

The case for mindful economics

Colin Camerer
HSS Caltech 91125
camerer@hss.caltech.edu

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“The besetting fallacy of writers on economic method has been justly said to be the fallacy of exclusiveness. A single aspect or department of economic study is alone kept in view, and the method appropriate thereto aggrandized, while other methods, of equal importance in their proper place, are neglected or even explicitly rejected. Hence, the disputants on both sides, while rightly positive, are wrong negatively. Their criticisms on rejected methods are, moreover, too often based on misapprehension or misrepresentation.” [John Neville Keynes, 1891, pg 10]

1. Introduction

This paper is about how ideas and measurements from psychology and neuroscience (behavioral economic and neuroeconomics, respectively) might be generally incorporated into theories of economic choice. I define economics as the study of the variables and institutions that influence “economic” choices, choices with important consequences for health, wealth, and material possession and other sources of happiness.

The term “mindful economics” (hereafter, neuroeconomics) serves as a counterpoint to the language of Gul and Pesendorfer (GP) [this volume] who call the revealed-preference, rational-choice approach “mindless economics”. I encourage readers to read both their essay and mine consecutively, in either order.

Neuroeconomics is a specialization of behavioral economics that plans to use neural data to create a mathematical and neurally disciplined approach to the microfoundation of economics. Data from brain imaging attract the most attention, but it is crucial to note that neuroscientists also use animal studies, behavior of patients with permanent lesions and temporary disruption or stimulation (TMS), response times,¹ tracking eye movements to measure information acquisition, psychophysiological measures (skin conductance, pupil dilation etc.), and computational modeling. The variety of complementary approaches usually means that an obvious limit of one method can be overcome by another method. So one should not be too quick to criticize the flaws of any single method without considering whether another method overcomes those flaws.

Note that the behavioral approach should ideally fully encompass rational choice approaches as a special case. Keep in mind that behavioral economists *do not* doubt that incentives matter and *do not* believe that traditional analysis is useless. (As Spiegler [this volume] argues eloquently, there will always be a role for careful grounding of functional forms in choice-theoretic axiomatization to reveal all the implications—including hidden predictions—of those functional forms.) Indeed, since behavioral economics is meant to be a generalization of rational choice theory which incorporates limits on rationality, willpower and self-interest in a formal way. These generalizations allow the possibility that conventional rationality is an adequate approximation, and often permit a parametric way to measure the “degree” of limitedly rational behavior and its economic impact.

The paper develops my latest view about grounding economic choice in neural details. This perspective is developing rapidly. As a result, viewpoints expressed only a couple of years ago are updated and informed by the latest data and perspective on methods. Revision of viewpoints, particularly the details of language and its sweep, is common and desirable in empirical science as new data and methods arrive. Early neuroeconomics papers which describe ideas and potential discoveries [such as Camerer, Loewenstein and Prelec, 2005] should therefore not be viewed as logical conclusions derived from mathematical analysis. These early neuroeconomics papers

¹ Economists also use response times, e.g. to infer the depth of strategic thinking [Rubinstein, 2006].

should be read as if they are speculative grant proposals which conjecture what might be learned from studies which take advantage of technological advances.

The paper is also a positive rebuttal of the “case for mindless economics” in the form of the case for mindful economics (i.e., its potential). Their paper consists primarily of two arguments. The first is simply a fundamentalist definition of economics which excludes non-choice data and limits the role for psychological facts by appeal to the claim that there are differences in tastes and interests between the two fields. This argument is simply a definition of economics as inherently mindless, and there is no debating a definition. The definition simply draws a preferred boundary rather than makes an evidentiary “case” for mindless economics.

The second, much more interesting, argument is that rational choice theory is sufficiently flexible to explain behavioral anomalies using the conventional language of preferences, beliefs and constraint. This second argument is worthy of discussion. Indeed, my view is that conventional economic language can indeed approximate a lot of neural phenomena (which, if true, undermines the argument that the two approaches are fundamentally incompatible). But at some point it is more efficient to simply adopt constructs as they are defined and understood in other fields, because defining those constructs in economic language is clumsy.

I. The case for mindful economics

Keep in mind throughout that my assumed goal is that economics should make predictions about important choices and say something disciplined about the welfare implications of policies.

One way to make predictions is to look at past choices, and use those data to specify functions which express an unobservable basis of choice (e.g. utilities and beliefs), in order to predict how choices might respond to changes in observable variables. “Mindless economics” (a/k/a rational choice theory or revealed preference) relies solely on observed choice patterns and mathematical restrictions on what choice patterns imply about underlying functional representations that are predictively useful.

Neuroeconomics has the same aspirations. Neuroeconomics is not in opposition to rational choice theory, but sees potential in extending its scope by observing variables that are considered inherently unobservable in rational choice theory. As Glimcher and Rustichini [2004] put it, the goal of neuroeconomics is a “mathematical, behavioral, and mechanistic” account of choice. What their definition adds to rational choice theory is the mechanistic component. So the goals of neuroeconomics are not fundamentally different than those of rational choice theory, since neuroeconomics strives to link mathematical formalisms and observed behavior just as rational choice theory does. The central issue is therefore whether having a mechanistic basis will improve the capacity to understand and predict choice, while maintaining a mathematical discipline and use of behavioral (choice) data.

Inferring preferences from observed choices also has limits. For example, an important problem for companies and regulators is forecasting demand and welfare consequences of introducing new products. By definition, a rational choice theory which relies on previously observed choices of old products is limited in its capacity to forecast behavior toward new choices (particularly those which do not share a lot of attributes with previous products, such as some new gadgets, genetically engineered foods, and so forth). Policymakers who decide whether to permit a new product must also forecast how much consumers will buy and whether the product will work. They cannot rely on observed choice data. It is conceivable that a neuroeconomic model of

preference could add to extrapolations from similar old products in forecasts of demand for new products.

One way to see the potential for neuroeconomics is by analogy to organizational economics (cf. Sanfey, Loewenstein, Cohen, and McClure, 2006). Until the 1970's, the theory of the firm was a reduced-form model of how capital and labor are combined to create a production function. The idea that a firm just combines labor and capital is obviously a gross simplification— it neglects the details of principal-agent relations, gift exchange and efficiency wages, social networks and favor exchange in firms, substitution of authority for pricing, corporate culture, and so forth. But the gross simplification is useful for the purpose of building up an industry supply curve.

Later, contract theory opened up the black box of the firm and modelled the details of the nexus of contracts between shareholders, workers and managers. The new theory of the firm replaces the (perennially useful) fiction of a profit-maximizing firm which has a single goal, with a more detailed account of how components of the firm— individuals, hierarchies, and networks – interact and communicate to determine firm behaviour.

