Economics and Philosophy, **24** (2008) 431–448 Copyright © Cambridge University Press
doi:10.1017/S026626710800206X

PROSPECTING NEUROECONOMICS

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The following is a set of reading notes on, and questions for, the Neuroeconomics enterprise. My reading of neuroscience evidence seems to be at odds with basic conceptions routinely assumed in the Neuroeconomics literature. I also summarize methodological concerns regarding design, implementation, and statistical evaluation of Neuroeconomics experiments.

It occurs to me that maybe we're heavily invested in finding answers to which we don't know the corresponding questions. Maybe the availability of the new technology is running the science rather than the other way round.

(Fodor 1999)

I contribute a series of (numbered and partially ordered) notes and statements. For good measure, I also throw in a question and a modest suggestion. Some statements attempt to encode my reading of what we know, some statements are opinionated and speculative. NE, BE, and EE are abbreviations for neuro-, behavioural, and experimental economics. *Disclaimer:* I'm all in favour of prospecting the brain . . . at the right time . . . with the right questions . . . and the right techniques.[1]

* CERGE-EI is a joint workplace of Charles University and the Academy of Sciences of the Czech Republic, both located in Prague. I thank, without implicating, Michael Anderson, Sasha Bourgeois-Gironde, Glenn Harrison and Angelika Weber for useful comments on an earlier draft. Support by the Boston Consulting Group in Prague is gratefully acknowledged. This manuscript was, for the most part, written while I was teaching at CIFREM, the Interdepartmental Centre for Research in Economics and Management of the University of Trento, Italy. I thank CIFREM for its hospitality.

[1] One of my favourite articles is Johnson *et al.* (2002) where the authors use a non-invasive process tracing technique, MouseLab, to tease apart the relative contributions of social preferences and cognitive limitations on deviations from subgame perfect equilibrium. Another favourite of mine is Lo and Repin (2002) whose authors overcome the confines of the lab by wiring up traders to obtain physiological measures of traders' anxieties when

0. *A definition*
prospect (from: www.m-w.com)
intransitive verb **:** to explore an area especially for mineral deposits
*transitive verb***:** to inspect (a region) for mineral deposits; *broadly*: explore

1. Our synaptic self, how it comes about, and where it ends.
Environmental stimuli (visual, auditory, olefactory, etc.) determine to quite some extent who we become (are): "You are your synapses"[2] (LeDoux 2002: ix, 323, 324). "They are who you are" (LeDoux 2002: 324).
1.1. The qualifier "to some extent" is important: We may be our synapses, and they may be who we are, but that does not mean necessarily that our neurons make us do it. (See Murphy and Brown 2007, and LeDoux 2002: specifically ch. 11.) For the most part, however, I shall not pursue issues of free will here any further as they are tangential for the thrust of my contribution (but see 2; see also Harrison 2008).
1.2. (Important) environmental stimuli are encoded in neural networks that constitute our memory of them. Similar stimuli are encoded by similar networks that can be thought of as patterns of neural networks, or attractors, or generic memories, i.e. memories that encode the essence of similar constructs or experiences such as a hat, or having (been) cheated in a principal-agent situation. A store of generic memories is assembled over time. The neuronal substrates of external stimuli, or outer worlds, make possible the neuronal substrates of internal stimuli, or inner worlds, which are a prerequisite for consciousness and free will.[3] (See Murphy and Brown 2007, and LeDoux 2002: specifically ch. 11, see also Goldberg 2001, 2005.) The inner worlds strike me as a likely contributor to the (unsteady)

trading huge amounts. Yet other examples are the studies reported in Glimcher (2003) and Glimcher *et al.* (2005), about which more below, and Rumpel *et al.* (2005) where the authors study the encoding of memories in the amygdala and the usefulness of the long-term potentiation model of synaptic plasticity. To make it absolutely clear: I find myself in disagreement with those that want to restrict economics to the study of outcomes (e.g., Gul and Pesendorfer 2008). I do have questions about the substance, rhetoric, and promise of the NE enterprise.

