Neuroeconomics: Infeasible and Underdetermined

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Abstract: Advocates of neuroeconomics claim to offer the prospect of creating a "unified behavioral theory" by drawing upon the techniques of neuroscience and psychology and combining them with economic theory. Ostensibly, through the "direct measurement" of our thoughts, economics and social science will be "revolutionized." Such claims have been subject to critique from mainstream and non-mainstream economists alike. Many of these criticisms relate to measurability, relevance, and coherence. In this article, we seek to contribute to this critical examination by investigating the potential of underdetermination, such as the statement that testing involves the conjunction of auxiliary assumptions, and that consequently it may not be possible to isolate the effect of any given hypothesis. We argue that neuroeconomics is especially sensitive to issues of underdetermination. Institutional economists should be cautious of neuroeconomists' zeal as they appear to over-interpret experimental findings and, therefore, it may provide a false prospectus seeking to reinforce the nostrums of *homo economicus*.

Keywords: brain, neuroeconomics, underdetermination, unified behavioral theory

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For its advocates, neuroeconomics has the potential for profound scientific insight. It is embedded within the recent so-called "behavioral turn" in economics, associated with the popularized "nudge" approach. Neuroeconomics endeavors to interrogate human behavior through the attempted development of a synthesis of neuroscience, psychology, and standard economics. Conceptual trading is viewed as generating a host of scientific advances, with the neuroscientist Paul Glimcher (2003) arguing that neuroscience will benefit considerably from the importation of the tenets of mainstream economics. Neuroscience undoubtedly offers extensive clinical benefits, but, as the philosopher Patricia Churchland (2008, 2011) argues, it presents a series of conundrums for philosophy and perhaps the social sciences. Churchland considers that the pattern of the history of science is that "speculative" philosophy gradually cedes intellectual ground to empirically based sciences. In the case of neuroscience, for instance, it offers a direct challenge to the Cartesian duality of mind and body, as the mind may be seen as embedded in brain processes. As Harold Wolozin (2004, 2005) argues, such a view risks the conflation of mind and brain: "[M]ind is not another word for brain" (Wolozin 2004, 565). This is something with which institutional economists would concur. After all, Thorstein Veblen ([1914] 2000) pioneered the use of psychological insights in economics, and developed a stratified conception of the mind: instincts, habit, and conscious deliberation that retain considerable salience.

Given its Veblenian heritage, institutional economists should seriously reflect on the pedigree of neuroeconomics, its engagement with neuroscience and psychology, and the basis of the intellectual claims it makes. In short, neuroeconomics is subject to flaws anticipated by both Thorstein Veblen and John Dewey in that it makes unwarranted knowledge claims. Investigating and demonstrating this is our primary motivation in this article.

The emergence of neuroeconomics has been swift. The field has exhibited an impressive growth in its literature over a relatively short period of time (see, for example, Glimcher 2008) and has recently received its dedicated *Journal of Economic Literature* (JEL) code (D87) in recognition of its growing importance. This has been accompanied by the establishment of a professional association, the Society for Neuroeconomics, founded in 2005 with the purpose of fostering research on the foundations of economic behavior through the facilitation of scholarly collaboration between economists, neural scientists and psychologists, and the "continued advancement of the field." Dedicated neuroeconomics facilities have been established at more than a dozen — mainly U.S. — academic institutions, such as the California Institute of Technology, the Claremont Graduate University in California, Columbia University, and Duke University, among others. This trend is not confined to economics. Similar, albeit less developed movements and literatures have emerged in marketing (for example, Lee, Broderick and Chamberlin 2007), sociology (Bone 2005), the so-called "neuropolitics" (for an exploration of "neuropolitics," see Brack 2011), and law (for an examination of the "benefits" of neuroeconomics to law, see Smith, Chorvat, and McCabe 2004).

Neuroeconomists argue that the field provides "ground-breaking" empirical insights into human behavior, offering the prospect of discovering the proximate causes of behavior by allegedly measuring thoughts, feelings, and hence utility, thus enabling the development of a unified theory of human behavior (Camerer 2007, 2013; Camerer, Loewenstein and Prelec 2004, 2005; Glimcher 2003, 2011; Glimcher and Fehr 2013; Rustichini 2005; Zak 2011). Robert Schiller (2011) writes:

> Economics is at the start of a revolution that is traceable to an unexpected source: medical schools and their research facilities ... These findings will inevitably change the way we think about how economies function. In short, we are at the dawn of "neuroeconomics." (Schiller, 2011, online)

These striking claims are ultimately founded on the empirical application of a variety of neuro-imaging and neuro-pharmacological experimental techniques. Among these are: including electroncencephalogram (EEG); positron emission topography (PET); (repeated) trans-cranial magnetic stimulation ((r)TMS); functional magnetic resonance imaging (fMRI); single neuron electrophysiology experiments that attempt to establish the specialized neurons in decision-making processes, and the measurement of specific neuro-hormones, such as oxytocin, in blood sampling — neuro-pharmacology.

Despite Schiller's praise for neuroeconomics, its application of neuroscientific techniques in economics has drawn criticisms from both mainstream (i.e., Bernstein 2009; Harrison 2008a, 2008b; Harrison and Ross 2010; Rubinstein 2008), and non-mainstream economists and philosophers (i.e., Davis 2010; Fumagalli 2010, 2014, 2015; Herrmann-Pillath 2009; Martins 2011). The mainstream economist Glenn Harrison (2008a, 41), for instance, contests that neuroeconomics is beset by "marketing hype," and that confounding evidential problems and lack of empirical transparency will transpire to make it "even harder for anyone to know what poses for scientific knowledge ... and what is just story-telling."

Roberto Fumagalli (2015) questions the feasibility of attempting to integrate modelling techniques and tools from diverse disciplines. Carsten Herrmann-Pillath (2009) further argues that neuroeconomics is irrelevant for economic theory as it neglects the importance of institutions and evolution. Similarly, John Davis (2010, 2011) analyses the issue of individual identity, arguing that there is a potential for neuroeconomics to inform the conceptualization of the individual. However, the prospectus may not be encouraging as neuroeconomics may be confined to providing corroboration for existing standard economic beliefs. Nevertheless, there is also recognition of the potential of neuroeconomics to inform the social sciences (Bernstein 2009; Mäki 2010), and that any new scientific endeavor is likely to be subject to initial methodological and procedural difficulties (Mäki 2010).