Neuroeconomics proposes to do the same by treating an individual economic agent like a firm. The last sentence in the previous paragraph can be exactly rewritten to replace firms and individual agent the components of firms with individuals and neural components of individuals. Rewriting that sentence gives this one: The *neuroeconomic* theory of the *individual* replaces the (perennially useful) fiction of a *utility-maximizing individual* which has a single goal, with a more detailed account of how *components* of the *individual*— *brain regions, cognitive control, and neural circuits* – interact and communicate to determine *individual* behaviour.

The case for the potential of mindful economics rests on several principles, which will each be discussed in turn:

- The brain is the organ that makes choices
- More will be known about the brain due to technological advances
- Sciences should respond to technological advances
- Neuroeconomics can use technological advances in understanding the choice-making organ (the brain) to find non-price neural and psychological variables that predict and change economic choices
- Rational choice theory can be enriched by new psychological constructs
- Behavioral economics and neuroeconomics could lead to improvements in welfare economics
- Drawing sharp boundaries between fields is difficult and, fortunately, is not necessary

A. The brain is the organ that makes choices

Every economic choice (even institutional choices) depends on an individual saying “Yes” , nodding, handing something to a cashier, signing a contract, reaching into a wallet, clicking “submit” online, releasing an earnings announcement, or executing some other action that requires brain activity. In this sense, all economic activity flows through the brain at some point. Even economic institutions rely on expectations, sometimes mystical ones like credibility of Fed governors or consumer confidence, which exist in the brain. So it is hard to imagine that understanding brain function could not be useful for understanding some aspects of economic choice and its aggregated consequences.

Of course, it is often useful to abstract from these details and posit a higher level abstraction (e.g., utility maximization) which is a reduced-form representation of some neural process. The value of such abstractions—which is beyond dispute— does not imply that unpacking the reduced form couldn't be valuable too.

Indeed, one argument for the use of neural data is that economic theorists are almost too good at rapidly producing sensible characterizations of simple behavioral regularities based on different axiomatic systems (the supply of theories outstrips the supply of diagnostic data). Experimental evidence of ambiguity aversion, for example (à la the Ellsberg paradox), has given rise to about a dozen different reduced-form theories. All these theories can explain the basic Ellsberg patterns. How do we choose among them? A bad way is by weighing informal opinions about plausibility or reasonableness of the underlying axioms. A slightly better way is a new wave of experimentation designed to distinguish among competing theories (which is laborious, but certainly useful; e.g. Halevy 2007). An even better way, which is even more laborious, is to apply these different theories to models of contracting, asset pricing, consumer choice, and so forth, and see which theories can best explain existing stylized facts and accurately predict surprising new patterns.

Another way is to assume that if two theories can both explain the Ellsberg paradox, and appear equally promising for explaining some pattern in, say asset prices, then if one of the theories also is neurally plausible that theory should be taken more seriously. This criterion is a simple application of the idea that the theory which can explain the most data (especially data which discriminate theories strongly) wins incumbency. Essentially, neuroeconomists are betting that in some cases, neural tests² could winnow a crowded field of possible theories down to the more plausible theories, and that doing so economizes on the hard work of figuring out all the implications of the different theories for different domains, such as asset pricing, and testing them.

B. More will be known about the brain due to technological advances

While journalists might inadvertently do so, it is hard to exaggerate the genuine scientific advances in understanding brain function in recent years. An important part of the advances is that many tools are developing at the same time. These include tools from several different disciplines including genetics, psychology, biology, and computer science. For example, the ability to map animal and human genomes and correlate them with phenotypic behavior is enormous. Genes can be easily manipulated in mice; so we can “knock out” a gene and see exactly which tasks require that gene; doing so is an important clue about the gene's expression and function. fMRI brain imaging is only about 10-15 years old and data are accumulating at a rapid pace. Diffusion tensor imaging (DTI) is showing more and more about connectivity of brain regions. Transcranial magnetic stimulation (TMS) and administration of drugs and synthetic hormones permit causal experiments in which brain areas or neurotransmitters are disrupted or stimulated. What happens when area X is disrupted, for example, can establish the necessity of X for various tasks.

In addition to technological advancements, simple methodological improvements have advanced our understanding of choice on a more basic level. This is particularly apparent in the study of childhood development, which is important to adult choice behavior because of the path

² Keep in mind that I am not referring to fMRI alone, which is admittedly noisy and still developing rapidly (it is only a little more than 10 years old), but instead to the combination of complementary methods including fMRI, lesion patient tests, eyetracking, and causal manipulations like priming and TMS.

dependence and irreversibility of development. Data bases of patients with lesions in specific areas are also growing; their growth permits a jump from tiny to modest samples of patients with damage in specific areas. Just as with TMS and stimulation inference, what patients with damage in area X cannot do (compared to matched controls) tells us what area X is necessary for.

C. Sciences should respond to technological advances

Industries take advantage of new technologies and substitute resources into them and away from less relatively valuable technologies. Science is the same. The boundaries of biology, astronomy, and neuroscience were shifted and expanded by the microscope, telescope and satellites, and neuroscientific tools. If choice occurs in the brain, there should be some degree of substitution into tools that can understand the brain, to predict choice. The range of tools described above also implies that an interest in neuroeconomics is not a speculation about brain imaging or any other single tool since all tools are being explored, linked and improved.

A milder way to put this argument, as one prominent economist put it, is that neuroeconomics has option value. The very fact that its potential payoff has variance increases option value.

D. Neuroeconomics can use technological advances in understanding the choice-making organ (the brain) to find non-price neural and psychological variables that predict and change economic choices

Arguments A-C above suggest it is conceivable that something could be learned about economic choices from recently developed neuroscientific measurements. In fact, there are already many examples of where psychological or neural measures either predict later choices, or actually influence choices causally.

In their paper GP write: “The authors [Camerer, Loewenstein, and Prelec 2004] (hereafter, CLP) have no example of observing a choice parameter—such as the coefficient of relative risk aversion or the discount factor—through brain imaging, no suggestions as to how such inference could be done.”

GP are simply wrong. CLP [2004] did cite³ a paper containing an example, a paper by Hsu, Bhatt, Adolphs, Tranel, and Camerer [2005] which reported an inferential procedure (a clear “suggestion”) and was already in press when GP’s paper was first circulated. In that study, a choice parameter expressing the degree of ambiguity aversion was inferred from subjects’ choices for money. Those parameter estimates were correlated (across subjects) with activity in right orbitofrontal cortex (OFC) observed in fMRI brain imaging: That is, subjects with low parametric ambiguity aversion had less activity in OFC. So GP’s “no example” criterion was refuted in print just days after their paper was circulated. Furthermore, extrapolating the correlation between the choice parameter and brain imaging activity to patients with no activity in the relevant area (OFC) implied that patients with lesions in that area would behave as if they had a particular numerical parameter (.85). Later experiments with those patients based on their choices yielded a numerical estimate of .82. So the fMRI measurement delivered a choice parameter value which actually predicted later choices of a separate group of subjects.