[2] Synapses are the near contacts between the axon sender of one neuron and the receiver dendrite of another neuron; the sender releases chemicals, or neurotransmitters that change the electric potential of the receiver dendrite. Essentially, synapses are strengthened or weakened through repeated use or non-use, respectively. Synapses thus are the main channels of information flow and storage in the brain; synaptically connected neurons constitute neural networks. The same environmental stimulus typically involves several neural networks such as those for sights, sounds, and smells (see e.g., LeDoux 2002: especially ch. 11; see also Beauchamp 2005 for a review of recent evidence and open problems in the analysis of multi-sensory integration).

[3] Without memory there can be little anticipation, understanding, or intentional action, among other cognitive acts.

ground level of activations that neuroscientists seem to have started paying attention to only recently (Wang *et al.* 2007). See below note 15.3.

2. The unbearable lightness of being and its potential costs
As John Lennon once put it, "Life is what happens to you while you are busy making other plans." ("Beautiful Boy"): Stimuli that we encounter are – rather automatically – matched with the store of generic memories. How we (manage to) snap out of routine processing is a fascinating problem without, at this point, a clear answer.
2.1. If a match is found then automatic, routine (and parallel) processing of the stimulus (and its various facets: sights, sounds, smells) is triggered that often is "subconscious" and hence beyond our control.
2.2. Up to 95 % of our daily behaviour is prompted by automatic, routine processing (e.g., Baumeister and Sommer 1997). Whatever the exact number, the automaticity of everyday life is a well-established and undisputed fact (e.g. Bargh and Chartrand 1999).
2.3 Because it economizes on our cognitive resources, automatic, routine processing has important benefits. Automatic, routine processing has also potentially important costs: By matching stimuli routinely with a store of generic memories, it might make us susceptible to stereotyping, "cognitive illusions", and misreadings of laboratory one-shot scenarios for what they are (Ortmann and Hertwig 2000; Binmore 2007a, 2007b; Levitt and List 2007).
2.4. If a match is not found, then less automatic, less routine, and quite deliberate (and sequential) processing of the stimulus (and its various facets) might get triggered.
2.5. The process of switching from automatic, routine processing to deliberate processing of stimuli seems not well understood, but it is likely to be triggered by mismatches of stimuli with the store of generic memories, the expectations that store creates, and the self-monitoring that we continuously do (Goldberg 2005: especially 201–35; see also Anderson forthcoming and Glimcher *et al.* 2005).

3. Routinization
A process of routinization condenses our experiences (e.g. for some fascinating evidence, Goldberg 2005: fig. 14 and 15, 204–13 and also ch. 11).[4] Deliberation and the analysis of challenges (cognitive and emotional, on this pair of descriptors more below) seem to involve the right hemisphere's prefrontal cortex significantly; as we experience similar situations again

[4] I'm talking about experiences that are of importance to neuroeconomists. Linguistic intuitions work differently. However, there are rather interesting interactions between comprehension and motor control. For a good entry point to this literature see Anderson (forthcoming).

and again, we see the activations that build up generic memories drift to the left hemisphere's subcortical limbic structures.

4. Intuition is the result of routinization
Intuition is "the condensation of vast prior analytic experience; it is analysis compressed and crystallized. . . . intuitive decision making is postanalytic, rather than preanalytic or nonanalytic. It is the product of analytic processes being condensed to such a degree that its internal structure may elude even the person benefiting from it. . . . The intuitive decision-making of an expert bypasses orderly, logical steps precisely because it is a condensation of extensive use of such orderly steps in the past. It is the luxury of mental economy conferred by vast prior experience" (Goldberg 2005:150, 152; see also ch. 8). That vast prior experience is, of course, the store of generic memories that gets consulted for guidance.

5. Emotions are the result of routinization
What is true for intuition is true for some emotions (Murphy and Brown 2007: 24–7; see also Goldberg 2005: 185–235; and, for that matter, Glimcher *et al.* 2005 and Pessoa 2008). As it is, there is a long-standing debate in psychology on the cognitive foundation of emotions (e.g. Ortony *et al.* 1988) of which neuroeconomists seem curiously unaware. Overall, it seems that little progress has been made since Adam Smith lucidly discussed the nature and causes, and proposed a classification, of emotions (see Meardon and Ortmann 1996a, 1996b).