In this spirit, we aim to analyze the claims made by the nascent field of neuroeconomics. Unlike existing economic critiques, we explicitly invoke consideration of the Duhem-Quine thesis in our analysis. We argue that neuroeconomic knowledge claims are susceptible to issues of underdetermination, and hence it over-interprets data and presents what Dewey (1981) may have considered as unjustifiable claims primarily about the ontology of the brain and human behavior. Our fear is that Davis's suspicion about the instrumental use of neuroeconomic results to "confirm" the tenets of *homo economicus* (and variants) has considerable saliency and, as a consequence, the insights afforded by Veblen will remain beyond most economists' appreciation.

In the section that follows, we briefly set out the nature of underdetermination. In the third section, we outline neuroeconomics and identify the tensions within the literature, and then we analyze its underpinning assumptions, highlighting the potential of at least the weak form of underdetermination. In the final section, we offer some tentative conclusions.

On Underdetermination

Underdetermination is frequently associated with the Duhem-Quine thesis, which is derived from the independent work of the French physicist Pierre Duhem in the early part of the twentieth century, and the American philosopher Willard Quine in the middle of that century. Concisely, both Duhem (through experimentation in physics) and Quine (from a pragmatist philosophical perspective) argued that hypothesis testing is subject to a profound weakness: It is not possible to test an isolated hypothesis. A specific hypothesis is conjoined to other hypotheses, auxiliary assumptions, and *ceteris paribus* clauses, for instance, and any one combination(s) of these may be responsible for empirical findings (Quine 1969, 79). This is holistic underdetermination. In effect, empirical models lack sufficient data for the unequivocal realization of an interpretation: Data can be subject to a variety of interpretations, and are hence underdetermined. This is the weak form of the Duhem-Quine thesis, or "inductive underdetermination" (Douven 2000; Okasha 2002).

The stronger version, associated with Quine, suggests that any hypothesis or description can be rendered unfalsifiable through changes in conjoined auxiliary assumptions. Another way to express this idea is that there may be theories that no evidence can possibly arbitrate between (Okasha 2002). Thus, an unfavorable result can be "explained" by reference to accompanying auxiliary assumptions. The obvious implication is that judging between competing theories is frustrated. This further admits the possibility of an infinite regress of the form of testing the auxiliary assumptions of the auxiliary assumptions of the auxiliary assumptions, and so on. However, this may be tempered by Quine's pragmatist philosophical orientation that emphasizes both ongoing reflection and the principle of conservatism. This is reflected perhaps in Charles Peirce's (2006) cable metaphor — the breaking of one filament does not imply the end of the cable. Evidence contrary to some aspect of our belief system suggests that we revise — or at least reflect upon — our beliefs, but in a fashion that is proportionate.

Underdetermination may, in principle, also manifest itself as contrastive in form. This is defined in terms of conceptually incompatible rival theories possessing equivalent evidence to substantiate them, and should be seen as distinct from holist underdetermination (for example, Stanford 2016). Neither Duhem, nor Quine distinguished these types of underdetermination (Stanford 2016). In principle, however, the two distinct forms are possible.

As Kyle Stanford (2016) reports, Duhem felt that underdetermination was a problem specific to physics, but Quine (1969) argued persuasively that it applies to *all* types of scientific theories, and indeed to *all* knowledge claims. Despite this, there are relatively few recent examinations of underdetermination in economics (there are exceptions like, for example, McMaster and Watkins 2006; Rogeberg and Melberg 2011; Sawyer et al. 1997; Søberg 2005). Indeed, some are pessimistic about the ability of (mainstream) economics to deal with underdetermination. Hausman (1992), for example, argues:

> If the Quine-Duhem problem is posed as a purely logical difficulty then it may not in practice be very serious. But … if one is unable to place much confidence in the other premises needed to derive a prediction … from a hypothesis … then there is a serious practical problem. Indeed it becomes almost impossible to learn from experience. *This is the situation in economics*. (Hausman (1992, 307, emphasis added)

In the context of neuroeconomic claims, we agree with Hausman's assessment. We substantiate our argument in the sections that follow, but presently the important aspect of Hausman's argument, from our perspective, is the recognition of underdetermination and the associated caution it implies in making (scientific) knowledge claims. As Robert McMaster and Craig Watkins (2006) as well as Ole Rogeberg and Hans Melberg (2011) argue, there is evidence that some literature retains causal claims for theoretical expediency, lacking empirical basis. Nevertheless, the underdetermination thesis is not without controversy (McMaster and Watkins, 2006). There are two major areas of criticism that are of interest to the arguments we advance here: the unlikelihood of empirical equivalence and the presumption of a theory/data dual. Underdetermination implies that theory choice is mired in potentially intractable interpretive problems, inferring empirical equivalence. However, potentially contrastive underdetermination does not sufficiently acknowledge that science occurs in a dynamic context, so that in practice empirical equivalence is unlikely since new and potentially confounding evidence may emerge (Haack 1995; Okasha 2002; Stanford 2016).