³ It is true, however, that CLP did not specifically discuss the choice parameter/imaging link in their brief passage, but they did describe a paper containing such an example.

Furthermore, there are several more studies linking choice parameters inferred from behavior with neural processes. Tom, Craig, Trepel, and Poldrack [2007] and Sokol-Hessner, Delgado, Hsu, Camerer, and Phelps [2007] infer loss aversion parametrically from actual choices and correlate the parameter value with fMRI signals and skin conductance, respectively. Plassman, O'Doherty and Rangel [2007] correlate medial orbitofrontal cortical (OFC) activity with free bids for consumer goods. Kable, Louie and Glimcher [in press] correlate neural firing rates with discount rates inferred from choice. Hampton, Bossaerts and O'Doherty [2006] correlate activity with inferred prior beliefs and outcomes in model of learning about a Markov reward process. Knutson, Rick, Wimmer, Prelec, and Loewenstein [2007] find that neural activity helps (albeit modestly) predict of actual purchases of consumer goods, beyond stated preferences and stated consumer surplus. Many more examples are emerging rapidly.

Listed below are some other examples of non-price psychological and neural variables that affect choice. It is certainly true that many of these phenomena can be translated into conventional economic language. Indeed, a major contribution of neuroeconomics may be to provide a formal language for talking about these phenomena. As I argue below, however, other phenomena are more clearly understood by adopting new terms from psychology and neuroscience rather than struggling to fit the brain's complex activity awkwardly into the Procrustean bed of economic language.

It is useful to sort the examples into those which are close to how economists reason, and easily translated into economic language (the first three examples), those which illustrate how a psychological measure could improve upon theory testing (example 4), and several (examples 5-12) which show the causal effect of psychological or neural variables or treatments which appear to be distant from economic analysis and of special interest only to neuroscientists.

Examples which are easy to explain in economic terms:

1. **Gender of children influence parents' political preferences:** Washington [2006] finds that parenting an additional daughter increases the likelihood that legislators will vote for reproductive rights. What makes this result interesting is that gender is largely exogenous (female-minded fathers cannot order up girl babies instead of boys). Of course, the fact that parents' preferences are influenced by their children is hardly surprising or uneconomic; however, the details of how that process works could be illuminated further by understanding the neurobiology of parent-child attachment.
2. **Alcohol increases restaurant tipping:** Using field data, Conlin, Lynn, and O'Donoghue [2003] found that consuming any alcohol at a restaurant increased the tip percentage by about 2% (e.g. tips go from 13 to 15% of the bill). The number of times the patron visits a particular restaurant (a crude measure of reputational effects) increased tipping by .2% per visit. Drinking alcohol has an effect equivalent to 10 trips per month (and the alcohol variable is also statistically more significant).
3. **Verbal labeling of "mad cow disease" changes behavior:** Eating meat from cows infected with "bovine spongiform encephalopathy" (BSE) appears to have caused a couple of hundred cases of a variant of Creutzfeldt-Jacob disease in humans. When media outlets began to describe the cows' disease as "mad cow disease", rather than as BSE, people began to eat less beef [Sinaceur and Heath, 2005]. Lab experiments also show stronger emotional reactions and less rational reaction than when the scientific BSE label was used.

These three examples above are merely correlations between economic or political choices and interesting variables that are not obviously price, income, or information. However, it is easy to quickly describe these phenomena in the language of preferences, beliefs and constraints. We could infer from the data, for example, that alcohol and tipping must be complements, that parent preferences depend on child characteristics, or that relabelling bovine spongiform encephalopathy as “mad cow disease” genuinely conveys new information (doubtful) or grabs attention and activates emotion (more likely).

The fourth example is:

4. **Pupil dilation predicts deception in cheaptalk games:** Wang, Spezio, and Camerer [2006] record increases in pupil diameter (pupil dilation) and attention in a sender-receiver game of strategic information transmission. Pupil dilation is correlated with the amount of deception (the difference between the sender’s message and his or her privately known state). Pupil dilation and attention together have statistical value for predicting the state from the message.

This example is different from the previous three: It illustrates how a psychophysical measure might help differentiate theories. Pupil dilation is an involuntary (autonomic) response which is well known to be linked to cognitive difficulty and arousal. The advantage of an autonomic response is that it can be measured rapidly (many times per second) and it is more reliable than measures like subjective reports. (People might, for example, say they find a task easy or an image unarousing when their pupil dilation indicates otherwise.) For the purposes of studying strategic information transmission, measures like these (ideally, in conjunction with eyetracking) might enable us to separate theories which are importantly different but have similar observable implications. For example, the experimental data suggest that subjects without much experience seem to transmit too much information compared to equilibrium predictions (i.e., they are too honest) [Radner and Schotter 1989, Valley, Thompson, Gibbons, and Bazerman 2002, Cai and Wang, 2006]. One explanation is rooted in social preference or social image (e.g., they feel guilty lying). Another is that figuring out how much to deceive is cognitively difficult. A combination of looking at the payoffs of others, and dilation of pupils upon deception is consistent with the guilt explanation. Pupil dilation without looking at how much other players get is more consistent with the cognitive difficulty explanation. Based on the data, Wang, Spezio, and Camerer endorse the cognitive difficulty explanation but their conclusion is tentative.

Of course, a long string of careful experiments manipulating experimental displays and information treatments could also separate theories without using pupil dilation measures. But the marginal cost of those measures is essentially zero. Recording pupil dilation can only speed up the process of inference from an experimental program, and could do so rapidly.

The next eight examples all involve causal influences on choices:

5. **Childhood brain development creates adult human capital:** Knudsen, Heckman, Cameron and Shonkoff [2006] write: “...studies of human capital formation indicate that the quality of early childhood environment is a strong predictor of adult productivity and that early enrichment for disadvantaged children increases the probability of later economic success. Although explanatory mechanisms for interpreting these correlations still are being developed, recent advances in neuroscience are illuminating because they demonstrate the extent to which

experience influences the development of neural circuits that mediate cognitive, linguistic, emotional and social capacities” (p. 10155).