6. Affective–cognitive divide? Not!
An important implication of 5. and 4. is: The alleged dichotomy between automatic and non-automatic processes prominently on display in Camerer *et al.* (2005: 9), and in much of the current NE literature, seems a mis-specification. Glimcher *et al.* (2005: 251–2) are quite adamant about this issue, as is Pessoa (2008). Emotional experience and emotional expression (whatever that means), for example, involve the neocortex as well (Goldberg 2005: 220–1). In sum, the attempts to construct an affective–cognitive divide (e.g. Camerer *et al.* 2005) seem thoroughly misguided.

7. "Module madness"[5]
More generally, the notion that humans have specialized and tightly localized modules for almost everything – another view prominently on display in Camerer *et al.* (2005) and in much of the current NE literature – is a modern version of phrenology (e.g., Cabeza and Nyberg 2000; Fodor

[5] Goldberg (2001, p. 55).

https://doi.org/10.1017/S026626710800206X Published online by Cambridge University Press

2000;[6] Uttal 2001, no date; Goldberg 2001, 2005; Glimcher *et al.* 2005; Anderson 2007a, 2007b, 2007c, and forthcoming; Pessoa 2008; see also Posner 2003 and Prinz 2006).

7.1. Cabeza and Nyberg (2000) demonstrate convincingly by way of a meta-analysis of activations in 275 PET and fMRI studies that most brain regions[7] are engaged by a variety of cognitive challenges. This is so even though control/comparison tasks have been subtracted out and reported activations are therefore a lower limit of the true activations. In a recent set of articles that draws on the analysis in Cabeza and Nyberg (2000), Anderson (2007a, 2007b, 2007c) has quantified the number of activations for selected task categories. He finds that on average each of the 135 tasks in his sample activivated 5.97 brain regions (SD = 4.80) and that perceptual tasks and language activated 4.88 and 7.81 brain regions, respectively. The resultant brainmaps (e.g. Anderson 2007: 5) are not at all of the modular kind that the interested public is typically sold (e.g. Zak 2004: fig. 2).

7.2. It is noteworthy that regions associated with some sensory or motor activities (such as perception and language) show tighter localization than those associated with higher cognitive processes such as "working memory" or "problem solving" (e.g., Cabeza and Nyberg 2000: Table 5), where the data literally are all over (half of) the map. Narrow localizations, at least for higher cognitive processes, are a myth (Uttal no date: 12, see also 2001; Anderson 2007, 2007a, 2007b).

7.3. Anderson (2007, 2007a, 2007b) proposes an intriguing argument (the "massive redeployment hypothesis") for the empirical observation that cognitive challenges of a evolutionarily more recent nature are correlated with more dispersed activation patterns including activations in evolutionarily older brain areas (e.g. Anderson 2007: fig. 4).

7.4. A major problem, also mentioned by Cabeza and Nyberg (2000: 31, see also 35–6) and Pessoa (2008), is the deplorable fact that the authors of most functional neuro-imaging studies interpret activations within their own domain, ignoring the multiple functions that the same brain region might have, and hence the more general function it might fulfil.

7.5. If something is not black, it is not necessarily white. In other words, questioning extreme conceptions of the modularity of the mind (as they are reflected in almost all neuroeconomics studies and reviews) does not mean that, especially for evolutionarily earlier ("lower") cognitive challenges, one does not find narrowly localized regions of activations (Anderson

[6] Fodor (2000) seems to contradict to some extent his earlier views on the modularity of the brain (Fodor, 1983) although, as Prinz (2006) points out, that maybe partially due to the somewhat misleading choice of the title of the earlier book. See also Anderson (forthcoming).

[7] The regions were 26 numbered Brodman areas, plus the insula and MT, the basal ganglia, the thalamus, and the cerebellum, for each hemisphere, with each area divided into a lateral and medial segment, for a total of 124 brain regions.

2007a, 2007b, 2007c; Pessoa 2008). Surely one does. By the same token, for evolutionarily later ("higher") cognitive challenges, activation patterns tend to be highly interactive and distributed. The interesting issue is how much of the brain is localized in the sense of having only one clearly defined function. And whether indeed necessary activations are also sufficient.

