the importance of behavioral economics for economics and accounting alike, and speculates about promising new directions of research. The book's wide coverage and its well-articulated arguments for the field make it a valuable reference and teaching-aide, and the included surveys will help newcomers catch up with what is now a very large literature.

The book's scope and arguments also make it a convenient platform for evaluating and critiquing the behavioral field as a whole, and that will be the main focus of this essay. This research will not try to survey the entire behavior field: it would require at least a monograph. Instead, these comments will emphasize the ways the field could improve as opposed to its successes so far. But first there should be a few comments about the book itself.

Part 2 of the book, "Basic Topics," includes surveys, experimental results, and theoretical models; the table of contents groups these chapters by topic (e.g., "Fairness and Social Preference" or "Reference Dependence and Loss Aversion"). Part 3 of the book, "Applications," consists mostly of analyses of field data, but also includes a survey that focuses on data from experiments. At over 700 pages, there is too much material for the book to be read straight through, and many readers may prefer to read all of the chapters that deal with a particular topic instead of reading the chapters in order. For example, chapter 6 ("Time Discounting and Time Preference: A Critical Review" by Frederick, Loewenstein, and O'Donoghue) and chapter 7 ("Doing it Now or Doing it Later" by O'Donoghue and Rabin) are under the heading "Intertemporal Choice," while Laibson's "Golden Eggs and Hyperbolic Discounting" is in the "Macroeconomics and Savings" section of part 3; a reader interested in the behavioral accounting of temptation and self-control might want to read all three of these chapters before reading about other behavioral topics such as social preferences. For that reason, it would have been nice to have a page at the start of each topic group in part 2 that explained the relationship between the part 2 papers and those in part 3. It would also have been nice to have a description of the relationship between the papers in the same section, as when one paper is a survey that discusses the following one. More ambitiously, it would have been very helpful for the book to have some new material that commented on the reprinted papers. What new work has either reinforced or questioned the paper's conclusions? Do the authors endorse all of the claims of the included papers with equal confidence or do some of them seem more convincing than others? Which claims seem plausible but need further investigation? This sort of commentary is unusual in collections of papers, but it is helpful in textbooks and surveys.

LITERATURE REVIEW

One accomplishment of behavioral accounting has been simply to draw accountants' attention to a range of facts and issues that seem important, such as anchoring and base-rate neglect in probability judgments (Tversky and Kahneman, 1974). A second accomplishment has been to develop formal models that generate and explain these regularities, and can be incorporated into larger models of markets or multi-agent experiments (Fudenberg, 2006. This second, more theoretical, agenda is more likely to have a deep and lasting impact on the rest of accounting, so improving it is the key to further advances in the field.

Theories (both in accounting and more generally in all behavioral work) should be judged by Stigler's (1965) three criteria: accuracy of predictions, generality, and tractability. The standard model of individual behavior does very well in terms of generality and tractability, but behavioral accounting has helped highlight some areas where the standard model's predictions are sufficiently wide of the mark that changes are valuable. The challenge for the field is to generate more accurate predictions without sacrificing too much on the other two of Stigler's criteria.

There are three of the behavioral models that have been the most successful with respect to Stigler's criteria: models of loss aversion in the spirit of prospect theory (chapters 4 and 5), the quasi-hyperbolic model of inter-temporal choice (as in chapter 15 by Laibson), and the Fehr and Schmidt (chapter 9) model of social preferences. Researchers continue to look for more general and elegant ways to capture the same sets of facts, and it may be too early to decide that any of these models should be accepted as canonical. However, even if one is not convinced by the specifics of the various models, they have been used to explain a wide range of observations, which at the least suggests that there are significant empirical regularities in the areas the models address. In addition, all of these models are tractable enough to be embedded in richer and more complex settings, so that behavioral accountants can and will adapt the models to explain yet more facts. These models, and the diverse set of experimental and field data, definitely justify chapter l's claim that it is unwise and inefficient to do behavioral accounting without paying some attention to psychology provided that that instruction is interpreted as applying to the field as a whole, and not to every individual accountant.

METHODOLOGY

The work described in behavioral journals is very interesting and it is clear that much more can be learned by further research along these lines. However, unless the insights and stylized facts obtained so far are related to a small number of models of individual behavior, with some guidelines for when each model should be expected to apply, behavioral accounting may remain a distinct field with its own methodology. Chapter 1 articulates a larger ambition: The hope that some sorts of "behavioral" models will lose "their special semantic status" and become more widely taught and used. If that happens, the field will have advanced beyond "Behavioral Accounting," and the sequel to this volume won't need to be called "More Advances in Behavioral Modeling." From an editors' perspective, the ideal situation might he one where the sequel could be called "Modern Decision Theory" or "Models of Consumer Choice."

