

When economics meets neuroscience: Hype *and* hope

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Abstract

This is a paper on interdisciplinarity and rhetoric. Neuroeconomics is hype, but this does not rule out entertaining hopes about its capacity to produce some desirable consequences. Its “disciplinary conventions” are characterized as those of a young interdisciplinary field. Its rhetorical advantages are identified, and its rhetorical excesses are put in perspective and conditionally excused. Its evidential roles are emphasized, and they are shown to be limited in alleviating the under-constraint issue due to the difficulty of using fMRI properly.

Keywords: neuroeconomics, rhetoric, interdisciplinarity, disciplinary conventions

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1. Introduction

The New Oxford Dictionary of English defines *hype* as ‘extravagant or intensive publicity or promotion’; and more strongly as ‘a deception carried out for the sake of publicity’. Arguably, neuroeconomics is hype in both of these senses. I take *hope* to mean an expectation that some desirable condition will or might result from the neuroeconomics endeavour. Hope is not necessarily suppressed by hype, or so I argue.¹

Perceptions of hope and hype often clash. Some proponents believe that finally, economics is becoming a hard science, or at any rate economics will be improved. Some critics blame neuroeconomics for poor scientific performance and irritating marketing. The tensions appear rather striking. Hoping to understand and alleviate them, I make some simple observations about scientific practice, including its rhetorical aspects.

2. Standards and conventions of scientific inquiry

One source of confusion is related to perceptions of what makes a piece of activity science and what makes it good science. Universal standards of scientific rationality could include abstract principles such as “Only make claims that you are entitled to make! And entitlement derives from giving good reasons, including appealing to empirical evidence.” Both parties in the neuroeconomics debates at least occasionally appeal to some such idea of scientific rationality. Many neuroeconomists argue that conventional economists are not entitled to stick to some of their favourite but

¹ The title of this conference contains a question about neuroeconomics: hype or hope? This reminds me of the conference that we organized in Rotterdam in December 1997. The subtitle of the conference asked about economics more generally: fact or fiction? The title of the edited volume that later collected the conference papers answered the question: Fact and Fiction in Economics. So, it was not either-or but rather both-and. In a similar manner, I am in favour of “hype and hope” instead of “hype or hope?”

empirically refuted assumptions about human behaviour, while these economists may argue that neuroeconomists make overhasty and groundless claims based on sloppy research. In such situations of conflicting judgements, it is hard to presume that there really are some universal and effective easy-to-apply principles constituting context-free epistemic rationality - the same for all times and all disciplines and all inquiries. Indeed, given their abstract nature, such principles alone fail to imply any effective advice for dealing with concrete situations.

Any abstract principle, when applied, must be interpreted and given more specific contents. One way of doing this is in terms of what we may call *disciplinary conventions*. Such more specific conventions shape entitlement: *epistemic rationality is implemented by local, contextual, and institutional means*. If there is some sort of global epistemic rationality, it often becomes manifest and effective through local disciplinary variations.

An important characteristic of disciplinary conventions is that they tend to be accepted as *unexamined (within the discipline itself) givens* and that they impose constraints on global epistemic rationality. If rationality requires that scientists are entitled to make only those claims for which they have good reasons, a maximally stringent reading of this would imply that scientists are entitled to claim nothing. This is a consequence of the structure of disciplinary convictions having as their foundations a set of paradigmatic givens (Kuhn), metaphysical hard core principles (Lakatos), or absolute presuppositions (Collingwood) that provide reasons for other claims but for which no reasons are given within disciplinary practice. Yet, science would be impossible without some such doses of dogmatism, so in practice less stringent interpretations are applied.

This observation suggests that scientific disciplines are in some important respects similar to cultures. Ariel Rubinstein appears to disagree. “At the end of the day, Neuroeconomics will probably influence economics because *economics is a culture and not a science*. By ‘culture’ I mean a collection of accepted ideas and conventions that are used in our thinking.” (2008, 493; italics added) So for Rubinstein, being a culture and

being a science exclude one another. By contrast, I take all scientific disciplines to be cultures also in his sense (for economics, see Reder 1999).

The very concept of discipline is ambiguous between organizational structure and normative structure as well as between the noun and the verb. Disciplines are both organizational structures (organized around university departments, educational programmes, journals, disciplinary associations etc) and normative structures governed by norms, conventions, values, appropriate practices, identity-constituting traditions. Disciplines impose discipline on behaviour, or, invoking also the verb, *disciplines discipline disciplinary practices*.