8. The surprising, or at least ironic, (neuro)economics of decisions and games

An answer also seems to be outstanding to the interesting question whether the rational choice explanations of seemingly well established neuroscience results, both for simple decisions and simple games, generalize to more complicated contexts. In fact, this issue seems a major bone of contention between NE and those more critical of the NE enterprise.

8.1. Glimcher *et al.* (2005; see also Schultz 2006; Lee 2008; Doya 2008) make, to my mind, a rather persuasive case that a simple unified framework drawing heavily on standard expected utility theory has considerable power to explain the data for simple decisions and simple games (namely, the inspection game which is essentially a gift or trust game under a different name). The brain, in essence, is in these contexts an economic evaluation machine. The work by authors such as Glimcher or Schultz is particularly fascinating because it seems to suggest that desirabilities, whether strategic or not, have a physiological basis that involves LIP neurons.

8.2. The irony that a unified framework seems to draw heavily on exactly those formulations that certain representatives of BE and NE have worked hard to critique/undermine, has not been lost on some of its more open-minded representatives: "An irony of neuroeconomics is that neuroscientists [such as Glimcher or Schultz] often find the most basic principles of rationality *useful* in explaining human choice, while neuroeconomists like ourselves hope to use neuroscience to help us understand *limits* of rationality in complex decision making" (Bhatt and Camerer 2005: 449). In the footnote following this passage, Bhatt and Camerer suggest that "a way to reconcile these views is to accept that simple rationality principles guide highly-evolved pan-species systems necessary for survival (...) but that complex modern choices are made by a pastiche of previously-evolved systems and are unlikely to have evolved to satisfy rationality axioms only discovered in recent decades" (Bhatt and Camerer 2005: 449). Maybe, maybe not. For starters, it strikes me as problematic to rationalize this way the results that Glimcher and his colleagues have produced on the inspection game, a prominent workhorse of the social preferences crowd. More importantly, a flurry of recent articles and books (e.g. Cherry *et al.* 2002; List 2006; Levitt and List 2007; Binmore 2007a, 2007b) provided significant evidence questioning the reality of social preferences that cannot be explained through basic formulations of decision and game theory and appropriate experimental controls. [8]

9. Neurogenesis and plasticity

The adult brain produces new neurons well into adulthood (for references see Murphy and Brown 2007: 117) and, albeit at a decreasing rate, throughout one's life (Goldberg 2005: especially ch. 14). The adult brain is furthermore rather malleable, or "plastic" (e.g. Rumpel *et al.* 2005; Goldberg 2001, 2005) and to quite some extent idiosyncratic in detail (e.g. Goldberg 2001: especially ch. 7, Goldberg 2005: ch. 14).

9.1. Neurogenesis, or neuronal growth, can keep us sharp until late age, can slow down progressive brain disorders such as dementias, or even reverse the consequences of head trauma, strokes, or plain underusage of the grey and white matter (Goldberg 2005).

9.2. Neurogenesis can be enhanced through cognitive enhancement programmes (Goldberg 2005).

9.3. Being an academic constitutes a naturally occurring cognitive enhancement programme.

9.4. Relatedly, a consensus seems to emerge that the organization of the brain (functional and anatomical) is highly plastic across one's lifetime (e.g. Rumpel *et al.* 2005). How tremendously plastic our brain is, even at an advanced age, has been demonstrated with striking success by Taub's and his collaborators' work with stroke patients (e.g. Liepert *et al.* 2000, Taub *et al.* 2006).

9.5. Neurogenesis and plasticity further question primitive notions of localization/modularity. In general, while people are pretty much the same in their basic make-up, they are idiosyncratically equipped through nature and nurture.

10. Lab environs, oh how artificial thou are

Lab environs for NE experiments qualify as exhibit A for the artificiality of lab environs. The artificiality of lab environs is likely to reduce the signal-to-noise ratio of techniques such as PET and fMRI.