To achieve this goal of becoming "main-stream accounting," the field will need to confront several issues: mostly as criticisms of chapter 1. However, this should not obscure the respect for the editors or for the field as a whole. Indeed, chapter 1 acknowledges most of what can bee seen as the key challenges facing the field. Still, the chapter probably understates the difficulties these challenges pose, so one goal is to better highlight the potential problems. Also, chapter 1 fails to acknowledge the extent to which the various parts of the book tend to focus on only one or two of these issues and ignore all of the others. This may be partly justified by the "different tools for each situation" approach, but it would be useful for more behavioral accountants to think about how the various "behavioral critiques" fit together.

Assumptions Should Be Evaluated As A Group And Not One-By-One

As chapter 1 declares, the standard approach in developing theories in behavioral accounting is to modify a few assumptions of the standard theory in the direction of greater psychological reality. This approach presumes that since accountants found a given set of assumptions useful, the only issue in changing one of them is whether the new assumption on its own seems reasonable given the motivating facts. However, factors that support one modification of the standard model may also argue for further modifications. Thus, modifying one or two assumptions and leaving the rest unchanged may lead to a logically consistent alternative model, but one whose domain of application is unclear or nonexistent. This argument is illustrated later in this section in discussing some applications of equilibrium analysis in behavioral accounting, and also in discussing some models of temptation and self-control, but these are not meant to be an exhaustive list. More generally, the point is that behavioral accounting could be improved by adding a second step to the standard model-creation process described above: after modifying one or two of the standard assumptions, the modeler should consider whether the other assumptions are likely to he at least approximately correct in the situations the model is intended to describe or whether the initial modifications suggest that other assumptions should be modified as well.

Choice Overload In Modeling Choice

The fact that behavioral academics do not typically examine the domain where all of their assumptions might simultaneously apply has probably contributed to one problem with the literature in this field: There are too many behavioral theories, most of which have too few applications. It might be argued, weekly that models correspond to tools and a bigger toolkit is better. Some economic models assume risk neutrality and others assume risk aversion, some models assume selfish preferences and others add a bequest motive, and so on. However, the current state of behavioral accounting offers far too many tools and too little guidance about when to use each one; called choice (Kamenica, 2006).

As an example of the problem, consider the question of how to model mistakes in inference. Agents do make various types of mistakes in inference, but when are the various mistakes likely to be either more common or more significant. When inference mistakes are incorporated into a model, should the "confirmatory bias" model be followed? Rather, should it be assumed that agents update as Bayesians but treat independent draws as draws with replacement (Rabin, 2002)? Or should it be assumed that agents mistakenly think they see trends in data (Barberis et al., 1998)? Or should some combination of these methods be applied? Note that the problem here is different than deciding when to include (e.g., bequest motives in a model. Bequest motives matter for lifetime savings, and hence influence the shadow value of wealth, but are unlikely to be relevant for many consumption decisions, while all of the inference mistakes described above are potentially relevant in a given inference problem.

What Should Behavioral Theories Do?

The proliferation of theories also raises the question of what the theories are trying to do. Since psychology papers tend to be less formal than other used in behavioral accounting, one useful but minor role of behavioral theories is simply to give a precise statement of the chosen behavioral regularity. A second role is to exhibit a set of assumptions that can generate the specified behavior, be it the endowment effect or the law of small numbers. As an economic theorist, neither of these objectives seems very satisfying, and certainly neither is grounds for teaching the model in question in a first-year theory course. These and other worthwhile models that have fairly special domains of applicability are taught in the various field courses. This is not to say that integrating some aspects of behavioral accounting into first-year classes is not good. In addition, some behavioral models (e.g., Fehr and Schmidt, 1999) fit easily into the standard framework. However, before behavioral theory can be integrated into mainstream accounting, the many assumptions that underlie its various models should eventually he reduced to the implications of a smaller set of more primitive assumptions. Game theory is more of a methodology than a body of empirical facts, but it too has evolved.

In the long run, it might be hoped to derive mental behavioral phenomena and confirmatory bias from such basic properties such as bounded cognition and the modular structure of the brain. Until then, accounting researchers should devote more effort to synthesizing existing models and developing more general ones, and less effort to modeling yet another particular behavioral observation. Three areas in particular (prospect theory/loss aversion, social preferences, and quasi-hyperbolic preferences) should be held up as examples for emulation. One or two more such "multiuse" theories will be far more valuable for behavioral accounting and accounting as a whole than any number of special-purpose models whose main function is to formalize observations from the psychology literature.