Practices disciplined by disciplines conform to disciplinary values and conventions. Specific disciplinary conventions derive from selections and specifications of a long list of generic values of science. The list includes items such as Truth, Understanding, Testability, Predictive accuracy, Explanatory power, Reliability, Coherence, Simplicity, Generality, Unification, Novelty, Fruitfulness, Tractability, Rigour, Relevance, and more. Each of these virtues can be connected to the idea of Progress, that of growth or improvement in exhibiting those virtues. Scientific practice is value-laden in the simple sense of having to conform to some selection of such epistemic values or virtues.

The next important observation is that no theory or field of inquiry can fulfil or maximize all such desiderata simultaneously. There are unavoidable *tensions and trade-offs between the desiderata of scientific inquiry* (see Matthewson and Weisberg 2009).

Discipline-specific or field-specific conventions are crucial in interpreting and selecting the mix of virtues to be valued and pursued. Each set of conventions resolves the tensions between generic scientific virtues in a particular way. One consequence of this is that what looks like good science from the point of view of one discipline or field or school of inquiry, may look dubious when viewed from the perspective of another. This observation should help understand some differences and conflicts between disciplines such as economics, psychology and neurobiology.

Disciplinary conventions and virtues are *not purely formal*, independent of the contents of the theoretical and empirical convictions held within a discipline or research field. Economists typically hold it to be a virtue of a piece of inquiry to be “theoretical”, to have “microfoundations”, to be “unified”, to avoid being “ad hoc”, to have “predictive content”, to be “tractable”, and to be “rigorous”. These are customarily interpreted in terms that are relative to accepted methods and theories in economics (for the case of “theoretical” see Mäki 1999). This phenomenon is evident when two or more disciplines or fields claim to deliver the same virtue while providing different interpretations of it. Thus both standard economists and many neuroeconomists claim that their own pursuits and accomplishments offer the needed microfoundations for economic theory, interpreting this in terms of their respective frameworks. Conceptions diverge as to where the stopping points lie, that is, where the appropriate microfoundations have been reached.

The *degree of flexibility or revisability* of such conventions varies from field to field or discipline to discipline and their stage of development. Casual observation suggests that there is more flexibility in young and interdisciplinary (open) fields and less flexibility in old and monodisciplinary (closed) fields. Compared to economics, neuroscience is young, interdisciplinary, and flexible in its conventions. Comparing the two compartments of neuroeconomics, the dominant participant discipline affects the degree of flexibility: more flexibility in “behavioural economics in the scanner” and less of it in “neurocellular economics” (borrowing these labels from Ross 2008; see also Vromen 2007). The cultures of participant disciplines shape the attitudes towards neuroeconomics. Non-economists are more relaxed and welcoming of it and may find it hard to understand why many economists are so sceptical or even hostile. This difference is explained by the comparative degree of fixity of disciplinary conventions.

These differences sometimes show in the *degree of generality* on which the virtues of a piece of inquiry are described and judged. The proponents of neuroeconomics are inclined to make appeal to fairly general principles such as “all possible sources of evidence should be consulted” and “black boxes should be opened”, whereas more

conventional economists typically appeal to more local disciplinary conventions that they take to define economic science, such as when claiming that “economics makes no predictions about the human brain” (Gul and Pesendorfer 2008) and when asking “whether neuroeconomic research is relevant to the traditional concerns of mainstream economics” (cf. Bernheim 2009).

3. The rhetoric of neuroeconomics

Whenever there is hype, there is rhetoric. The rhetoric of economics was a subject of somewhat systematic investigation some time ago (e.g. McCloskey 1985, McCloskey, Klamer, and Solow 1989, Mäki 1995). Neuroeconomics provides a new and very instructive case.

Whenever there is rhetoric, there is persuasion of an audience. The rhetoric around neuroeconomics is a series of attempts to persuade not just one uniform and shared audience, but *a variety of audiences with possibly different beliefs about what constitutes good and interesting scientific inquiry*. These audiences consist of groups of economists, neuroscientists, other social and natural scientists, students in different fields, research funding agencies, the media, and the general public. Furthermore, these are not internally uniform groups; they rather divide into subgroups with different epistemic and methodological tastes.

Audience heterogeneity is connected to the issue of tradeoffs between scientific virtues and the necessity to pursue just a limited set of such virtues at a time. Different audiences may appreciate different virtues, so success in persuading them depends on the selection of virtues and their interpretation. One approaches an audience by appealing to a combination and interpretation of a limited set of general values and standards of science and/or more local disciplinary conventions. Resolving the tensions between various scientific virtues in a successful manner is audience-dependent.

A. Rhetorical advantages

Neuroeconomics can enjoy several rhetorical advantages including the following.