6. **Disruption of brain activity increases ultimatum-offer acceptance:** Using fMRI, Sanfey, Rilling, Aronson, Nystrom, and Cohen [2004] found activity in dorsolateral prefrontal cortex (DLPFC) associated with evaluation of unusually low offers in ultimatum games. Building on this observation, Wout, Kahn, Sanfey, and Aleman [2005] and Knoch, Pascual-Leone, Meyer, Treyer, and Fehr [2006] used repetitive TMS (magnetic stimulation that temporarily disrupts brain activity) to deactivate the DLPFC when people received offers. Disrupting the DLPFC increased acceptances of low offers. A key point here is that you would not know which area to disrupt, in order to increase acceptances, without the geographical specificity of brain activity that comes from fMRI.
7. **Cognitive load reduces resistance of temptation:** Shiv and Fedorhokin [2002] find that when subjects are under cognitive load (having to remember a seven digit number rather than a two-digit one) their ability to resist a tempting snack falls.
8. **“Cognitive re-appraisal” of gamble outcomes reduces parametric loss-aversion:** Sokol-Hessner, Delgado, Hsu, Camerer, and Phelps [2007] instructed subjects to “cognitively reappraise” gamble choices (by imagining they are traders making choices for others, and that they make choices often). During reappraisal the degree of loss aversion inferred parametrically from actual choices for money drops. Changes in skin conductance are weakly correlated with changes in inferred loss aversion.
9. **Subconscious exposure to happy and angry faces influences demand for liquid:** Hundreds of studies show that ‘priming’ (subconscious exposure of subjects to semantic or visual stimuli) can change behavior in remarkable ways. Niedenthal, Barsalou, Winkielman, Krauth-Gruber, and Ric [2005] found that showing angry versus happy faces increased the amount thirsty subjects would pay to drink a small amount of liquid.
10. **Identity priming affects test scores:** A large, fast growing body of research shows that “priming” behavior by exposing people to words and images, can affect later behavior. For example, asking questions related to an Asian female’s ethnic background before a math test increases her actual test scores; asking gender-related questions decreases test scores [Shih, Pittinsky, and Ambady, 1999].⁴
11. **Disgust erases and sadness reverses buying-selling price gaps:** Lerner, Small and Loewenstein [2004] showed subjects four minute film clips which reliably induce sadness, disgust, or neither emotion. Disgust erases the typical gap between buying and selling prices for goods (a highlighter set) and sadness reverses it (buying prices go up, à la “retail therapy”).
12. **Oxytocin causes trust:** Oxytocin is a powerful hormone in social bonding. Kosfeld, Heinrichs, Zak, Fischacher, and Fehr [2005] had subjects play a trust game in which one player could choose whether to invest money or keep it, and another player decided how much to repay from the tripled investment amount. Subjects who were given a synthetic dose of oxytocin trusted more (compared to a placebo control group and a random risky choice control group).

⁴ These priming effects do tend to wear off rapidly and have not yet been shown experimentally to cause very consequential behavior, although my sense is that most researchers in the field are optimistic that they can do so.

Before proceeding, first note that some of these effects are large and some are small; some represent a single intriguing finding (e.g. TMS disruption causes acceptance of low ultimatum offers) and others are examples of a well established phenomenon that is related to economic choice (e.g., cognitive reappraisal) This list is also just the tip of a scientific iceberg, which is expanding rather than melting. The point of the list is that the dependent variables are all either economic choices for money or goods, or other outcomes that economists study,⁵ the list is not short. All the examples also show causation of choices in neurally interpretable ways, with variables that are not prices, income, or information. This is challenging for the conventional view of stable preference. A narrow way out is to infer that the causal variables must be providing information, then invent exotic types of information to explain the causal effects.⁶

Admittedly, some of these phenomena can certainly be understood by economic theories which stretch the language only a little. For example, the effect of identity priming on test scores can be expressed in a model where the priming influences belief of success and people have a preference to behave consistently with that belief. That is roughly how the psychologists characterize the phenomenon too, and is similar in structure to Benabou and Tirole's [2003] model of crowding out of intrinsic motivation [and cf. Benabou and Tirole 2006].

Other phenomena are sensibly described as expressions of state-dependent preferences, where states include "states of mind" influenced by hormones, cognitive disruption, cortical damage, induced emotions, and other forces. This interpretation raises the question of which state variables matter, and whether states can be self produced, overridden, and so forth. If we are to make progress in understanding the nature of state dependence it is hard to imagine we can do so without knowing some facts and ideas from psychology, evolutionary modeling, and neuroscience.

Even the examples which seem to only narrowly interest neuroscientists are tools for creating neuroeconomic theory which should eventually have broader implications. Consider the causal influence of oxytocin on trust. Many observable social behaviors (e.g. touch) can generate oxytocin. It could be, for example, that business practices which emphasize socializing and getting to know one another before dealmaking have to do with production of oxytocin or some other biochemical process that genuinely increases trust. Hormones like oxytocin often operate differently across people and across the life cycle. For example, testosterone (T) drops with age. If T is linked to an economic behavior (e.g., violence), and the behavior is correlated with an observable like age, then understanding of T unpacks the reduced form model of age correlating with violence. Put differently, if one could measure T and include it in regressions predicting violence, it might be that age effects disappear and are shown to be driven by T. Similarly, the studies of subconscious face priming and sadness and disgust effects on prices (examples 9 and 11) are simply illustrations of how emotional state variables can influence choice subconsciously. The subconscious influence is a reminder that decisions may be influenced by exogenous non-price variables we are not aware of (or variables we are aware of, but whose

⁵ E.g., labor economists are interested in test scores and human capital, and public health economists are interested in smoking choices.

⁶ This strategy is what doomed the behaviorist emphasis on reinforcement as an all-purpose theory of complex human behavior. Evidence emerged that rewarding behaviors emerge spontaneously without any reinforcement (e.g., children learn to utter phrases without being directly reinforced). So behaviorists would infer that a condition must be a reinforcer if its presence predicted behavior (e.g., children are reinforced for imitating parental speech and imitation leads to their own accurate speech). The business of inferring what must have been reinforcing after observing the behavior became more and more contrived and was gradually supplanted by the cognitive processing paradigm and later waves which were more fruitful and disciplined.

effects we believe are overcome). In thinking about field phenomena, the analog to a study like the sadness-disgust study is find some observable event or variable which is likely to induce emotion reliably and study its effects [e.g., Hirshleifer and Shumway [2003] find that sunshine is correlated with stock returns].

E. Rational choice theory can be enriched by new psychological constructs

The most useful debate between the rational-choice mindless approach and the mindful approach is how sensibly psychological and neural phenomena can be characterized by the language of preferences, beliefs (especially information imperfections) and constraint. GP argue that

“The methods of standard economics are much more flexible than it is assumed in the neuroeconomics critique and [we] illustrate this with examples of how standard economics deals with inconsistent preferences, mistakes, and biases” (p xx.)

First, as a description of how neuroeconomists think, they are wrong. It is impossible to have an economics or business Ph.D. from the University of Chicago (as I do) and not know the rational model is flexible.⁷ Behavioral economists are also reminded of its flexibility constantly in referee reports and seminar comments. The issue is whether there are any phenomena that are better described by simply importing language and constructs from other disciplines as needed rather than mixing and matching familiar language. After all, economics doesn't have a name for everything in the world. And we have adopted other language when it is useful to do so (e.g., “laissez-faire” or “tâtonnement”).

Furthermore, if it is true that the language of preference, belief and information, and constraint can characterize “inconsistent preferences, mistakes and biases” then this is great news for psychology and neuroscience. It means there is some prospect for an increase in common language despite GP's insistence (discussed further below in point F) that the tastes and motives in disciplines are fundamentally different.