In order to substantiate the claim of empirical equivalence, the underdetermination argument presupposes a theory-data dual. This, we believe, is of more importance in the case of neuroeconomics. Like other areas of standard economics, neuoeconomoics is predicated on a series of dualisms, such as endogenous-exogenous and theory-data (Dow 1990), and game theoretic binaries, such as true-false and trust-distrust. Neuroeconomics tacitly assumes that imaging and other experimental data may be taken as givens and, as with the standard approach in economics, data may accordingly be assumed as neutral and external. Yet, there is a compelling case contending that description, data, and observations are theory-laden. The work of significant philosophical figures, such as Dewey and Peirce, questions the theory-data dual. Dewey (1981) argued, data are not "given," but "taken," i.e., there is a conceptual prerequisite to the generation and apprehension of "facts" and "data." Thus, for example, describing the components of water (H_2O) necessarily invokes chemical theory. What may be considered as data is sensitive to the parameters and intent of scientific investigation (Churchland 2002; Dewey 1981; Haack 1995). Samir Okasha (2002, 315) argues that, "[w]ithout a principled distinction between theory and empirical data, we cannot sensibly ask whether the latter underdetermine the former or not." Without a clear and obvious distinction between theory and observation, the notion of empirical equivalence is further questioned and (contrastive) underdetermination itself is weakened. Yet, we contend, by adopting a dualistic approach to theory and data in assuming that data are given, neuroeconomics reinforces the potential of underdetermination by virtue of its own terms. Accordingly, we feel it is legitimate to consider the epistemic claims of neuroeconomics in this light — in its own terms.

What Is Neuroeconomics?

According to the originator of the term, Paul Zak, neuroeconomics seeks to combine the methods of neuroscience, endocrinology (more specifically, hormones and neurology), psychology, and economics as a means of comprehending and explaining social decision-making. It is the "consilience of brain and decision" (Glimcher and Rustichini 2004). For another prominent advocate, Paul Glimcher (2008, 2), "[t]he goal of Neuroeconomics is to combine … three approaches (economics, neuroscience and psychology) into a single discipline that employs constraints and insights from each level of analysis [to understand how we make decisions]." A third substantial contributor, Colin Camerer (2013, 1556), described the *raison d'être* of neuroeconomics as follows: "The main goal of neuroeconomics is to supply a mechanistic account of how economic choices are made."

Neuroeconomics is founded on Daniel Kahneman and Amos Tversky's (1979) article on the seeming irrationalities of decision-making under risk and subjective valuation (Davis 2011; Glimcher 2008). Their study prompted further investigation of the psychology of rational choice and the apparent persistence of irrational behavior, as well as evidence appearing contrary to the tenets of standard economic behavioral theory. This (new) behavioral literature, however, retains this tenet, but seeks to develop variations of rational choice (Hands 2010; Sent 2004) in conjunction with analyses of the impact of "irrational" emotions and information asymmetries. Indeed, in their history of neuroeconomics, Paul Glimcher and Ernst Fehr (2013) identify the challenges and opportunities presented by Kahneman and Tversky as one of the two developments that combined to form neuroeconomics (the other being technical progress in measurement in neuroscience).

Colin Camerer (2004) further describes neuroeconomics as offering significant scientific opportunity:

> The use of data on brain processes to suggest new underpinnings for economic theories, which explain how much people save, why there are strikes, why the stock market fluctuates, the nature of consumer confidence and its effect on the economy, and so forth. This means that we will eventually be able to replace the simple mathematical ideas that have been used in economics with more neurally-detailed descriptions. (Camerer 2004, online)

The "neurally-detailed descriptions" and measurement of brain processes, to which Camerer refers, are applied in conjunction with a variety of game theoretic experiments in attempts to apprehend behavioral phenomena, including preferences, utility and rewards, cooperation, fairness, trust and altruism, and learning and strategy (for example, Camerer 2013; Camerer, Loewenstein and Prelec 2004, 2005; Glimcher 2003, 2011; Glimcher et al. 2005; Kenning and Plassman 2005; Vercoe and Zak 2010; Zak 2011).

Neuroeconomic "games" and experiments typically invoke what are taken to be "intentional" and "random" constituents, with the latter acting as a control. Thus, for instance, in the intentional element of the "ultimatum" game (as, for example, applied by Zak (2011) in his work on cooperative behavior and fairness), subjects are randomly assigned one of two roles: “proposer” or “responder.” The former initiates an offer for a potential transfer of a proportion of a fund allocated to them at the outset of the game, which the responder may accept or reject. If there is an agreement (i.e., the responder accepts the offer), the players receive the sums contained in the offer. But in the event of disagreement, the players receive nothing. The second experiment consists of a random draw from an urn containing eleven balls numbered from 0 to 10. This draw held constant the amount of money received by the responder from the initiator, but removes the intentional signal from the interaction and employs the “subtraction” method (which we discuss in the following section). In both experiments brain patterns and/or blood composition are measured at various junctures.

Neuroeconomic Claims

Based on the findings of such experiments, neuroeconomists make several notable claims. First, that economics will be enriched methodologically in that greater levels of precision and explanatory depths can be provided by, for example, rendering “as if” reasoning redundant (Camerer, Loewenstein and Prelec 2005; Camerer 2007). Second, a richer psychological profile of the individual can be modelled, which goes beyond the stereo-typical *homo economicus*. This also suggests that there is no longer any need to “sidestep” psychological detail (Camerer, Loewenstein and Prelec 2005; Kenning and Plassmann 2005; Rustichini 2005; Zak 2010). For example, third, Camerer (2007, 2013) argues that neuroscience enables the *direct* measurement of thoughts and feelings. In their introduction to the second edition of *Neuroeconomics: Decision Making and the Brain*, Glimcher and Fehr (2013, xxiv) repeatedly discuss how neuroeconomics provides an algorithmic analysis of the “physical mechanism of choice,” again reiterating the epistemic claim that the processes of human cognition are measurable.

The claims continue. Fourth, Aldo Rustichini (2005, 203-204) for instance, states that the more ambitious aim for neuroeconomics is “[t]o complete the research program that the early classics (in particular, Hume and Smith) set out … to provide a unified theory of human behavior.” Indeed, some neuroeconomists further (fifth) claim to have discovered the “moral molecule” in oxytocin (Zak 2008b) — they declared the measurement of thoughts *and* morals appears to be in our grasp. It is on these grounds that neuroeconomists claim to offer comprehension of the “ultimate black box” — the brain (Camerer 2007).