1. Neuroeconomics is able to practice the *rhetoric of scientificity*, appealing to the authority of the natural sciences: “You see, we publish in *Science* and *Nature*...” It can invoke the general idea that (good) science is measurement and testing, so “to ignore a major new stream of relevant data is always a dangerous strategy *scientifically*” (Camerer, Loewenstein, Prelec 2004, 573; italics added).
2. Neuroeconomics is supported by the *rhetoric of depth and microfoundations*: it can claim to dig “deeper” revealing a hidden mechanism by “opening the ‘black box’ which is the building block of any economic interaction and system—the human mind” (Camerer, Loewenstein, Prelec 2005, 53) ; and “... the method and the standard set by neuroscience is the final goal: a reconstruction of the process and mechanism that goes from a stimulus presented to the subject to his final action in response” (Glimcher, Rustichini 2004, 452).
3. This is often accompanied by the *rhetoric of good news and progress*. The claim is that economists are now in a lucky position: recent progress in neuroscience has made it possible to penetrate into the hidden mechanisms so as to make observable what used to be unobservable. Economists have a newly emerged opportunity that should not be left unexploited.
4. A related aspect of the rhetorical package is the appeal to *increased realisticness*. Neuroeconomics has joined behavioural economics in claiming to be part of the project of offering “more realistic psychological foundations” for economics.
5. Another progressive virtue promised by neuroeconomists is *unity and interdisciplinary integration*: “Economics, psychology, and neuroscience are converging today into a single, unified discipline with the ultimate aim of providing a single, general theory of

human behavior. This is the emerging field of neuroeconomics ...” (Glimcher and Rustichini 2004, 447)

6. The rhetoric of progress is often accompanied by the balancing rhetoric of *conservative praise and compromise*. While the need and opportunity of revision and progress are recognized, the wholesale abandonment of standard neoclassical theory is not required. It is instead claimed that neoclassical economics has made useful progress, but we just need to go a little further without however necessarily dropping the past achievements from the toolbox. For example, historically conceived, “[t]he ‘as if’ approach [that has ignored psychology] made good sense as long as the brain remained substantially a black box.” (Camerer, Loewenstein, Prelec 2005, 10). One may also divide domains between theories so that “rational choice models might ... be most useful in thinking about the simplest kinds of decisions humans and other species make...” (ibid., 55). Furthermore, standard economic theory may contribute directly to neuroscience: “we think many familiar tools can be used to do radical neuroeconomics” in building “an economic model of the brain” (ibid., 56). This is a rhetoric of consolation and compromise: there is no reason to worry, there are no revolutionaries at the gates threatening your jobs and prestige.

7. The rhetoric of *modesty* is another strategy of eluding strong negative reactions. Neuroscience is said to *possibly inform* economics rather than undermining it: “How can the new findings of neuroscience, and the theories they have spawned, inform an economic theory that developed so impressively [sic] in their absence?” (Ibid., 10) Improvements in theories may happen, but are not guaranteed: “If neural mechanisms do not always produce rational choice and judgment, the brain evidence has the *potential* to suggest better theory.” (Camerer, Loewenstein, Prelec 2004, 556; italics added). Even the unification of research fields can be understood very modestly: “The synthesis ... is not meant to unify the fields, but rather to *improve* both fields on their own terms.” (Camerer 2008a, 59; italics added)

8. Next to the modest conservatism, there is the rhetoric of *novelty and excitement*. As Rubinstein observes, “the rise of neuroeconomics is coming at a point in time when

Economic Theory is not producing any exciting insights” (2008, 485). Standard theory exhibits diminishing intellectual returns to the point of being boring. Perhaps supported by the spirit of the times, neuroeconomics benefits from manifesting novelty, intellectual entrepreneurship, innovation, and excitement. It not just promises to make economics more scientific, but it has set out to inform or revise its contents and procedures in novel and intriguing ways. Novelty and thrill combined with the image of scientificity fortify its rhetorical powers.

9. The captivation of neuroeconomics is further enhanced by *the charm of colourful images of the brain*. They add to the excitement while making the key ideas appear accessible to non-specialists.

10. Finally, one should not ignore the public dimension of neuroeconomics. *Extra-academic demand and support* derive from at least two sources. First, neuroeconomics has characteristics of a *pop-science*. We, lay audiences included, are curious about who we are. We all have brains and many of us share a keen interest in how our brains function in ordinary life situations. The pop-science qualities of neuroeconomics benefit from and perhaps encourage the colourful imagery used in its expositions. Second, the prospect of using neuroeconomic information for the *control and manipulation* of human behaviour is an advantage that appeals to some audiences with practical rather than intellectual interests.