Besides the examples in the last section, I will discuss three phenomena—cues, Stroop mistakes, and emotional regulation-- and suggest the mixture of rational-choice language and new language that best describes them:

Cues: In the addiction literature a “cue” is a sensory stimulus which triggers a drug craving because the cue was learned to be associated with drug use in the past (e.g. walking past a neighborhood where a recovering junkie shot up heroin; the neighborhood is the cue). GP state that, “For economists, the notion of a cue is not useful because it lumps together two distinct economic phenomena”. I think the opposite is true— precisely because there is no special word in economics language for a good or state variable that is both complementary and (potentially) external. It might be useful to have such a word, if the goal is to predict addict behavior and also think about policy. Furthermore, cues have other properties: Typically, cue effects can be extinguished with repeated exposure (this is a common basis of therapies), but can also be rapidly reinstated. Cues also are typically asymmetric—that is, seeing “Scarface” might increase demand for cocaine, but ingesting cocaine does not create demand for seeing “Scarface”. So we *could* adapt the language of economics to describe “cues” as “dynamically adaptive, rapidly reinstatable asymmetric

⁷ As a graduate student I recall overhearing a late-night conversation in the library between an enthusiastic student and his apparent girlfriend. He explained that he loved her because their utility functions were interdependent. Using the flexible rational model, I inferred from her subsequent choice that she preferred hearing more poetic language.

complements to consumption”. Or we can just learn a new vocabulary word— “cue” which summarizes certain kinds of complements.

Stroop mistakes: Take the example of American tourists crossing the street in London, who often look in the wrong direction, to the left (the direction familiar from American driving, in which cars drive on the right hand side of the road), which leads to pedestrian accidents. GP explain this in the language of standard economics by saying that

“the strategy ‘only cross the street when no car is approaching’ may be unavailable in the sense that it violates a *subjective constraint* on the set of *feasible* strategies” (italics mine).

This explanation puts a strain on words like “constraint” and “feasible”. A dictionary definition⁸ of feasible closest to what GP seem to have in mind is “feasible: capable of being accomplished or brought about; possible”. Is looking to the right really impossible? If the visiting American wore a neck brace (from an injury) which made it excruciatingly painful to look to the right, then a term like “feasible” or “constraint” would be appropriate. But is looking to the right really not “feasible” (not in the choice set)?

Psychology has an ideal term for precisely this kind of mistake—a “Stroop mistake”. In 1935 John Stroop created a task⁹ to measure mental flexibility. He asked subjects to rapidly name the color of ink a word is printed in. When the word “green” is printed in black ink, for example, many subjects rapidly say “green” and then correct themselves.

“Stroop task” is now a generic term for any choice in which there is an automatic, highly practiced response which is incorrect, and which must be inhibited or controlled by a slower deliberative process. (Another famous example is the game “Simon says”, in which people must perform physical actions only if the commands are preceded by the phrase “Simon says”.) Americans looking for cars in London are performing a Stroop task.

To an economist, a natural way to describe a Stroop task is that one element in the choice set is chosen automatically unless some scarce cognitive resources are expended to override it. A model like Samuelson’s [2001] model of overadaptation to familiar tasks, or a variant of Fudenberg and Levine’s [2006] planner-doer model in which the planner must incur utility costs to restrain the doer, are probably the right sorts of models. GP’s use of the phrase “subjective constraint” is on the right track— except the subjectivity can only be fully understood by thinking about the psychology and doing experiments.

The reason why I am resisting language like “feasible strategy” to explain Stroop mistakes is that experimental data suggest some other interesting regularities which are hard to accommodate in an explanation grounded purely in feasibility. For example, when subjects do Stroop tasks over and over, they get better at them (essentially, the correct response becomes more automatic). A process of learning the correct default (or the state of nature which makes one response optimal) is needed to explain this, so you need a learning component which, in the translated language, makes strategies more feasible or less subjectively constrained.

Another fact is that Stroop mistakes are sensitive to cognitive overload, fatigue, and other variables. For example, mountain climbers at high altitude probably make more Stroop-like mistakes, often leading to death.¹⁰ A full model would include therefore biological variables like

⁸ <http://www.bartleby.com/61/8/F0060800.html>

⁹ <http://www.snre.umich.edu/eplab/demos/st0/stroopdesc.html#The%20Neurophysiology>

¹⁰ <http://www.pbs.org/wgbh/nova/everest/exposure/brainrest.html>

oxygen and visceral states (e.g., in a Fudenberg-Levine type model, the planner needs oxygen to restrain the doer.)

Based on the effect of cognitive load (illustrated by the Shiv-Fedhorikin [2002] study in the last section's long list), we could predict that Americans talking on their cell phones in the UK are more likely to fail to look in the correct direction and more likely to be. Any other variables which increase cognitive load would increase mistakes too. It is hard to see how to make sense of all these facts without concepts of learning-by-doing in the brain (the right choice becomes more automatic) and scarce cognitive resources that are required to make the right choice.

Emotional regulation: In the study by Sokol-Hessner, Delgado, Hsu, Camerer, and Phelps [2007] described in the last section, subjects made a series of choices between certain amounts and 50-50 gain-loss gambles. They were instructed at the start that any gambles they chose would be immediately played out and generate gains or losses, which would accumulate across the task and paid at the end. They were trained to turn on and off a "cognitive reappraisal", in blocks of 10 trials, which is intended to control their emotions and influence choice. The instructions are:

One way to think of this instruction is to imagine yourself a trader. You take risks with money every day, for a living. Imagine that this is your job, and that the money at stake is not yours – it's someone else's. Of course, you still want to do well (your job depends on it). You've done this for a long time, though, and will continue to. All that matters is that you come out on top in the end – a loss here or there won't matter. In other words, you win some and you lose some.

Their choice behavior is measurably different when they are thinking this way, compared to the control condition. Loss aversion parameters estimated from a standard maximum likelihood logit model are lower when they are doing the cognitive reappraisal, and their palms sweat less (a standard measure of arousal, used in lie detectors for example).

How do we explain this in standard economic language? Keep in mind that the subjects know that in every trial they are winning or losing money. So while they are simulating the idea that it is "not yours—it's someone else's" they also "know" that it is their money. The handiest conventional language explanation is that they misinterpret the instruction to mean that they won't be paid on those trials (i.e., the instruction changes their belief about payment). But they *do* know they will be paid (and tell us so).

The psychological explanation is the following: People have the capacity to imagine how they would feel and behave in different states. When they imagine these states neural activity (and skin conductance) actually change, and so does behavior. Another way to think of it in Sokol-Hessner et al.'s experiment is that they know they will be paid, but in the cognitive reappraisal they also "know" (i.e., simulate the state of knowing) that they won't be paid. Attention to the simulated state crowds out attention to the true state, which changes behavior.