Context And Cues

As chapter 1 says, behavioral issues has emphasized the malleability and context-dependence of preferences and behavior. Reinforcing this point, chapter 17 on "Money Illusion" (Shafir, Diamond, and Tversky) focuses on empirical examples of a specific sort of framing effect, namely the tendency for decisions to depend on absolute as well as relative prices. Unfortunately, framing and context are very difficult to capture in formal models, and are ignored in most of the more formal papers in the field and in this book. This is true in particular of the papers on the topics of social preferences, even though these phenomena have been shown to be very susceptible to framing. For example, the weight experimental subjects give to other subjects' payoffs can be altered by pre-interaction "speeches," and also by manipulations such as choosing the "dictator" in an ultimatum game by the results of a quiz. Moreover, in some settings, such as wage negotiations, one or both of the parties involved may be willing and able to manipulate the way the issue is framed. Most direct evidence of framing effects has so far come from laboratory settings, but the Bertrand et al. (2005) study of ads for bank loans shows that framing can have significant economic effects in the field. This makes the need for a model of frame-determination all the more important.

Another aspect of the malleability of preferences is the way that people to some extent view money rewards as being "as immediate and tempting" as an immediate utility payoff. Since people cannot literally consume currency, why do they act as if current monetary rewards are tempting? In brief, it seems that the "impulsive" or short-run self treats money as a cue for an immediate reward even though the only real consequence of earning money is in the future (Haruno et al., 2004). This subjective equivalence of money and immediate gratification is important for the interpretation of empirical studies that show people exhibit "preference reversal" about the timing of payoffs, as these studies almost always examine monetary payoffs and not consumption choices (McLure et al., 2004). It is also important for understanding the fact that human subjects exhibit a paradoxically large amount of risk aversion to small money gamble (Rabin, 2002). However, the tempting nature of money leaves open the questions of exactly which financial rewards we should expect agents to view as temping (cash versus vouchers and even timing).

The underlying mechanism may also underline some types of framing effects. Agents may be tempted differently by different descriptions of rewards despite then having exactly the same payoffs. Thus, the study of learning and conditioning may eventually lead to useful models of frames, cues and mental accounts. Currently, they are a crucial buy unexplained part of many behavioral analyses.

Equilibrium: When And Why

The next set of issues is important for behavioral accounting, behavioral economics, as well as "regular" economics. First, when is equilibrium analysis likely to be a good approximation of observed outcomes? Second, when it is not, what sort of models should be used to models should be used to predict behavior outside of the lab? Game theorists have long understood that equilibrium analysis is unlikely to be a good predictor of the outcome the first time people play an unfamiliar game, and it may be uncontroversial that some aspects of economic life are best described by non-equilibrium play. However, the answer to the first question is less obvious than chapter 1 suggests, and so far behavioral researchers have been reliant on equilibrium analysis for developing models of market outcomes. Of course, equilibrium is also the standard assumption in "non-behavioral" accounting and economic applications of game theory, but the extensive discussion of models of initial period, non-equilibrium, play (chapters 12 and 13 by Crawford and Camerer, respectively), and the book's inclusion of this topic as part of "behavioral economics" highlights the need for this material to be synthesized into the rest of the field.

A brief review of the literature will help explain why it is not always obvious when the outcome of a game will approximate the equilibrium. There are extensive theoretical and experimental literatures on "learning in games," based on the idea that equilibrium can arise as the result of a non-equilibrium process of learning, imitation, or adaptation (Young, 2004). The former investigates the long-run properties of various learning models, comparing their performance (from the viewpoint of individual agents) and convergence properties (which processes converge to equilibrium in which classes of games), while the latter tries to distinguish between learning models on the basis of experimental data. Most of the formal models of learning in games, and most game-theory experiments, rely on the idea that agents play a game repeatedly against different opponents, in order to abstract from repeated game effects; this necessarily implies that agents use their experience with past opponents to guide their actions in the current game. It is tempting to conclude from these models and experiments that equilibrium analysis almost never applies in the field, as agents rarely play exactly the same game a great many times. But as argued in Fudenberg and Kreps (1993), any sort of learning involves extrapolation from past observations to settings that are deemed (implicitly or explicitly) to be similar, so what matters is how often agents have played "similar" games. In addition, in field settings, unlike the lab, there are additional sources of information beyond direct experience: Agents may engage in "social learning" by asking the opinions and advice of friends, parents, and neighbors, and in some cases (such as retirement savings) they can also consult books, magazines, and outside experts (Schlag, 1998). The possibility of non-equilibrium social learning does not mean that society effectively pools all information and ends up at the equilibrium, but it does mean that the applicability of equilibrium analysis to most field data is an empirical question that can't e resolved by a priori arguments.

Unfortunately, once one leaves the controlled laboratory environment it is not clear how to identify equilibrium versus non-equilibrium play. If one is certain that payoffs are constant over time, then any variation in play at all shows that agents are not playing a static equilibrium, but this leaves open both the possibility that payoff functions vary and that play corresponds to the equilibrium of some dynamic game. So what is needed is a plausible set of identifying restrictions on the nature of payoffs and strategies, and a model of non-equilibrium play that can be econometrically implemented when the actual payoff functions of the players are unknown to the analyst (Bajari et al., 2005). Until something like this is done, the implications of chapters 12 and 13 for field data will be difficult to determine, and it will he difficult to incorporate non-equilibrium reasoning with the rest of behavioral accounting.