10.1. At the minimum this artificiality, and the neural activations that it produces, adds to the ground noise (recall Wang *et al.* 2007; Fox *et al.* 2006,

[8] Cherry *et al.* (2002) show that asset legitimacy and minimal social distance drive out other-regarding behaviour. List (2006) attempts to calibrate laboratory gift exchanges and to control for experimenter effects. His results are intriguing, and his conceptual attempt is impressive, although questions can be asked about aspects of his experimental field implementations (e.g. Were the experimenters that instructed field participants one-on-one blind to the research hypothesis?) and the details of his reporting. Levitt and List (2007) review List (2006) and provide additional (albeit somewhat opportunistic) evidence on the nature and causes of social preferences in the lab. See also Ortmann and Hertwig (2000) for related arguments.

2007; Fox and Raichle 2007). Part of the noise may have no functional purpose, part of the noise may come from subjects engaging in thoughts – not necessarily very conscious ones – like: Why am I lying here? What's that dizziness? What's that metallic taste in my mouth? Is this harmful to my health? Have they lied to me after all about the health risks? What if I think nasty/naughty thoughts? Can the experimenter decipher them?

10.2. "(Modern cognitive neuroscientists and neuropsychologists) have focused on how humans solve problems, but have largely ignored the issue of how they choose the problem to be solved" (Goldberg and Podell 1999: 376; see also Goldberg 2001: 77–85). To the best of my knowledge, this issue remains terra incognita for neuroeconomists, too. I believe this to be one area where neuroscience could contribute to our understanding of the brain's working.

10.3. More generally, all the motivations that have made experimental economists so eager to leave the lab and venture out in various kinds of field settings (Harrison and List 2004), apply to current neuroeconomics experiments. Of particular note here are the issues – such as the question of the representativeness of the stimuli, or the appropriate form of information presentation) – that have been the bone of contention in the debate between the heuristics and biases school (e.g. Kahneman and Tversky 1996) and those arguing from an ecological rationality point of view (e.g. Gigerenzer 1996; see also Hertwig and Ortmann 2005).

11. Selection biases

The artificiality of environs is likely to produce important selection biases, a problem whose importance is enhanced through the typically exceedingly low number of subjects. Harrison (2008) gives a number of examples. I'm not aware of any NE study that addresses this problem which is of some concern to experimental economists, even under the best of circumstances.

12. The need for better controls and higher numbers of subjects.

The between-subjects designs typically used in NE do not control for cognitive-sociodemographic characteristics that have been shown to be of significant importance in other experimental contexts (e.g. Holt and Laury 2002 and Harrison *et al.* 2005). Again, this problem gains in importance because of the exceedingly low number of subjects per treatment.

12.1. Wei *et al.* (2004), for example, argue that inter-subject variability is typically higher than intra-subject variability. While there is a host of antidotes (spatial normalization procedures, fancier tests) recruiting healthy right-handed, or normal subjects seems not enough of a control by a wide margin. Even controlling for cognitive-sociodemographic characteristics may be too little (e.g. Thirion *et al.* 2007).

12.2. Thirion and his colleagues (2007) propose explicitly that fMRI inter-subject variability ought to be related to "behavioral differences and individual or psychological characteristics of the subjects" (115). They find empirically that 20 subjects or more (preferably 27) should be included in functional neuro-imaging studies to reach sufficient reliability. They identify as a key problem that activation detection on the basis of smaller numbers might overlook subtle (differential) activation patterns and argue that the proposed number of subjects is the minimum for auditory and motor activations. So, numbers ought to be higher for more subtle functional activation patterns. It seems to me that such a study is urgently needed for a couple of NE poster childs.

13. Standards and practices of experimentation in NE.
Standards and practices of experimentation in NE are problematic.
13.1. Deception, for example, seems to be accepted by some in a rather cavalier manner. Rilling *et al.* (2002), Sanfey *et al.* (2003), Coricelli *et al.* (2005), and Knoch *et al.* (2006) are prominent examples but see also the pointed observations in Jaffe (2006) whose default assumption was only the experimenter that greeted him would be in the control-room and was "unsettled" when he realized that in fact there were four people watching him. Unfortunately, there is presently no such thing as "a guarantee of no deception" (Zak 2004: 1738). And, apparently, subjects seem well advised to abandon even elementary default assumptions at the lab door. The empirical consequences of deception by commission or omission are presented in Ortmann and Hertwig (2002) and Hertwig and Ortmann (2008) for the behavioural level; it can only be guessed what ground noise (see note 15.3.) these practices provoke on the neural level.
13.2. Relatedly, it is a problematic (albeit, maybe, unavoidable) procedure in neuroeconomics labs that subjects are instructed individually. Apparently, however, they are typically not instructed by researchers blind to the research hypothesis – a procedure that is likely to make an experimenter's wishful thinking a reality, as Ortmann (2005), based on readily available and persuasive evidence from psychology and taking aim at Henrich (2000), argues.
13.3. Last but not least, a well-established (albeit occasionally ignored) practice of experimental economists of concatenating new experiments through replication of old results seems almost completely amiss in the NE literature. I do understand that the strategy of concatenating new experiments in this manner is very expensive, but there is evidence that the inter-subject and -laboratory variability of results is high so that the strategy of systematically trying to build on earlier studies ought not to be just dismissed.