More specifically, Colin Camerer, George Loewenstein, and Drazen Prelec (2005, 11) stress two neuroscientific findings that could have profound implications for *homo economicus*. First, the human brain implements “automatic” processes more quickly than conscious deliberation or calculation, where these processes have emerged to address problems of “evolutionary importance rather than respect logical dicta.” Second, human behavior is strongly influenced by emotions — the “affective” system. These claims are made without any acknowledgment or reference to Veblen’s analysis of the mind and its implications for economics made a century before.

Camerer, Loewenstein, and Prelec’s (2004, 2005) neural architecture draws from cognitive localization theories: Different parts of the brain are primarily responsible for specific functions. For instance, the amygdala is associated with various emotions, the hippocampus with long-term memory, the cingulated cortex with attention and error detection, and the olfactory cortex with smell, etc. Camerer, Loewenstein, and Prelec present the notion of the brain as two distinct and separable competing systems: (i) the “affective” or emotions and (ii) the “cognitive” or rational thought. Zak (2008, 2011) also identifies the emotional-moral processes in behavior, associating these with Adam Smith’s *Theory of Moral Sentiments*, and contrasts this with rational deliberation, redolent of the (self-regarding) behavior identified in

Smith's *Wealth of Nations*. Zak, however, presents a more nuanced case in attenuating the oppositional dimension of self (rational) and other regarding (emotional-moral) behaviors, arguing that the two are not mutually exclusive and the dominance of one may be context-dependent.

Cracks in the Neuroeconomics Facade?

The above discussion, however, masks the evolution of neuroeconomics along potentially divergent paths — from Camerer, Loewenstein, and Prelec's variation of "behavioral economics in the scanner" (Harrison and Ross 2010) that potentially challenges rational choice, to "neurocellular economics" (associated woth Glimcher [2003] and Ross [2005, 2008]) that is highly supportive of rational choice. Zak's (2004, 2008, 2011) neuropharmalogical approach, presenting markets as sites of morality, potentially offers a further dimension that is closer to Camerer, Loewenstein, and Prelec (2004, 2005).

Given this status quo, there may be doubts over the coherence of neuroeconomics. Roberto Fumagalli (2010, 2014, 2015), for instance, refers to a "labyrinth" and "panoply" of differences. In particular, Fumagalli (2015) refers to the differences in techniques across economics, neuroscience, and psychology that hamper attempts to develop a common framework. Similar to mainstream economic critics, Fumagalli further queries the relevance of neuroeconomic findings to economics:

> Neither constructing a neural measure of subjective value nor establishing under what circumstances choices maximize the value of such measure bears directly on the merits of economists' models. For these models make no assumption regarding what internal value function individuals actually use when assigning subjective values to particular choice options. (Fumagalli 2015, 9)

By contrast, Jack Vromen (2008) considers fracture points as exaggerated, with neuroeconomics possessing a fixed reference point — rational choice. Nonetheless, potentially significant differences appear to be evident at two levels: neural architecture and departures from utility maximization (or rational choice). Specifically, "neurocellular economics" conceptualizes the brain as a distributed information-processing network, and employs game theory and constrained optimization to model it (for example, see Ross 2005, 2008). The brain is considered to control behavior through learning about associations between reward predictors and categories of actions, and hence may be modelled by simulations and experiments (Glimcher 2003, 2011). The brain is redolent of the market in terms of its function in that they are both parallel processors of information and valuations (Ross 2005, 2008). Therefore, both possess efficiency properties.

Furthermore, Don Ross (2005, 248) contends that neuroscience instructs us that neurons and neural structures demonstrate the property of servosystematicity, which refers to the ability of self-maintenance. After all, he argues, the starting point of neuroscience is the segmented brain structure. This case is of some importance as it implies that agency resides in the optimizing *neuron*. For both Glimcher and Ross, the brain and the individual are most appropriately analyzed and conceptualized as a cooperative game between utility-maximizing neurons. As Davis (2010, 2011) notes, this implies that, for Glimcher and Ross, the individual has multiple selves, but these are constructed internally — i.e., the individual is a collection of sub-personal agents. This markedly contrasts with an institutional and social explanation, whereby the individual is defined in relational terms to other individuals in groups and institutions (Davis 2010; Herrmann-Pillath 2009; Wolozin 2004, 2005). Indeed, Herrmann-Pillath proposes the notion of the "extended brain," which acknowledges the social embeddedness of the individual and considers individuality as a social phenomenon.

These disparities between the internal construction of the individual, associated with Ross, and the external institutionalist approach are obvious, and have been elaborated to great effect by Davis (2010, 2011), Herrmann-Pillath (2009), and Nuno Martins (2011). For our purposes, the contrasts between neuroeconomists is worthy of further consideration. Ross's (2005, 2008) and Glimcher's (2003, 2008, 2011) analysis leads to the emphatic rejection of the competing systems frame advocated by Camerer, Loewenstein, and Prelec (2004, 2005), and Zak (2004, 2008, 2011). For Glimcher (2003), (mainstream) economics permits the specification of the computational goal — (expected) utility maximisation — of the brain as it furnishes the benchmark for survival and reproductive success in human and non-human species. Therefore, Glimcher offers the prospect of a *literal* application of the rational choice model. To reiterate, *utility maximization occurs at the level of the neuron and hence throughout the brain*. *Contra*, Camerer, Loewenstein, and Prelec, Glimcher and his colleagues contest that there is no evidence to substantiate the case for two "independent" systems — emotional or irrational and rational — within the brain. As far as we can ascertain, in his pioneering book *Decisions, Uncertainty and the Brain*, Glimcher (2003) makes no reference to emotions.

Neuroeconomists, however, do not seek to explore or exploit these differences. For instance, in a review of Glimcher's (2011) *Foundations of Neuroeconomic Analysis*, Camerer (2013, 1178) wrote: "Paul Glimcher's book makes a strong, empirical argument for the potential of neural measures of subjective value to match up to, and potentially inform, the simplest economic concepts and questions." Camerer (2013) considers that Glimcher's approach is "patient and conservative,", whereas his own is "fast" and "rushed," although, "[h]appily, these slow and fast approaches can both be pursued in parallel." (Camerer 2013, 1178). We are to believe then that differences in neuroeconomics are more a matter of style than of substantial ontological divergences. Yet, Camerer, Loewenstein, and Prelec's (2005) dualistic systemic view of the brain's structure adopts a localization position that there are distinct decision-making modules within anatomical regions emerging from differing evolutionary origins (see also, Zak 2010). Contrastingly, both Glimcher's (2003, 2011) and Ross's (2005) positions promotes a monistic ontology, which conceives a unitary neural structure that is also shaped by evolution, but in a frame that promotes a unified behavioral pattern tailored to maximize reproductive success (given environmental conditions). Thus, this seems more than a question of style.