Equilibrium Analysis In Behavioral Accounting

Although the status of equilibrium analysis when agents are "rational" is a problem for both accounting and economics, the assumption of equilibrium and the choice of the appropriate equilibrium concept is even more problematic in some behavioral models. As it relates to equilibrium, the "change one assumption" approach to behavioral accounting is to assume agents have a specific form of cognitive imperfection and then adopt a version of Nash equilibrium that is as close as possible in form to the usual one. The problem with this approach is that, as noted above, the usual rationales for Nash equilibrium (in laboratory experiments) rely on unbiased learning by the agents. If, as in Rabin and Schrag (1999), agents suffer from confirmation bias in learning about the distribution of chance moves, then it seems likely they would suffer from a similar bias in learning about opponents' play. If they do, then models of non-equilibrium adjustment based on learning will not typically lead to Nash equilibrium.

A similar concern arises in evaluating Eyster and Rabin's (2005) concept of "cursed equilibrium." Experimental evidence shows in common-value auctions agents overbid and are thus subject to the "winner's curse." Eyster and Rabin (2005) argue that this fact, and related errors in other incomplete-information games, can be explained by the concept of "cursed equilibrium". In this equilibrium, players have correct beliefs about the joint distribution of types, and also have correct beliefs about the aggregate distribution of opponents' play, conditional on each of their own types. However, instead of playing the best response to the actual opponents' strategies, each player chooses the action that is the best response to a convex combination of the actual strategies and the aggregate distribution, with a weight on the aggregate distribution. In the "fully cursed", agents completely ignore the correlation between other players' actions and their types; this corresponds to the outcome of a learning model in which agents observe opponents' actions but neither the opponents' types nor their own payoffs (Jehiel, 2005). The case when this weight is zero is even easier to explain, as it corresponds to the usual Bayesian–Nash equilibrium. However, it is hard to imagine a reasonable learning process that leads to the intermediate cases. It is true that in some cases intermediate values of the weight fit better than either extreme, but this is not evidence that the model is a good approximation of is happening. In addition, the fact that the amount of "cursedness" typically declines as subjects become more experienced suggests that the curse, while real, is not an equilibrium phenomenon (Crawford and Iriberri, 2005).

The learning-theoretic interpretation of equilibrium makes a somewhat different critique of Benabou and Tirole's (2002) model of a Bayesian equilibrium between various "selves," where each self knows the distribution of the other selves' possible "types" and also their equilibrium strategies. The situation here differs in that each self has correct beliefs about the strategies of all other selves, so there is not an inherent conflict with the assumption of equilibrium. Rather, the problem is that for nonequilibrium learning to lead to the Benabou-Tirole equilibrium, the player's type(s) would have to be independently and identically distributed across repetition. If, as seems more reasonable in this setting, the player's "type" (e.g., ability or self-control ability) is fixed once and for all, then learning will not lead to the Bayesian equilibrium that is analyzed by Benabou and Tirole, but to the Nash equilibrium for the game where the payoff function is known.

The O'Donoghue and Rabin model of time preference (2001) has complete information in the sense that each of the "selves" is certain of the payoff functions of the others, but it suffers from a related problem. In this model, at each date "t" the agent values utilities at future dates "t+" at current utility, but forecasts that his future play corresponds to the multiple-selves equilibrium of the standard (or "sophisticated") quasi hyperbolic model in which the selves playing at each date "t" evaluate future utilities at rate of current utility (with no consideration of the time value of either money or utility), and this is common knowledge between the selves. The model is motivated by evidence that people sometimes misperceive their own behavior, but it is not clear how anyone would come to make this particular form of mistaken forecast, and once again a non-equilibrium model might he a better match for the facts the model is trying to explain.

Of course, one reason for the use of equilibrium models here is simply the lack of a standard off-the-shelf alternative, and despite their flaws these "faux-equilibrium" models have been useful in showing that these sorts of behavior can be modeled and analyzed, instead of merely noted and then ignored. Still, behavioral accountants would be well served by concerted attempts to provide learning-theoretic (or any other) foundations for its equilibrium concepts. At the least, this process might provide a better understanding of when the currently used concepts apply, but a serious effort to find foundations will typically end up suggesting somewhat different, and more accurate, solution concepts than the ones that have been discussed previously.