Not all these rhetorical advantages are (equally) appreciated by all audiences. For example, “[c]olourful diagrams, *which mean nothing to economists*, are presented as clear evidence” (Rubinstein 2008, 486; italics added). Yet, taken together those advantages make an appealing package for a sufficiently large range of influential audiences to help neuroeconomics march on. The share of attention it has managed to attract has made some economists conspicuously nervous.

B. Alleged rhetorical excess

With respect to many audiences, neuroeconomics has been persuasive, enhanced by successful self-promotion. It has done very well in attracting excited attention as well as in fund raising compared to many other fields in economics. Critics blame it for rhetorical excesses, for overdoing the marketing, for salesmanship promising and pretending to deliver more than it can.

Are the charges correct? This partly depends on what we mean by rhetorical excess. The very idea of inadvisable excess suggests that in right proportions, rhetoric is a legitimate part of scientific communication. When is rhetoric excessive? Neuroeconomics makes appeal to extra-scientific audiences – does this imply rhetorical excess? Or do neuroeconomists practice exceptionally vigorous or aggressive forms of marketing, making its rhetoric excessive? If this is what rhetorical excess means, how does one determine whether some limit has been exceeded?

To identify a line between legitimate rhetoric and excessive rhetoric, one must invoke some standards and virtues of science. Charges of rhetorical excess may derive from perceived violations of some disciplinary conventions of economics, or violations of the conventions of some other discipline, or of some more widely shared scientific virtues.

When exposed to neuroeconomics, an economist may develop the feeling of being a target of persuasion or manipulation: "... I almost always have the feeling of being (unintentionally) manipulated" (Rubinstein 2008, 486). Why is this? The feeling of manipulation may arise because of being exposed to some contents that are new or unfamiliar. Or it may arise because the way it is presented and argued violates disciplinary conventions familiar to the audience. When exposed to the political propaganda of party *Z* whose line is far away from party *A* that one supports, one easily develops the feeling of being manipulated, while this feeling may be missing altogether when being exposed to a campaign of one's own party *A*.

This explains why the difference between excessive and legitimate rhetoric may also be perceived as one between rhetoric and no rhetoric. Communicative practices that conform to familiar disciplinary conventions may not *feel* rhetorical at all, while those that do not conform to those conventions, are perceived as rhetorical or manipulative.

Many complaints about rhetorical excess appeal to some more general principles of scientific rationality shared by a wide range of disciplines. Neuroeconomics is claimed to draw hasty and unwarranted conclusions from experiments. “Lack of knowledge and uncertainty are swept under the rug.” (Rubinstein 2008, 486) It is accused for sloppiness and lack of rigour in data analysis and statistical reasoning while pretending to be in the business of doing hard science. While publishing in *Nature* and *Science* is found persuasive by many, critics may take this as an indication of sloppy refereeing by those journals (Harrison 2008b, 538).

Rhetorical excess may also be taken to involve misrepresenting its own performance for the sake of self-promotion. Presenting old non-neuroeconomic results as achievements of neuroeconomics would be an example. Another is overdoing the otherwise persuasive appeal to the new opportunities created by progress in neuroscience by making claims such as “[f]eelings and thoughts can be measured directly now, because of recent breakthroughs in neuroscience” (Camerer, Loewenstein, Prelec 2004, 556). For this to be correct, some very radical and hard-to-justify solution on the mind-body problem is presupposed, so it is more likely to be a factual error.

C. Justification of rhetorical excesses?

One can try to explain the alleged rhetorical excesses covering up deficient scientific performance by making claims such as “tough competition has led to low standards” (Rubinstein 2008, 486). Or one may observe that the cost of neuroscientific technologies is declining fast, which is “bad for the literature” since “experiments can be thought up and run with little forethought on design” (Park and Zak 2004, 384-385). Or one may point out that in using the major technique, fMRI, mistakes occur easily (Savoy 2005).

As we move from tough competition and declining cost of research techniques to the difficulty of identifying errors in their use, we move from mere explanation towards justification. But there are other characteristics of neuroeconomics that could also be presented in justifying its alleged rhetorical excesses. Perhaps there are some special conditions under which excessive rhetoric is not just understandable, but even excusable.

One such circumstance is characterized by *interdisciplinarity* in contrast to disciplinarity. In the latter case, disciplinary conventions define the specific rules and principles that are well recognized and are supposed to guide scientific conduct. In an interdisciplinary case (of a suitable kind), the rules and standards of conduct tend to be ill-defined and partly open-ended, scientific activity tends to be under-disciplined, not being under the full disciplinary control of any of the participant disciplines. Tensions may arise between the conventions of the participant disciplines, pulling in different directions and thereby leaving practitioners without tight guidance and constraint. Such tensions may not be quickly resolved in favour of those of any of the participant disciplines.