In fact, this sort of imagination is used routinely in life and in economics. One approach to acting is to imagine previous experiences which produce the emotion that is desired. For example, if you imagine how you would feel if your child died, you might feel genuine sadness. This doesn't mean that you "think" your child is dead; it just means you have the capacity to do counterfactual reasoning, and that reasoning can produce powerful emotions and can change behavior.

In economics, the ability to imagine what you might do in another state is essentially assumed in game theory when there are information asymmetries (e.g., a bidder must imagine what a bidder with a different value than their own will do, in an auction, unless he or she learned an

equilibrium bidding function over time). So in economic language, we could translate the psychological concept of emotional regulation into “the use of scarce cognitive resources to self-create alternative states”. Or we can just learn some new vocabulary—cognitive reappraisal.

F. Behavioral economics and neuroeconomics could lead to improvements in welfare economics

Behavioral economics and neuroeconomics present a challenge to the conventional view that choices reveal unobservable preferences and should be the basis for welfare economics. This is an important challenge for behavioral economics, but is not one I have much to say about.¹¹

It is true that the revealed-preference approach kills two birds with one stone: By using observed choices to infer unobserved underlying preferences, and by using those choices as evidence of what people truly prefer. Behavioral economists who think choices and preferences are not always the same now must supply a theory of when they are different, and what governments should do (if anything).

The sensible route is to list defensible cases in which choices are mistakes, explain why those choices are mistakes (preferably basing the judgment that they are mistakes on a person’s own choices; e.g., Koszegi-Rabin, this volume), then explain how mistakes might be identified and avoided in a way that political and professional organizations would accept. This is where behavioral economics is likely to make some inroads (see Bernheim and Rangel, this volume; Loewenstein and Haisley, this volume).

The solution will not be as elegant and simple as the conventional view, of course. The revealed-preference approach solves the problem of figuring out when choices betray true preferences by assuming it never happens. This is like an ostrich solving the problem of escaping danger by sticking its head in the sand.

Furthermore, note well that societies already have a fabric of paternalistic interventions which reveal an implicit theory about situations in which people make bad choices that must be restricted. In most societies, those subject to paternalism include minors, the mentally ill, and in many countries, women and ethnic minorities. Behavioral economics might provide a language for characterizing the preference for these paternalistic restrictions and most importantly, passing judgment on which ones make economic sense. For example, in most American states the age of sexual consent is around 16, the voting age is 18, and the drinking age is 21. Either this composite policy arises from idiosyncracies of historical practice, interest group pressure, or local moral norms, or it reflects a coherent legal concept of human nature and the development of that nature during adolescence. It is hard to believe that adolescents are able to wisely make choices about

¹¹ A popular view is that behavioral economists are eager to regulate based on what we learn about human behavior. This view is mistaken. First, it is clearly not necessarily true that more mistakes should lead to more regulation, as Camerer, Issacharoff, Loewenstein, O’Donoghue, and Rabin [2003] pointed out and Glaeser [2006] showed more generally. Mistakes that are easily remedied by market-supplied restraint or advice might imply less regulation. Second, my own limited writing on behavioral economics and paternalism is not motivated by a desire to parent; it is motivated purely by the demand for such thinking and a feeling that it is an important professional challenge to the field (much as engineering presents the challenge of putting science to work). It is striking how interest in paternalism and regulation is described. For example, GP state that “neuroeconomists plan to enlist the support of the state—a stand-in for a benign therapist—who may, on occasion, conceal facts and make decisions on behalf of the individual’s future selves” (p. xx). This is simply false (particularly the charge that facts should be concealed). Indeed, the FTC invited several behavioral economists to an April 2007 conference on how behavioral economics might inform regulation. The “state” was trying to enlist my “support” (or at least, was interested in my ideas) rather than vice versa.

whether to have sex (which may lead to childbearing) several years before they can decide whether to have a single beer. If there is no coherent legal concept, behavioral economics might provide an improvement in coherence.

G. Drawing sharp boundaries between fields is difficult and, fortunately, is not necessary

The least interesting part of this debate is what “is economics” and “isn’t economics”. Much of GP’s paper is linguistic gerrymandering by defining economics as the revealed-preference approach, then constantly reminding the reader that anything else is, by their definition, not economics.

Furthermore, drawing sharp boundaries between academic disciplines, like other complex categories, is notoriously difficult. Precise definitions are necessary in mathematics, and the invention of abstract symbolic systems permits them. In virtually all other domains, the more important a concept is, the less simple it is to define it precisely. Is Marcel Duchamp’s notorious sculpture “*Fontaine*” “art”? (It’s just a toilet.) Is a blog “journalism”? Is the Unification Church a “religion”?

Happily, it is not necessary to have sharp categorical boundaries to answer these questions. If it is necessary to divide objects into categories for practical purposes— as a museum curator, White House credential-giver, and taxation agency must— then institutions generally develop vague categorical boundaries, then decide what is in the category and what is not on a case by case basis. The result is not like separating a set A into disjoint subsets because there is no clear separation. The result is like dividing objects into two sets with a fuzzy boundary and then debating cases that are close to the boundary to clarify the boundary. As Justice Potter Stewart put it, avoiding a precise definition of obscene material: “I know it when I see it”.

It is clearly true that researchers in different disciplines often use different tools, pose questions at different levels of analysis, and are interested in different applications. There is no doubt about this. There is also no doubt that some of what scientists do in different fields overlaps. The synthesis of neuroscientific facts and methods and economic tasks and analysis in neuroeconomics is not meant to unify the fields, but rather to improve both fields on their own terms. At the same time, our view is that some degree of shared language can’t hurt and might help. In CLP [2004, pp. 573-73] we wrote:

“It is possible that a biological basis for behavior in neuroscience, perhaps combined with all purpose tools like learning models or game theory, could provide some unification across the social sciences (cf. Gintis, 2003)”

GP disagree. They say (p. 9) that “far from being an all purpose tool, game theory is a formalism for stripping away all strategically irrelevant details of the context, details that Gintis describes as central for psychologists.”

The mild point we were trying to make is perhaps expressed better by the game theorists Sergiu Hart and Robert Aumann. In an interview Hart notes that

“This is a good point to discuss the universality of game theory. In the preface to the first volume of the *Handbook of Game Theory* [iv] we wrote that game theory may be viewed as a sort of umbrella or unified field theory. [Sergiu Hart, Sept 04 interview]”.

Aumann then adds, “It’s a way of talking about many sciences, many disparate disciplines.”