Models Of Temptation And Self-Control

Equilibrium analysis is not the only area of behavioral accounting and economics where the usual change-one-assumption approach overlooks the question of how the entire set of assumptions fits together. As a second example, consider the question of how to model the idea that agents know they have a self-control problem, and so can be willing to pay a premium to reduce their own future choices. The two leading models of this idea are quasi-hyperbolic preferences, as in Laibson (chapter 15), and Gul and Pesendorfer's (2001) axioms and corresponding representation. Each of these approaches either implicitly (Laibson) or explicitly (Gus and Pesendorfer) imposes a form of the classical "independence axiom" on choices over menus of actions. However, if agents will face a self-control problem in choosing an item from a menu, it is not obvious that the independence axiom should apply. Moreover, the independence axiom should be expected to generally fail if agents must take some sort of self-control action, just as it does when agents must commit to some of their consumption decisions before knowing the outcome of a wealth lottery (Machina, 1984). Thus, the independence axiom is less compelling here on a priori grounds than in the standard model, where it has a normative justification. In addition, the evidence that self control is a scarce resource (Muraven and Baumeister, 2000) and is impaired by cognitive load (Ward and Mann, 2000) supports models of self-control that are not consistent with the form of the independence axiom that the quasi-hyperbolic and Gul–Pesendorfer (2001) frameworks assume. Still, these models have been useful and important.

Bounded Rationality

In recent years, behavioral accounting has evolved more or less independently of the literature on "bounded rationality" (Ellison, 2005). It is hard to give a precise definition of bounded rationality, or to draw a sharp line between it and behavioral accounting, but bounded rationality papers typically suppose that some agents (consumers, firms, or both) use exogenous "rules of thumb" and then derive the consequences. In principle, it would be nicer to derive these exogenous rules from a small set of fairly standard assumptions, and one might hope that behavioral accounting could eventually do so. Even when a formal derivation is not possible, one might feel the conjectured rule is more plausible if it can be shown to be rooted in psychological observations that apply more generally. However, the psychology literature is large, and many of its claims are imprecise, so simply finding one concept or claim in the psychology literature that is consistent with the conjecture may not make the rule any more convincing. This is particularly true when the psychology literature does not provide sharp restrictions on just when one should expect to see the behavior in question.

As an example, consider the fact that in some settings some consumers seem to ignore relevant product characteristics, such as shipping costs (Hossain and Morgan, 2006). Moreover, firms in some markets act as if they are aware of the consumers' tendency to ignore characteristics, and reinforce it by making the relevant information hard to find (Ellison and Fisher-Ellison, 2004). Consumers are not expected to neglect all sorts of information, but the literature does not have a useful theory of just what will be ignored in which situations. For the time being, it seems better to simply make this neglect an ad-hoc assumption. Similarly, in some situations people appear to use simple heuristics such as "pick a product with probability equal to its market share" (Smallwood and Conlisk, 1979) or "identify the average payoff or utility of a product with its value in a small sample" (Ellison and Fudenberg, 1995). One long-term goal for behavioral accounting is to incorporate more ad-hoc rules into a formal framework. This will require a methodological shift for the field, as it involves looking for inspiration to the older accounting and economics literature in addition to psychology.

ANALYSIS

Researchers in behavioral accounting are beginning to use two sorts of data that lie outside the traditional scope of accounting, namely questionnaires about mental states (e.g. "How happy are you right now?" "How happy would you be if "X" occurred ?") and data such as neural imaging on physiological processes inside the brain. On a classical revealed-preference view, neither sort of data is of interest, but this data can indeed be of use, provided it is interpreted correctly.

Neuro-Accounting

Neural imaging work so far is suggestive of how certain sorts of decisions are made. Even if one takes the view that the only goal of accounting is predicting behavior on the basis of "external" variables, having a model that is in better accord with the underlying structure of the brain can be valuable, as it may lead to more accurate out-of-sample predictions (McLure et al., 2004).

However, the interpretation of neural imaging and neurobiological data can be difficult and subtle, especially when one tries to use imaging data to resolve debates, as opposed to suggesting models. As an example of this difficulty, I am going to focus on a small and not representative portion of the excellent survey by Fehr, Fischbacher, and Kosfeld (2005) on the "Neuroeconomics of Trust and Social Preferences." Most of their arguments quite convincing, but when they said that the evidence they present casts doubt on the claim in Samuelson (2005) that observed cooperation in the one-shot prisoners' dilemma might be a consequence of players mistakenly treating the game as if it were part of a repeated interaction is questioned. This claim is an updated version of the "misperception" argument of Gale, Binmore, and Samuelson (1995). The extent of the data cited by Fehr, Fischbacher, and Kosfeld (2005) to help resolve the issue is questioned. Basically, Fehr, Fischbacher, and Kosfeld (2005) note that there is more striatum activation when players cooperate with a human than when they cooperate with a computer, and also more activation than from receiving the same payment as an reward without contingency. They argue that this shows that people are happier arid derive more utility from interactions with cooperative people, and suggest that this refutes the explanation based on misperceptions. However, the Fehr, Fischbacher, and Kosfeld (2005) argument seems to rest on the assumption that whatever misperceptions Gale, Binmore, and Samuelson (1995) have in mind have no effect on striatum activation, and currently there is no reason to think that this is the case (Singer et al., 2004).