14. Standards and practices of experimentation in NE, continued: Lack of social distance as the key confound.
Most importantly, issues of social distance and experimenter confounds that recent papers in economics have wrestled with (e.g. List 2006; Levitt

and List 2007) seem to be completely ignored in NE. That's deplorable because these articles provide, in my view, rather damaging results to the BE enterprise and, by extension, also to the NE enterprise.

14.1. The subjects in McClure *et al.* (2004) were asked to make choices between an immediate reward and a distant reward and between a distant reward and an even more distant reward. All choices showed activation in the lateral prefrontal areas and associated parietal areas of the brain. Choices involving immediate rewards were correlated with BOLD responses in the limbic system. When the immediate reward was available, but subjects chose the delayed reward, the authors found greater BOLD responses in the lateral prefrontal and parietal areas of the brain compared to its limbic areas. This pattern strikes me as observationally equivalent to that of subjects being worried about the credibility of the experimenter and/or assessing the transaction costs. See Harrison and Lau (2005) for the detailed arguments and empirical evidence supporting the credibility/transaction costs explanation. Roth (2007) offers the additional observation that, as a matter of fact, all rewards are delayed as long as the subject lies in the scanner and that "immediate versus delayed seems to be at least in part a *verbal* phenomenon" (slide 16).

14.2. McCabe *et al.* (2001), using a binary choice version of the trust game, argue that their seven "cooperators" showed greater prefrontal activations than the five "defectors". The observed activation pattern is also consistent with subjects' concerns about what the experimenter might think about them.

14.3. Sanfey *et al.* (2003) find that their subjects reject roughly 50% of the unfair offers by humans (and only 20% of the unfair offers by computers). Is the observed activation consistent with negative emotions or with concerns about what the experimenter might think about them? (Both.) Could these explanations be distinguished? (No.) Should they be distinguished? (Yes.)

14.4. Xiao and Houser (2005) find that subjects that are allowed to express anger to their counterpart are more likely to accept the offer. Is the observed behaviour consistent with lesser concerns about what the experimenter might think about their action? (I think so.) Could the competing explanations be distinguished? (No.) Should they be distinguished? (Yes.)

14.5. Spitzer *et al.* (2007) identify a brain locus for norm compliance. Using that old workhorse, the dictator game, they study activations in a no-punishment and a punishment condition. Subjects transfer more money in the punishment condition. The authors also find increased activations in some parts of the prefrontal cortex and the caudeate nucleus in that condition. Again, the question is to what extent these activations could have been a reflection of the subjects' concern about the experimenters' opinion of them.

14.6. Even in as seemingly innocent a context as the neuro-imaging study of Coricelli *et al.* (2005), the regret – and blame-taking – demonstrated there might have been directed towards the experimenter.

14.7. And so it goes. In their mini review of related literature on the occasion of the publication of Spitzer *et al.* (2007), Montague and Lohrenz (2007) not once bring up the issue of the social distance confound. Given the evidence, for example, in Cherry *et al.* (2002) and List (2006), that strikes me as remarkable. Surprisingly, even experimenters that should know better (since, in fact, they have laid the ground work for some of these kind of concerns; see e.g. Hoffman *et al.* 1996) rarely ever seem to discuss the social distance confound. It seems clear that, apart from adding inevitably ground noise to the activation patterns, this confound must interfere with all attempts trying to understand the neural correlates of social, risk and time preferences.