To illustrate their belief in a strict disciplinary division of labor, GP also offer an example of how different fields use different models:

“...a learning model in economics *is different than* a learning model in psychology. For an economist, a theory of learning might be a process of Bayesian inference in a multi-armed bandit model. This theory of learning is useful for addressing economic phenomena such as patent races but *may be inappropriate* for cognitive psychologists.” (p. 9, italics mine)

Just after the GP paper was circulated, neuroscientists [Daw, O’Doherty, Dayan, Seymour, and Dolan 2006] immediately proved GP wrong, by publishing a paper about neural circuitry that implements components of multi-armed bandit optimal search. So the claim that learning is modeled differently in economics and psychology is false. Similarly, reinforcement models originating in behaviorist psychology have been widely applied in economics (e.g., Erev and Roth 1998).

II. Conclusion

This paper is intended as part of a conversation about how psychological and neural measurement might inform economic theory and analysis in the long run. From papers by myself, Loewenstein and Prelec, and others, GP infer something about the beliefs and interests of neuroeconomists and compare those inferred beliefs to “economics”, by which they mean the traditional revealed-preference approach and accompanying tools and applications. Their paper does not argue against the potential of learning something from neural data, and admits to no understanding or interest in details of those data, but nonetheless quickly rules out such data as non economic as a matter of definition.

I define economics more broadly, as the study of the variables and institutions that influence “economic” (large, consequential) choices. This definition allows Jim Heckman to take neuroscientific data seriously in an attempt to explain the importance of early childhood development for human capital formation and labor economics outcomes. It also allows Vince Crawford (this volume) to measure attention directly in order to infer algorithms used when people choose strategies in games. The broader definition also includes Princeton colleagues Alan Blinder and Alan Krueger, who have both worked with non choice data,¹² to be called economists.

To reiterate my initial points above, another argument for paying some attention to psychological and neural data is technological substitution and option value. Advances in neuroscience make it possible to measure and causally manipulate many processes and quantities that were not imaginable a hundred years ago when the foundation of neoclassical economics was being laid. Quantities that were previously considered unobservable are now partially observable. (As Gabaix and Laibson (this volume) note, science has often progressed by being able to observe smaller and smaller units of analysis which are invisible to an earlier generation.) To ignore these developments entirely is bad scientific economizing. Can you imagine an astronomy profession, which spent centuries making remarkably accurate inferences by peering through increasingly

¹² Blinder, Canetti, Lebow, and Rudd [1998] is a survey of reasons for sticky pricing, and Kahneman, Krueger, Schkade, Schwarz, and Stone [2004] used survey data on subjective well-being.

sophisticated telescopes, refusing to send up planetary probes or send people to the moon because “that’s not astronomy!”?

Along these lines, a heuristic way to think of the potential of neuroeconomics is this: Economic discussions sometimes refer to “unobservables” such as beliefs, or emotions, and vague concepts like confusion (a common ‘explanation’ for experimental results that contradict theory). The presumption in neuroeconomics is that many “unobservables” are observable, in the usual sense (i.e, that strong correlates of the unobservables can be observed). That is, every time the term “unobservable” pops up, one should ponder whether that variable is truly unobservable.

For example, how might we measure confusion (rather than simply infer it from surprising behavior)? Confused subjects should take longer (or perhaps answer too rapidly) to respond. They may exhibit correlates of anxiety and cognitive difficulty, like skin conductance or pupil dilation or (in fMRI) cingulate activity. Eyetracking could be used to measure whether subjects actually read the instructions (or at least, looked at them). These kinds of measures are easiest to collect in lab experimentation, but even in field settings one might be able to measure quite a lot. Lo, Repin and Steenbarger [2005] collected daily emotion surveys of day traders. Lo and Repin [2002] recording psychophysiological responses of foreign exchange traders. Coval and Shumway [2001] recorded the noise level in the Chicago Board of Trade pit and found that it correlated with price volatility and other trading measures. Surveyors who collect important large scale surveys like the Panel Study of Income Dynamics could, in principle, use computerized surveys with Mouselab eyetracking, and recordings of response time as correlates of confusion.

Another argument sometimes raised about measuring quantities other than choices is that the conclusions we will reach from these studies could have been reached by other studies which only observed choice. This might be true, but using only choices will typically be inefficient since we have other tools.

For example, in the 1980’s experiments with bargaining choices showed that in alternating-offer games, opening offers typically lie somewhere between an equal split and the subgame perfect prediction (assuming mutual knowledge of self interest). One view, consistent with choices in the clever experiments by Neelin, Sonnenschein and Spiegel [1988] varying the number of bargaining rounds, was that people do not look ahead and use backward induction. Another view is that people are looking ahead and making subgame perfect equilibrium choices, but care about other players’ payoffs (or believe others have such social preferences).

It would certainly be possible to distinguish between these two views with more experiments observing only choices.¹³ But since the key distinction between these two theories is what people are looking at, measuring what people are looking at is the most efficient way to make progress.

Similarly, somebody could have conjectured that damage to the orbitofrontal cortex (OFC) would change preferences over ambiguous money gambles (à la Knight and Ellsberg) and then done an experiment to test that conjecture by comparing choices of people with OFC damage with

¹³ Schotter (this volume) describes clever experiments with Partow on equilibrium refinements. Their experiment compares behavior in two conditions: In one the players know each other’s payoffs; in the other they do not. The difference in behavior between the two conditions tell us whether attention to the information that is present influences behavior. With modern eyetracking, the same experiment could be done more efficiently with half the sample size, by presenting the payoff information to *all* subjects and measuring how much people attend to it. The subjects’ attention then self-sorts them into low- and high-information treatments (and also provides finer-grained measurements than are available by simply varying the amount of information presented, without measuring attention to that information). Similarly,

choices by control subjects. But in the long history of study of ambiguity, nobody ever made that conjecture. It came only because Hsu, Bhatt, Adolphs, Tranel, and Camerer [2005] could see, using fMRI, that there was activity in the OFC.¹⁴

One more small issue is worth addressing before ending this conclusion. I have heard several people say that neuroeconomics is interesting but is too expensive. This is a dangerous myth. First, whether it is too expensive is an empirical question, and one that should be judged by whomever is paying for the research. Second, it is true that fMRI is expensive at the margin, but other neuroscientific techniques are not. For example, experiments with small samples of lesion patients are cheap at the margin, and good attention-measurement Mouselab software is free (see <http://mouselabweb.org>). Third, while the activity being measured is sometimes physically small (e.g., pupil dilation), measures can be so accurate that very strong inference emerges from small sample sizes, which keeps costs down. (That is, don't mistake our inability to measure with the naked eye with the ability of a specialized instrument to measure something that we cannot see very accurately.) Fourth, virtually none of the funding sources for this research (mostly private foundations and NIH, in the US) are shifting grants away from other kinds of economics research.¹⁵ So even if neuroeconomics funding were shut down, it would not produce an increase in funding for other economics research. Fifth, if neuroeconomics is judged expensive, then all types of economic research should be judged by the same standard. Researchers should be forced to substitute into lower cost alternatives when feasible (e.g., experimental economists should have to do most of their experiments in poor, literate countries). Judgment of expense also needs to include a calculation of expected benefit. Research that is cheap but which does not produce measurable expected benefit would become endangered.