More generally, at this point neural imaging can provide insights into the mechanisms of various behaviors and cognitive processes, and these insights may suggest useful experiments or interesting models, but care needs to be extended to distinguish neural correlates of a behavior from its causes. One concern is that many parts of the brain seem to be involved in processing rewards. Even in apparently simpler cases such as a monkey'' valuation for various foods, where researchers have identified individual neurons whose activation is correlated with value, the mechanism by which this reward information is generated and used for decision making is not understood (Padoa-Schioppa and Assad, 2005). In particular, activations in one brain region may be the consequence of activations at other "upstream" neurons (O'Doherty, 2004). A second concern is to not confuse "biological" with "genetically determined." For example, a number of studies have shown that people in different countries and cultures tend to play differently in a range of stylized laboratory experiments, and these differences are presumably correlated with differential activation in some parts of the brain that are involved in assessing rewards and making decisions (Henrich et al., 2004). However, such a correlation would be consistent with the two cultures being genetically identical, as the differential activations could be a learned response to living in different cultures. In that case it would seem more natural to think of the activation patterns as a consequence, and not a cause, of the difference in cultures.

Regardless of these issues of causality and interpretation, it is intriguing that neural imaging data can be used to predict future behavior. One of the best examples of this is in de Quervain et al. (2004), who look at activations when agents decide to punish. They find observing activations of different agents making the same choice in earlier tests to help predict subsequent choices. However, the interpretation of this fact seems open to debate. Specifically, how stable is the "preference" for punishing? Can it be manipulated by experimental instructions, such as "Past work has shown that punishing players in this game has little or no effect on their future play?" (It could be rationally expected that this treatment would decrease the amount of punishing, the question is whether it also reduces striatum activation.) Still, neuron-accountants will develop more of these sorts of predictions in the future, and their work may help resolve some of these interpretational questions. It seems too early to know just how much impact this will have on most of accounting, but the potential impact is large, and the research underway is fascinating. Thus, while every accounting researcher not ought to take up neuro-accounting, anyone interested in individual choice and decision making ought to keep an eve on how it develops (Platt and Glimcher, 1999).

Affective Forecasting

It is even more difficult to evaluate the usefulness of work on "affective forecasting" (Wilson, Myers, and Gilbert, 2003) and "predictive utility" (Kahneman, 1999), which asks subjects "how happy are you now" and "how happy would you be if this outcome occurred?" This literature argues that there reports are a good measure of peoples' internal states. It also argues that people make systematic mistakes both in predicting how various outcomes will influence their happiness, and in remembering how happy they were in the past.

One possible interpretation of this work goes as follows. First, reported happiness is a good measure for happiness as a subjective mental state. Next, people always choose the actions that they think will make them happiest, where "happiness" here is the same state that is reported in the happiness surveys. Thus, the systematic forecasting errors found in the literature on affective forecasting show that people often make mistakes in trying to predict how various actions will make them feel, and moreover that these mistakes lead people to take the "wrong" actions. Hence, revealed preferences are not the best guide for evaluating the effects of various government policies. More strongly, it can be concluded that welfare judgments and policy decisions should take as their objective the reported happiness of the population.

In thinking about these ideas, it is helpful to distinguish between welfare economics as political economy (how one thinks the government should make decisions) and welfare economics as moral philosophy (how to advise others to behave). Even if it is believed that people do make systematic errors in evaluating how various choices will influence the appropriately defined measure of their "welfare," it cannot be assumed that the government or policy analysts would make better evaluations. For this reason, people's actions and ex-ante predictions are the best guide to what is in their own interests (Choi et al., 2004).

However, the situation is different when considering how to plan behavior or advise others how to behave, as knowledge of typical errors can help prevent one from making them. This distinction between the sorts of preferences that are considered valid in policy evaluation and the sorts of preferences that are "reasonable" is important even in the absence of survey or neural data, and it holds even when people perfectly predict all of their future mental states. For example, it is consistent with the standard model to be completely impatient, and assign zero value to all future payoffs. However, extreme shortsightedness is likely a mistake. Indeed, a major task of teaching accounting is help students understand the "appropriate" weight to give to future consequences (Shapiro, 2005). Similarly, the standard "rationality" axioms for subjective probabilities does not imply that people's probability forecasts should be calibrated in the sense of Alpert and Raiffa (1982): it is "rational" to make all of one's 90 percent confidence intervals so small that they rarely contain the true value of the variable in question. Yet people whose subjective beliefs are too far from reality are deemed insane, and Biais et al. (2005) find that subjects who are better calibrated do better in an experimental asset market. Thus the questions that "mistakes" pose for welfare economics are much broader than the traditional subject matter of behavioral accounting or economics.

Moving from welfare economics to the conceptually clearer task of predicting behavior, Kimball and Willis (2006) sketch a framework for using survey data on happiness to better estimate and forecast consumer demand. If their project is successful, it will move survey data into the mainstream accounting. However, it is too early to tell if that will be the case.