An example of how the abovementioned can lead to divergent interpretations of the same results, hence suggest weak underdetermination, is provided by experiments into the production of dopamine in monkeys in response to a reward, conducted by a team led by the neuroscientist Wolfram Schultz (and cited by both Camerer Loewenstein and Prelec [2005] and Glimcher [2003]; see also Glimcher, Michael Dorris, and Hannah Bayer. [2005]). The experiment involves the measurement of neural activity in monkeys, sitting passively in a quiet environment, to establish the baseline resting state of neuron firing. "Quite" thirsty monkeys in the same environment and condition were then subjected to a "tone" followed by a "squirt" of juice directly into their mouths. No difference was observed in neural activity (specifically, dopamine neurons), which Glimcher, Dorris, and Bayer (2005, 242) describe as prima facie "a curious result," given that the fruit juice is "reinforcing to thirsty monkeys." Without warning, the amount of the juice (or reward) was increased substantially. In response, there was a "dramatic increase" in neural activity of the monkeys. The continuance of this increased rate of reward through subsequent rounds led to a diminishing rate of neural activity until it eventually returned to the initial restive state. Additionally, when the tone sounded and was not accompanied by juice, the firing rate of neurons diminished. The authors then concluded that the neuron "seemed" to encode the difference between the expected and actual rewards incorporating any sustained change into modified expectations, which correspondingly demonstrates the innate ability to value.

Camerer, Loewenstein, and Prelec (2005) offer a differing interpretation of the same experiment. They highlight homeostasis — the process by which the body adjusts to external stimuli, such as sweating in relatively warm conditions — to maintain a "set-point." Homeostasis, however, can also, involve conscious and deliberate actions, such as putting on a coat when feeling cold. Thus, returning to the "set-point" or "equilibrium" engenders some feeling of satisfaction. On this basis, Camerer, Loewenstein, and Prelec (2005, 27) contend that the standard economic approach of utility maximization "starts in the middle of the neuroscience account." They argue that pleasure should not be viewed as the goal of human behavior, but as a homeostatic "cue" or signal. Homeostatic systems are sensitive to changes in stimuli as opposed to their levels. Hence, Camerer, Loewenstein, and Prelec (2005) also interpret the results of the Schultz monkey experiments in terms of expectations, but not in the literal application of Glimcher Dorris, and Bayer (2005). Somewhat ironically, Camerer, Loewenstein, and Prelec (2005, 28, emphasis added) employ the "as if" clause: "When the juice was expected from the tone, but was not delivered, the neurons fired at a very low rate, *as if* expressing disappointment." For these authors (2005), homeostasis is important in explaining sensitivities to change. Thus, they deduce that this type of reasoning and experimentation can provide a robust economic explanation, for example, of the sensitivities of measures of happiness to changes in income, as opposed to levels of income (Camerer, Loewenstein and Prelec 2005).

However, Vromen (2008) asserts that the differences represented by Camerer and Glimcher are more illusory than real. Vromen observes that Glimcher's approach relates to computations at the level of the neuron and not the level of the mind. In other words, the two approaches are ontologically distinct. Vromen argues that Glimcher's argument in no way implies that neural activity is initiated in a deliberative fashion, and that it is effortless and proceeds in a highly mechanical manner (Glimcher and Rustichini 2004). In other words, expected utility maximization is the default mode arising from evolution.

As noted, Camerer, Loewenstein, and Prelec (2005) and Zak (2004) define rationality as a deliberative process, noting that most behavior is non-deliberative, and, on this basis, they deduce that it is non-rational in the standard economics sense. For Vromen (2008), it is this difference in the definition of "rationality" that leads to the apparent divergences. He considers that Glimcher's neural level computations are equivalent to Camerer's affect-driven or automatic (and hence Zak's allusion to the emotional-moral dimension of behavior). All are effortless and do not involve any deliberation. From this, both Glimcher's and Camerer's camps indicate anomalies between "actual behavior" and the standard rational choice model.

Yet, Vromen's (2008) compromise is not entirely satisfactory. Issues of underdetermination and divergent interpretations persist. For instance, the stratified ontological properties Vromen refers to may be less than watertight, and the interpretations of departures from rational choice exaggerated. Milan Zafirovski (2000) usefully distinguishes between "first and second-class axioms" of rational choice theory. First-order approaches emphazise hyper-rationality or perfect rationality, and are "hard" and "thin." Second-order models are characterized by quasi-, pseudo-, or imperfect rationality, and are and "soft" and "thick." The two differ in terms of their teleological definitions — in identifying actors' ends and motives — with first-order models demonstrating utilitarianist agnosticism. Following

Zafirovski's delineation, in contrast to Vromen, Glimcher's neuron level computations are not equivalent to Camerer, Loewenstein, and Prelec's affect-driven behavior, but manifestations of different classes of rationality. Arguably, Camerer, Loewenstein, and Prelec's (2004) and Zak's positions are most appropriately represented as second order, given the roles ascribed to the "affective system" and emotions, whereas Glimcher, Dorris, and Bayer's (2005) defense of utility maximization suggests a first-order orientation. Given this, neuroeconomics may be subject to weak underdetermination concerning the saliency of first- and second-order rational choice. There may also be a case for articulating the first-order position, as in Ross's (2005, 2008) and Glimcher's (2003, 2011) models, in terms of the strong variant of underdetermination which implies that utility maximization is unfalsifiable. We believe that the above is at least partly attributable to the underpinning techniques of neuroscience embraced by neuroeconomics.