Where do we go from here? A debate about the merits of “mindless” and “mindful” economics cannot possibly be won or lost in the short run. Behavioral economics drawing heavily on psychology has already “won” because it has proved to be useful and popular. And the case for mindful neuroeconomics cannot lose in the short run because it (admittedly) is mostly based on promise. It cannot lose until enough time has passed to declare its promise unfulfilled.

Perhaps the debate is moot because we don't have to choose between the approaches: Economists can do both, and should. The proliferation of dual process models, informed by psychological and neural evidence to various degrees (e.g., Bernheim-Rangel, Fudenberg-Levine, Benhabib-Bisin, Brocas-Castillo, Loewenstein-O'Donoghue), shows the potential of using familiar pieces of economic modeling to explain psychologically and neurally grounded facts and develop new predictions.

Indeed, the difference between the latter models and the mindless style is mostly a matter of how much psychological detail is used to motivate the modeling. GP concede that psychology can be ‘inspirational’ but it is not essential to invent good mathematical models. In contrast, the dual process papers mentioned in the last paragraph are full of thoughtful distillation of large psychological and biological literatures. These facts constrain modeling choices by forcing the

¹⁴ Of course, economists are not keenly interested in the OFC per se. As with many neuroscience studies, the reason that identifying specific regions is important is to understand individual differences, differential development in the lifecycle (including childhood, adolescence, and aging), constrain evolutionary theorizing, to guide choice of economic institutions, and because different regions can be stimulated and disrupted with drugs, deep-brain stimulation, and other methods in different ways.

¹⁵ For example, most of my own research in fMRI and eyetracking so far has been supported by universities and the private Moore Foundation. The Moore Foundation grant is explicitly aimed at high-risk research which NSF will not support.

model to explain a lot of related phenomenon, rather than one small piece of a literature. In contrast, the philosophy GP espouse suggests that knowing a lot about actual human behavior, as established by psychology and neuroscience, is a waste of time in improving economic models of decision making. It is ironic that mindless economists prefer less knowledge to more, since preferring more to less is such a fundamental premise in economics. And sciences which have found new tools have always become more productive by using them.

Appendix: The style and rhetoric of “The case for mindless economics”

It is necessary to comment about the rhetorical style of the GP paper. The style of their paper is sweeping and therefore technically incorrect. This material is deliberately placed here since it would interrupt the narrative flow of the text, and it isn't really important for explaining what neuroeconomics is trying to do; it is important only for readers new to the debate to judge whether they have characterized neuroeconomics sensibly, in the course of articulating their arguments for mindless economics.

Readers who are familiar with the background debates in behavioral economics and neuroeconomics that are summarized in GP's paper will recognize that their summaries are either overgeneralized or wrong in almost every sentence, if their sentences are read literally as a claim about what other social scientists think or do. As a result, any reader who is learning about what behavioral economists or neuroeconomists have discovered, learn, think, theorize, or plan to do from the GP paper ends up at least somewhat misled.

One problem is that the field is moving rapidly. Perspectives expressed a couple of years ago may be replaced by more thoughtful ones. Furthermore, there are a lot of data emerging rapidly so it is difficult to keep on top of the field and describe it accurately. For example, as noted in the text, they make two concrete claims which were already wrong at the time their paper was first circulated (namely, that neuroeconomists have never linked brain activity to choice parameters—many have¹⁶—and that learning models in psychology and economics are different). Indeed, readers would be surprised at how rapidly the choice-parameter approach is spreading throughout cognitive neuroscience; by the time you read this there may be dozens of such examples.

However, the central semantic problem is that GP typically use broad generalizations without qualifiers. Neuroeconomics and behavioral economics are deliberately grouped together. Broad grouping is a confusing mistake and is misleading because there is much disagreement among those researchers about basic facts and the value of different methods. (Many behavioral economists—perhaps most—are skeptical that we need to know much about brain detail.)

Several examples illustrate how misleading the inclusive rhetorical style is. At various points GP state that neuroeconomists or neuroeconomics

- “proposes radical changes in economics” (p. xx). This is wrong. CLP [2004] clearly distinguish between an incremental and radical changes; an incremental approach “add[s] precision to functions and parameters in standard economic models” and is not at all radical.
- “...import the questions and abstractions of psychology and reinterpret economic models as if their purpose were to address those questions” (p xx). This is wrong. We can search for neural firing rates correlated with utility numbers inferred from choices and still understand that utility theory was not developed with that purpose in mind.
- “...insist on a new notion of welfare based on these answers [to age old questions]” (p xx). This is wrong: We don't insist; we suggest exploring the possibility.
- “...plan to enlist the support of the state—a stand-in for a benign therapist—who may, on occasion, conceal facts and make decisions on behalf of an individual's future selves”. This is wrong. If behavioral economics can document systematic mistakes it may have something to say about paternalism (or may not), and at some point should meet the

¹⁶ At a summer 2006 meeting, John O'Doherty suggested that the model of correlating behaviorally derived parameters, often trial-by-trial, with brain activity might rapidly become the dominant statistical style in neuroeconomics.

challenge of doing so. This doesn't imply "enlist[ing] the support of the state" or "conceal[ing] facts".

- "...is therapeutic in its ambitions: it tries to improve an individual's objectives" (p xx). This is wrong.
- "The central questions of neuroeconomists are: How do individuals make their choices? How effective are they making the choices that increase their own wellbeing?" (p xx). This is wrong; the second question is not central for most neuroeconomists.
- "...argue that the time is ripe for the methodology of economics to be brought in line with the methods and ideas of psychology and neuroscience" (p xx). This is wrong. The idea is simply to see whether psychology and neuroscience can help economics on its own terms.
- "...critique begins with the implicit or explicit assumption that economics, psychology, and possibly other social sciences all address the same set of questions and differ only with respect to the answers they provide" (p xx). This is wrong too. The explicit assumption is that in some cases any one discipline can learn something about how to answer the question its discipline poses from facts and ideas in other disciplines.

Why is there so much deliberate overstatement in the language of their paper? Perhaps it is just a colorful style¹⁷ or is designed to sharpen the point or provoke a debate... which it certainly has. This thoughtful volume shows that the debate is a useful one because it forces mindful economists to articulate more carefully what they are trying to do, how new methods might achieve their goals, and draws out a variety of opinions and facts.

¹⁷ The conservative author Ann Coulter wrote: "Liberals hate America, they hate 'flag-wavers,' they hate abortion opponents, they hate all religions except Islam (post 9/11). Even Islamic terrorists don't hate America like liberals do. They don't have that much energy. If they had that much energy, they'd have indoor plumbing by now." Interviewed on the TV show *Hardball* by Mike Barnicle, Coulter was asked whether she really believed what she had written. Coulter replied, "I think I write in a colorful style."

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