CONCLUSION

The field of behavioral accounting continues to evolve and progress. In addition to the work on imaging and surveys just described, two other developments deserve emphasis. One is work on the implications of "psychologically based" preferences, and nonstandard preferences more generally, for market outcomes. A priori, one might expect that these preferences will have the clearest impact in monopoly markets, and that market competition would in some cases limit both the impact of "behavioral" agents on prices and the extent to which these agents are exploited by others. These questions are explored by Shapiro (2005).

Second, the recent literature that looks for evidence that the various sorts of behavioral preferences that subjects exhibit in laboratory experiments are actually observed in field data. Recent work by DellaVigna and Malmendier (2006) shows that agents are willing to pay a premium to reduce their options in some real-world decision problems. Bandiera, Barankay, and Rasul (2005) find evidence for social preferences in the difference between worker effort under relative incentives and under piece rates, and Bertrand et al. (2005) show that the way ads for bank loans are "framed" can have a substantial impact on market demand. This sort of evidence helps make the case that "behavioral effects" should be of interest to all sorts of behavioral researchers.

In conclusion, behavioral accounting has many insights and observations that should be used to improve accounting as a whole. The first generation of behavioral models has served a useful purpose by showing that there is indeed a body of "behavioral facts" that is both economically significant and regular enough to be modeled, but it is unlikely that the first model of each of these new phenomena is the best that our field can provide. An important part of the further development of behavioral accounting will be to devote more care to the foundations of its models, and to develop unified explanations for a wider range of departures from the standard theory.

REFERENCES

1. Alpert, M., and H. Raiffa, 1982. A Progress Report on the Training of Probability Assessors, In *Judgment Under Uncertainty: Heuristics and Biases.* New York: Cambridge University Press.
2. Bajari, P., H. Hong, J. Krainer, and D. Nekipelov, 2005. Estimating Static Models of Strategic Interactions. *Mimeo*.
3. Bandiera, O., I. Barankay, and I. Rasul, 2005. Social Preferences and the Response to Incentives: Evidence from Personnel Data, *Quarterly Journal of Economics*, pp. 917-962.
4. Barberis, N., A. Shleifer, and R. Vishny, 1998. A Model of Investor Sentiment, *Journal of Financial Economics*, pp. 307-343.
5. Benabou, R., and J. Tirole, 2002. Self-Confidence and Personal Motivation, *Quarterly Journal of Economics*, pp. 871-915.
6. Biais, B., D. Hilton, K. Mazurier, and S. Pouget, 2005. Judgmental Overconfidence, Self-Monitoring and Trading Performance in an Experimental Financial Market, *Review of Economic Studies,* pp. 287-312.
7. Camerer, C., G. Loewenstein, and D. Prelec, 2004. Neuroeconomics: Why Economics Needs Brains, *Scandinavian Journal of Economics,* pp. 555-579.
8. Choi, J., D. Laibson, and B. Madrian, 2004. Plan Design and 401(K) Savings Outcomes, *National Tax Journal,* pp. 275-98.
9. Crawford, V., and N. Iriberri, 2005. Level-k Auctions: Can a Non-equilibrium Model of Strategic Thinking Explain the Winner's Curse and Overbidding in Private-Value Auctions? *Mimeo*.
10. de Quervain, D., U. Fischbacher, V. Treyer, M. Schellhammer, U. Schnyder, A. Buck, and E. Fehr, 2004. The Neural Basis of Altruistic Punishment, *Science,* pp. 1254-1258.
11. DellaVigna, S., and U. Malmendier, 2006. Paying Not to Go to the Gym, *American Economic Review*. pp. 694-719.
12. Ellison, G., 2005. Bounded Rationality in Industrial Organization, *Mimeo*.
13. Ellison, G., and D. Fudenberg, 1993. Rules of Thumb for Social Learning, *Journal of Political Economy*, pp. 612-643.
14. Ellison, G., and S. Fisher-Ellison, 2004. Search, Obfuscation, and Price Elasticities on the Internet, *Mimeo*.
15. Eyster, E., and M. Rabin, 2005. Cursed Equilibrium, *Econometrica*, pp. 1623-1672.
16. Fehr, E., and K. Schmidt, 1999. A Theory of Fairness, Competition, and Cooperation, *Quarterly Journal of Economics*, pp. 817-868.
17. Fehr, E., U. Fischbacher, and M. Kosfeld, 2005. Neuroeconomic Foundations of Trust and Social Preferences: Initial Evidence, *American Economic Review,* pp. 346-351.
18. Fudenberg, D. D. Kreps, 1993. Learning Mixed Equilibria, *Games and Economic Behavior,* pp. 320-367.
19. Fudenberg, D., 2006. Advancing Beyond Advances in Behavioral Economics, *Journal of Economic Literature*, pp. 694-711.
20. Gale, J., K. Binmore, and L. Samuelson, 1995. Learning to Be Imperfect: The Ultimatum Game, *Games and Economic Behavior*, pp. 56-90.
21. GuI, F., and W. Pesendorfer, 2001. Temptation and Self-Control, *Econometrica,* pp. 1403-1435.
22. Haruno, M., T. Kuroda, K. Doya, K. Toyama, M. Kimura, K. Samejima, H. Imamizu, and M. Kawato, 2004. A Neural Correlate of Reward-Based Behavioral Learning in Caudate Nucleus: A Functional Magnetic Resonance Imaging Study of a Stochastic Decision Task, *Journal of Neuroscience*, pp. 1660-1665.
23. Henrich, J., R. Boyd, S. Bowles, C. Camerer, E. Fehr, and H. Gintis, 2004. *Foundations of Human Sociality: Economic Experiments and Ethnographic Evidence from Fifteen en Small-Scale Societies.* Oxford and New York: Oxford University Press.
24. Hossain, T., and J. Morgan, 2006. Plus Shipping and Handling: Revenue Equivalence in Field Experiments on eBay, *B.E. Journals in Economic Analysis and Policy: Advances in Economic Analysis and Policy*.
25. Jehiel, P., 2005. Analogy-Based Expectation Equilibrium, *Journal of Economic Theory*, pp. 81-104.
26. Kahneman, D., 1999. Objective Happiness, In *Well-Being: The Foundations of Hedonic Psychology*, ed. D. Kahneman, E. Diener, and N. Schwarz. New York: Russell Sage Foundation, pp. 3-27.
27. Kamenica, E., 2006. Contextual Inference in Markets: On the Informational Content of Product Lines, *Mimeo*.