"Doing" Neuroeconomics: Neuroscientific Assumptions

Neuroeconomics employs a range of imaging and measurement procedures, most notably fMRI. As noted earlier, other techniques include EEG and PET. fMRI scanning involves tracking blood flow by recording changes in the blood's magnetic properties. Specifically, it attempts to measure the difference between oxygenated and deoxygenated haemoglobin in the blood. Oxygenated haemoglobin has a weak response to an applied magnetic field. These techniques can be augmented by "hyperscanning" that, in the case of fMRI, allows two or more subjects in MRI scanners to interact via the internet. Zak (2004, 1740) hails this as a potentially significant advance that will permit a greater insight into social interaction: "This literally allows researchers to see one person's brain affect another person's brain." This technique is also complemented by blood sampling — neuro-pharmacology — to detect changes in the levels of specific neural-hormones following some action or decision-making process prompted in experimental games, for example.

In our view, the neuro-imaging processes are rightly lauded for their important clinical benefits, but neuroeconomists concede that imaging mechanisms furnish "only a crude snapshot of brain activity" (Camerer, Loewenstein and Prelec 2005, 12). Thus, there is some acknowledgment of interpretation difficulties. Zak (2004) comments on the necessity of convergent evidence to confirm experimental findings, and notes the sensitivity of results to underlying experimental procedures, measurements, and subject groups. This, he argues, is urgent "in moving from correlation to causation" (Zak 2004, 1745). Yet, while there is obviously some recognition of epistemological issues, there is no explicit cognizance of the possibility of even weak underdetermination. Indeed, the literature projects an air of confidence as demonstrated by Rustichini's (2009) claim that neuroeconomic investigation offers new evidence relating to choice behavior, and the neural structure implementing these choices. In short, Rustichini (2009) aligns with Camerer's (2007) claims to realizing the classicists' "project" of the measurement of utility

However, *all* imaging techniques measure brain activity *indirectly*, which is not necessarily problematic, but does indicate a complexity inherent to the interpretation of imaging results (see, for example, Churchland 2011). Imaging data records the perfusion of cerebral tissues, i.e., a measure of physiological parameters, such as blood flow, oxygenation, glucydic concentration, and not a direct representation of the synaptic activity present in a certain cerebral area. Linking synaptic activity to changes in such physiological parameters involves a host of key assumptions, including:

- Changes in cerebral blood flow (CBF) indicates changes in synaptic activity.
- An increase in CBF usually correlates with an excitatory process. The interpretation of fMRI scanning can be based on a proposition that additional brain activity is associated with higher glucose metabolism, leading to higher oxygen demand, which implies increased CBF. Thus, changes in this blood-oxygen-level dependent (BOLD) signal show maps of brain regions where there are statistically significant differences in the signal between distinct tasks or activities.
- The execution of a certain task requires an increase in neuronal activity.
- The greater the increment in the CBF in a specific cerebral area, the greater the contribution of that area to the accomplishment of a designated task under scrutiny.

Also, imaging techniques frequently apply the "subtraction method" to identify the location of neural activity. Subtraction is based on the measurement of brain activity during a task in the experimental context, followed by the elimination of the focus activity during a control task that is considered to provide a baseline of neural activity (Coltheart 2006; Henson 2005; Klein 2010; Park and Zak 2007; Uttal 2001). Importantly, experimental games in neuroeconomics, such as the ultimatum game outlined earlier, apply subtraction in order to delineate between intentionality and randomness. The baseline is the provision of monetary rewards to subjects predicated, for example, on random draws (or lotteries) as opposed to a conscious or intentional deliberation/decision-making choice. The subtraction thus attempts to remove that element of neural activity derived from receiving (or anticipating the receipt of) the monetary reward, and then allows access to measure the separable effect induced by the process of choice.

Concisely, subtraction assumes the following: a linear, unidirectional systemic model of the brain; neural activities are separable; brain activity is additive, i.e., the exercise of additional tasks involves additional brain function (Sidtis, Strother and Rottenberg 2003); the difference between subtracted recordings is the only source of significance; the components of cognitive function are "true"; and the subtracted activity is irrelevant. Yet, the consensus in the literature (beyond localization studies) indicates that the brain (and nervous system) is a highly complex, non-linear system with extensive feedback loops (for example, Churchland 2011; Coltheart 2006; Damasio 2006; Hardcastle and Stewart 2002; Savoy 2001; Uttal 2001). On this basis, William Uttal (2001) further asserts that there is no consensus as to a "true" theory of cognitive components. Moreover, Uttal strongly contends that there is no accord on the existence and nature of psychological components. In other words, there is a posited link between a particular neural activation, *N*, and a mental experience, M, (see, for example, Antonietti 2010). This correspondence involves three possible typifications (Antonietti, 2010):

1. Each time *N* occurs, M also occurs. Or each time *N* is altered or damaged, M does not occur, or occurs anomalously. Yet, M may occur without *N*.
2. Each time M occurs, *N* also occurs. Yet, *N* may occur in the absence of M.
3. There is a reciprocal implication: *N* always and only occurs with M, and vice versa.

Rustichini's (2009) argument, noted earlier, that neuroeconomics offers two sets of evidence invokes typification 3 — the strongest correspondence. Yet, as the above discussion suggests, there are ample reasons to query the veracity of typification 3, especially given the less demanding correspondences inferred by typifications 1 and 2. Further, a neural structure may demonstrate an association with a particular mental activity, but the nature of this remains unclear: *N* may be an initiator, coordinator, catalyzer, controller, specifier, and so forth (Antonietti 2010; Grabowski and Damasio 1996; Martins 2011).

The noted neuroscientist and psychologist, Antonio Damasio (2006, 93, emphasis original) observes: "It is appropriate to say that signals in the stream (of neural connections) move both forward and backward. Instead of a forward-moving stream, one finds loops of feedforward *and* feedback projections, which can create a perpetual recurrence." This may (to some degree) be demonstrated diagrammatically. Figure 1 provides a simplified and schematic representation of some of the major pathways across the various regions of the brain. The bidirectional property of many of these pathways is significant. The figure does *not* show the nature of any pathway connection — i.e., whether it is an initiation of some synaptic process, the coordination of a mental/synaptic process or processes, a catalyzer, a controller, or a specifier of such processes.