15. Data, blackboxed

The processing of data is too often a black box and something to be viewed with suspicion (e.g., Cabeza and Nyberg 2000; Uttal no date, 2001; Dobbs 2005; Thirion *et al.* 2007); it destroys the key premise of replicability and reproducibility upon which the credibility of experimentation techniques is built. The colourful pictures suggest more precision than there is and hence invite overselling and hyperbole.

15.1. Cabeza and Nyberg (2000: 1–3) discuss explicitly methodological concerns such as the inability of functional neuro-imaging techniques to determine the essentiality of various activations prompted by certain tasks; the "sluggishness" of haemodynamic techniques such as PET and fMRI; and the various problems of the subtraction method (including evidence that the insertion assumption is not always valid). Uttal (no date, 2001) covers much of the same ground but in much more detail and more critically, pointing out even if the brain were modular in a way that would allow haemodynamic techniques to succeed, and even if we were assured that the essentiality problem were reasonably addressed, that there remain important issues of the appropriate level of significance to define the boundaries of reported activations. All of this does not even question the quite questionable assumption that data of individuals can be averaged/aggregated in some fashion (see Dobbs 2005, for a good illustration of how heroic that assumption seems to be; see Beauchamp 2005 or Wei *et al.* 2004, for harder evidence).

15.2. More recently, Gerlach (2007) finds, comparing 20 fMRI studies that compared visual processing of natural objects and artefacts in normal subjects, that "not a single area is consistently activated for a given category across all studies" (296). Needless to say that his results – which he attributes to too few observations and a liberal adoption of statistical thresholds that led to false-positive activations, among others – are based on a rather simple task. Exploring social preferences is likely to produce variability of an order of magnitude higher.

15.3. Wang and his colleagues (2007) add yet another complication. These authors find that the brain (in the primary visual areas of normal-sighted subjects) is "a system intrinsically operating on its own, and sensory information interacts with rather than determines the operation of the system" (697). See also Fox *et al.* (2006, 2007) and Fox and Raichle (2007) for related findings. Apparently this ground noise can easily swamp the significant effects that the subtraction method identifies as activation patterns. This emergent discussion is of major importance to the reality of NE findings, be it only for the simple reason that ground level activations are likely to be more pronounced (and unsteady) in typical NE settings. See also 10., 12.–14.

15.4. The colourful graphics that are the outcomes of all that intransparent processing of fMRI raw data imply much more precision than there actually is. One rarely finds caveats formulated (for a laudable but shy attempt, see Zak 2004: 1745–6).

15.5. Which brings us to the marketing side of things: The ways most neuroeconomists go about the reality of their techniques reminds me of the ways certain behavioural economists go about the reality of cognitive illusions.[9] See Hertwig and Ortmann (2005) for a primer on the Cognitive Illusions controversy. As far as I am concerned, NE is the continuation of the politics of BE (in economics) and the heuristics and biases (in psychology) approach with different means. The difference is: NE is more intransparent since the barriers to entry (in human capital acquisition and scanner time) are rather high. These barriers to entry notwithstanding, we are likely to see the same kind of broadening scepticism and debunking of behavioural truths in NE that we currently see in economics (regarding BE: e.g. Cherry *et al.* 2002; List 2006; Levitt and List 2007; Plott and Zeiler 2005, 2007) and even earlier in psychology (regarding heuristics and biases: see Gigerenzer 1996; Hertwig and Ortmann 2005).

16. Availability of data as key condition for replicability and reproducibility
The reported non-availability of data (Harrison 2008) is scandalous by all standards to which experimental economists adhere (at least conceptually) and thoroughly discredits the NE enterprise.

16.1. Again, not making data available destroys the key premise of replicability and reproducibility on which the credibility of experimentation

[9] For a particularly questionable example see Rabin (1998) who, in his review of the evidence from psychology that might allow economists to mine insights that make their models more realistic, chose to ignore the considerable controversy about the reality of cognitive illusions that had found its manifestation in a very public dispute in the top psychology journal (Gigerenzer 1996; Kahneman and Tversky 1996).

techniques is built. I do understand the large amounts of data involved and the logistics problems that they might bring about. But something ought to be done in NE if the enterprise wants to keep a modicum of credibility. For example, NE could make it a precondition for publication that data are submitted to the fMRI Data Center (http://www.fmridc.org/f/fmridc), which is designed to be a publicly accessible repository of peer-reviewed fMRI studies that aspires to provide all data necessary to interpret, analyse, and replicate fMRI studies.