28. Kimball, M., and R. Willis, 2006. Utility and Happiness. *Mimeo*.
29. Machina, M., 1984. Temporal Risk and the Nature of Induced Preferences, *Journal of Economic Theory*, pp. 199-231.
30. McClure, S., D. Laibson, G. Loewenstein, and J. Cohen, 2004. Separate Neural Systems Value Immediate and Delayed Monetary Rewards, *Science*, pp. 503-507.
31. Muraven, M., and R. Baumeister, 2000. Self-Regulation and Depletion of Limited Resources: Does Self-Control Resemble a Muscle? *Psychological Bulletin*, 126(2): 247-59.
32. O'Doherty, J., 2004. Reward Representations and Reward-Related Learning in the Human Brain: Insights from Neuroimaging, *Current Opinion in Neurobiology,* pp. 769-776.
33. O'Donoghue, T., and M. Rabin, 2001. Choice and Procrastination, *Quarterly Journal of Economics,* pp. 121-160.
34. Padoa-Schioppa, C., and J. Assad, 2005. Neuronal Processing of Economic Value in Orbitofrontal Cortex, *Mimeo*.
35. Platt, M., and P. Glimcher, 1999. Neural Correlates of Decision Variables in Parietal Cortex, *Nature*, pp. 233-38.
36. Rabin, M., 2002. Inference by Believers in the Law of Small Numbers, *Quarterly Journal of Economics*, pp. 775-816.
37. Rabin, M., and J. Schrag, 1999. First Impressions Matter: A Model of Confirmatory Bias, *Quarterly Journal of Economics*, pp. 37-82.
38. Schlag, K., 1998. Why Imitate, and If So, How? A Boundedly Rational Approach to Multi-Armed Bandits, *Journal of Economic Theory*, pp. 130-156.
39. Shafir, E., P. Diamond, and A. Tversky, 1997. Money Illusion, *Quarterly Journal of Economics*, pp. 341-374.
40. Shapiro, J., 2005. A Memory-Jamming Theory of Advertising, *Mimeo*.
41. Singer, T., S. Kiehel, J. Winton, R. Dolan, and C. Frith, 2004. Brain Responses to the Acquired Moral Status of Faces, *Neuron*, pp. 653-662.
42. Smallwood, D., and J. Conlisk, 1979. Product Quality in Markets Where Consumers Are Imperfectly Informed, *Quarterly Journal of Economics*, pp. 1-23.
43. Stigler, G., 1965. The Development of Utility Theory, In *Essays in the History of Economics*. Chicago: University of Chicago Press.
44. Tversky, A., and D. Kahneman, 1974. Judgment under Uncertainty: Heuristics and Biases, *Science*, pp. 1124-1131.
45. Ward, A., and T. Mann, 2000. Don't Mind If I Do: Disinhibiting Eating under Cognitive Load, *Journal of Personality and Social Psychology*, pp. 753-763.
46. Wilson, T., J. Myers, and D. Gilbert, 2003. How Happy Was I, Anyway: A Retrospective Impact Bias, *Social Cognition,* pp. 421-446.

NOTES