Figure 1 about here

Colin Klein (2010, 268) considers that neurological images "do not provide even weak support for functional hypotheses." Indeed, this is reflected in diverging experimental results: Marginal differences in experimental design, and in the functional model adopted, produce widely differing results (Poppel 1997), and results are sensitive to statistical thresholds (Klein 2010). Indeed, in his comparison of the results of five PET studies on phonological processing, Ernst Poppel (1997) found that there were no localized areas demonstrating significant CBF increases across the studies. Andrew Brook and Pete Mandik (2007, 13, emphasis added) further argued that "[h]owever much MRI may assume and depend on the idea that cognitive function is localized in the brain, the idea faces grave difficulties. Even a system as simple and biologically basic as oculomotor control … is the *very* reverse of localized."[1]

In terms of background conditions, limitations in imaging techniques are recognized by neuroeconomists, but the underlying implications and assumptions (and hence theory-ladenness of data) remain an under-explored issue. Specifically, the core assumption in imaging studies relates to CBF and synaptic activity. This assumption is contentious as it disregards potential efficiencies arising from cerebral specialization. There is no guarantee that a systematic association between the increment in local neural activity and the degree of neural participation in the process under investigation is present. Moreover, learning effects that improve task performance are also disregarded (Uttal 2001; Sidtis, Strother and Rottenberg 2003). Indeed, John Sidtis, Stephen Strother, and David Rottenberg (2003) note that some imaging studies seem to indicate learning effects which *reduce* the recorded neural activity. Thus, *contrary to* the basis of much of neuroeconomics' approach, recorded reductions in neural activity, as signaled by CBF, may actually represent locations of cerebral specialization.

This finding challenges the framing of neuroeconomic practice, as well as the degree of confidence in underlying beliefs. Concepts like utility, feelings, trust, and happiness lack definitional precision, and frequently display over-arching functions, as Zafirovski's (2000) analysis of rational choice demonstrates. Quantification requires not only fairly well-established definitional parameters, but also certainty in measurement procedures and techniques.

This underlying condition of certainty in measurement is further queried by the current technical constraints inherent in imaging processes. For instance, the temporal resolution of measures and the frequency of scanning present limitations. Data obtained from brain imaging are static representations of synaptic activity that is, by its nature, continuous. Thus, it is necessary to evaluate what the relation is between the temporal resolution of these instruments and the temporal properties of the processes measured. Specifically, imaging techniques allow a very accurate representation of cerebral activity in the space domain (up to an order of hundreds of microns). Conversely, they perform poorly in the time domain: fMRI has the capability to produce up to twenty scans per second in analyzing patterns generated by stimuli or events, but this is tempered by the fact that recordings are non-continuous and that the scanning of entire cerebral areas can require a few seconds. Moreover, BOLD signal differences associated with brain activities are small, noisy, and temporally complex (see, for example, Horwitz and Poeppel 2002; Klein 2010).

A further problem is associated with the requirement to measure isolated activities. In order to reduce cerebral "background noise," imaging techniques use the averages of recordings from different subjects (Henson 2005). Cerebral anatomical differences between individual subjects are eliminated since, in order to compare recordings, it is necessary to use normalization methods leading to a kind of "standard brain" described by a system of three-dimensional coordinates (Martins 2011). It is further assumed that the cerebral activation state of subjects during the execution of tasks or cognitive processes is repeatable and comparable among different subjects. This again eliminates possibly significant individual differences.

Thus, two important considerations arise: First, the generation of a "standard brain" eliminates the principal source of background noise — the particulars of the functional organization and anatomical structures of each individual's brain (Bechtel and Mundale 1999; Uttal 2001). Such differences can be quite pronounced, with extensive variations in brain centers from individual to individual that can produce significant differences in fMRI and PET images.[2] Uttal (2001) animates this argument by emphasizing other avenues of variability: If the anatomical and functional centers of the brain demonstrate considerable differences, then it is not beyond the realm of possibility that human thought is also "extraordinarily" adaptive. As Uttal (2001, 196) states, "the brain mechanisms operating at one instant may not be exactly like those at work at the next." In the absence of the intermediate level between the physiological and behavioral levels, there are no guarantees that an anatomic-functional cerebral structure will react in the same way to the same stimuli on different occasions, and that its involvement in the process will be the same for subjects with different cognitive-emotive characteristics.

Second, following from the first, the process of averaging and standardizing "can produce the illusion of a localized process by emphasizing fortuitous regions of overlap to the exclusion of the more widely distributed active regions in the individual subject" (Uttal 2001, 197). Arguably, this "illusory effect" is further enhanced by possible ambiguities in neural function in any correlation between changes in measures in synaptic activity and decision-making or other mental activities. The evidential base is less clear-cut, and most certainly does not lend justification to the more strident claims made by neuroeconomists. For us, Hausman's (1992) assessment seems especially pertinent.

Some Final Thoughts

Biology, psychology, and neuroscience offer potentially interesting insights into human behavior, hence to social sciences in general and economics in particular. Indeed, institutional economists have long recognized this. That social sciences are reflecting once again on their relationships with biology and psychology is to be welcomed. Yet, neuroeconomics's attempt to coalesce neuroscience, psychology, and *standard* economics is highly ambitious. As such, it represents an endeavor with potentially significant sunk costs in terms of equipment, which may prompt some concerns over the saliency of "investments." There is a temptation to buttress the "benefits" of this research activity by emphasizing the richness of its findings — in effect, interpretive biases. However, while recognizing this as a possibility, we do not seek to over-emphasize it. Instead, we wish to highlight the formidable challenges of applying neuroscientific techniques to socio-economic issues, and the interpretation and underdetermination problems that accompany this enterprise. Given this, we call for caution. Yet, this does not appear to be part of the neuroeconomics' narrative.