16.2. The current practice, at least that's what economic theory suggests, is an invitation to play fast and loose with data processing as well as its interpretation. I do not easily trust results produced under these circumstances. Neither, apparently do others (albeit, predictably, these others are not behavioural economists).

17. Improved research methods? Not by a wide margin . . .

Because of the concerns formulated in 10 through 16, the same confounds that trouble EE and BE (Hertwig and Ortmann 2001a, 2001b) also trouble NE, only more so. I do not agree with the statement in Zak (2004: 1737) that neuroeconomics is improving research methods. Certainly not on the economics side.

18. So what?

Let us, for the sake of the argument, assume that all the problems discussed above can be solved: Let's specifically assume numbers and selection problems have been solved successfully, appropriate data processing strategies and tests have been used, experimental design and implementation issues have been addressed (or at least controlled to a reasonable degree), and we have therefore reason to believe that the neural hotspots that are superimposed on the brain scan *ex post* identify sufficient activations rather than necessary ones. What then could economists learn and do with their insights? Again, for the sake of the argument, let us assume that (the lack of) self-command on some issue (drinking, smoking, sweets, procrastination, norm violations) could be tied to some identifiable locale in the brain, what would be the consequence? Would it not be a behavioural one (e.g. make the proband play chess to learn to think forward) in ways similar to Taub's and his colleagues' strategies to help the patient (putting the functional hand into a boxer glove to force the stroke victim to train the incapacitated hand)? But, then, do I need NE results to devise such strategies? While I am very sympathetic to the neuroscience enterprise per se, I do not see any persuasive evidence that the kind of NE experiments currently done can contribute the kind of significant insights that experiments in economics promised almost from the first day (well at least the early work of Vernon L. Smith). And I definitely can not see how studying colourful pictures of the brain has implications for economic growth.

19. Modest suggestion
In order to gain some respect (and not just the huge grants it has managed to attract), NE would be well advised to take serious the conceptual and methodological concerns mentioned above. There are too many open questions that have been pushed aside by the easy availability of funding for projects that too often are built upon problematic conceptions of how the mind works and that too often have used pre-confused designs and implementations. See 10. and 14. and also Harrison 2008.

Most importantly, NE needs to find ways to address successfully the question whether it wants to adhere to the key premise of replicability and reproducibility upon which the credibility of experimentation techniques is built. If it does not, it will become a doomed enterprise and will surely invite a sequel to Binmore and Shaked (2007).

Specifically, NE ought to stick to the standard conventions that experimental economists have accepted for good reasons: financial incentives whenever possible and a complete ban on deception, both by commission and omission. Most importantly, NE needs to address the social distance issue. Since NE, with few exceptions, seems to be about the various aspects of social, risk, and time preferences, understanding experimenter – subject interactions seems to be of extraordinary importance.

I believe that in the last few years people were rushing into NE to stake claims and find the kind of gold nuggets that would be able to set the stage (and would make them sit pretty). This rush is understandable, and individually rational, but came at a cost of questionable practices that are hardly trust-building. So far (and it seems that McCabe agrees, as surely does Harrison) that rushing to stake claims has produced few (no?) nuggets of note. It is hard to avoid the impression that the availability of new technology has indeed run the science rather than the other way round.

Of course, given all the money that has been sunk into the NE enterprise, something is going to come of this. A key question is at what cost this something comes: To argue, as Rustichini (2005) does, that "at the very least, neuroeconomics provides new data in addition to those we have available from theoretical, empirical, and experimental research on human behavior" (201) is an obviously flawed argument.
I agree with Harrison (2008), and by way of reference Smith (2008), that NE has to go beyond the application of fancy new tools to old questions

(based on a problematic reading of experimental evidence in psychology) and address new questions. Such questions might require completely new experiments but should take cues from well-established principles of design, implementation, and replicability in EE.

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