For us, the current configuration of neuroeconomics and the epistemic claims articulated by its advocates provide sufficient grounds to query the foundations of this purported knowledge, and hence their epistemological stance and objectivity. As Roberto Fumagalli (2015) argues, there is little to suggest that neuroeconomics has advanced the evidence based of the mainstream in economics, despite its claims. For us, there has been, to date, a lack of consideration of issues of underdetermination. Neuroeconomics, much like standard economics, is based on duals: in this case, on the theory-data dual. We consider that such a dual is reductionist in that it presumes that data are given. This is patently not the case in neuroeconomics. Moreover, it signals insouciance over the underpinning assumptions of many neuroeconomic techniques. The neuroscientific assumptions we set out above provide sufficient grounds for querying the more strident claims made for neuroeconomics. There is an under-emphasis of neuroscientific assumptions, despite some recognition of measurement issues. Again, Hausman's concerns about the ability of economics to both recognize and address underdetermination resonates.

The fracture points in the nascent literature may be an indicator of some profound difficulties in that the opposing perspectives bring into sharp relief the prospect of underdetermination in both weak and strong variants. The sensitivity of experimental design, as well as the multitude of auxiliary assumptions underpinning neuroeconomic procedures, has an obvious echo of weak underdetermination, and highlight the profound difficulties in reconciling the distinctive techniques of economics, neuroscience, and psychology. Beyond this, however, is the divergence in interpretations regarding the site of agency and rational choice. This has important ontological bearing. For Ross (2005)and Glimcher (2011), the focus is on the neuron and for Camerer, Loewenstein, and Prelec (2005), as well as Zak (2004), on the "complete" particular brain systems. This may expose difficult ethical issues for neuroeconomics. For instance, does the ontology of competing systems — and indeed constrained neural optimization — imply that we can somehow overcome evolutionary "flaws" in directing behavior down specific pathways for the seeming benefit of society? Indeed, we venture to argue that the configuration of some experiments seems to suggest so. For example, Moana Vercoe and Paul Zak (2010, 139) speak of "manipulating physiology" to assess the behavioral impact of variations in the levels of selected bio-chemicals, such as oxytocin and arginine vasopressin. At best, this is an ethical quagmire.

We are inclined to conclude that many neuroeconomic claims are cavalier and humility seems in short supply. Nonetheless, we are in no way advocating the abandonment of the scientific collusion between social scientists, psychologists, and neuroscientists — that would be throwing the baby out with the bathwater. Churchland's (2011, 4) insightful observation has considerable appeal. Citing Adam Smith, she states that "'science is the great antidote to the poison of enthusiasm and superstition'. By *enthusiasm* here, he [Smith] meant *ideological fervor* … Realistically, one must acknowledge … that science is not on the brink of explaining everything about the brain … [T]here will always be further questions looming on the horizon" (Churchland's 2011, 4, emphasis original).

We fear that the imprudence and over-interpretation of many neuroeconomic knowledge claims has the flavor of "*ideological fervor*" and, as such, could impair the development of neuroscience and the social sciences. Moreover, neuroeconomists' embrace of mainstream economics, with its inadequate institutional analysis, may impose further obstacles to the appreciation of insights from neuroscience and psychology, as well as completely overlook the substantial work of Thorstein Veblen ([1914] 2000) in this area.

Footnotes

[1] Oculomotor control, for instance, ensures that the eyes are pointed in one direction as the head moves.

[2] William Uttal (2001) observes that neurosurgeons recognize that brain variability implies that no "standard" map of the brain can ever be precise enough for their purposes.

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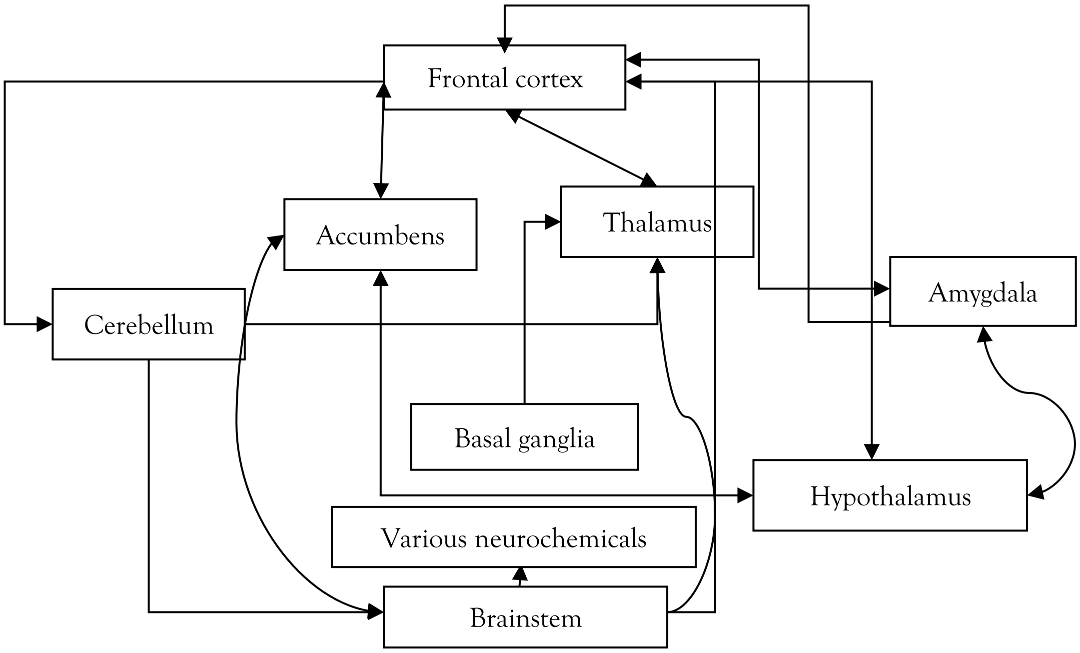
Figure 1. A Schematic of Connectivity in the Brain



Note: Adapted from Churchland (2011, 29).