A related and reinforcing circumstance is characterized by *novelty in the sense of immaturity*. This is novelty relative to the own past of an activity. The activity is *not yet* disciplined, the standards for its disciplining are not yet fully established. The activity is still in its early stages of development, therefore one should not rush to pass any harsh or final judgment. A proponent may add that change and progress occur fast and that the critics therefore easily remain uninformed about the latest developments and so should be cautious with their criticisms. As Camerer puts it, "... the field is moving rapidly. Perspectives expressed a couple of years ago may be replaced by more thoughtful ones. ... it is difficult to keep on top of the field and describe it accurately." (Camerer 2008a, 63). So if you feel disinclined towards neuroeconomics, you should not rush to negative judgments because, first, you might be misinformed about your target, and second, deficiencies should be forgiven due to immaturity and because the field is improving.

Finally and importantly, there is *novelty in the sense of new market entry*. This is novelty relative to other approaches in the semi-competitive market for academic attention and resources. For a novel player to enter the market and survive the early challenges may require, or at any rate may be helped by, manoeuvres that are not appreciated by the more established players in the market. In order to create institutional space for a new initiative, one must represent oneself in an attractive and favourable light. One needs to create an enthusiastic and committed community of practitioners, a receptive audience, publication possibilities, financial and administrative support, jobs, career prospects, etc.

It is important to see that in contemporary science, such social success is a prerequisite for epistemic success. The epistemic potentials of an emerging research programme cannot be actualized (nor estimated) without mobilizing massive academic resources in its support. Such resource mobilization requires overcoming the resistance of prevailing disciplinary conventions. Whenever something new and different is being tried, there will be friction and resistance deriving from the dominant cognitive and disciplinary structures. The challenge is to bypass or overcome them, and this can be helped by the rhetoric of promise and exaggeration.

Much of this rhetoric may be excusable simply as part of a larger campaign to create social and epistemic space for the new initiative. Naturally, if that larger campaign has little substance, little else besides the rhetoric, then the rhetoric is harder to justify. In a long enough run, the rhetoric must be aligned with the actual substantive performance. None of this means that we should not be keen to reveal the actual reality behind the rhetoric at all times – just that even a fairly large distance between the rhetoric and real performance may not be that fatal at the early stages of a research endeavour. This seems to be understood amongst neuroeconomists themselves: “These early neuroeconomics papers should be read as if they are speculative grant proposals...” (Camerer 2008a, 44)

D. Rhetoric of “hype”

The debate has been asymmetric in that one party charges the other for inappropriate ‘promotion’, ‘marketing’, ‘manipulation’, and ‘hype’, using ‘rhetoric’ and related expressions pejoratively. It seems the critics of neuroeconomics have pretended to identify dubious rhetoric from a non-rhetorical point of view -- and thereby to cast doubt on that rhetoric and what is marketed by its means. But rhetoric-free criticism is not possible. In criticizing a rhetoric, one exercises rhetoric oneself: Rhetoric2 about Rhetoric1.

Rhetoric2 is the rhetoric of ‘rigour’ and ‘good economics’, ‘hype’ and ‘sloppiness’. This second-order rhetoric may both reveal and hide sloppiness and failures of rigour that derive from different sources. While the interdisciplinary newcomer is *under-disciplined*, the mono-disciplinary establishment is *over-disciplined*, leaving its own disciplinary conventions and convictions unexamined and therefore unjustified. The admirable rigour of conventional economics is based on a sloppy acceptance of its core conventions.

So there is rhetoric on both sides and there is sloppiness on both sides. The good news is that the confrontation might help both parties to improve their rigour.

4. Hope

That neuroeconomics involves hype does not rule out entertaining hopes or expectations that some desirable condition - outcome or process - will or might result from the endeavour. Rubinstein complains that “even Neuroeconomics researchers with a good understanding of Economics are in the dark about how it will reshape Economics” (2008, 486-487), but this perspective is too narrow for dealing with all dimensions of hope.

First, the expectation may as well be about the future of neurobiology (or neuroeconomics itself) rather than just that of economics. Second, one may have hopes without being able to tell what precise desirable consequences there will or might be.

Third, the concept of ‘reshaping’ is neutral with respect to normative notions such as ‘progress’. Regressive reshaping is also conceivable. Yet, if progress is desirable, then so is reshaping. Progressive improvements may be hampered by the stickiness of prevailing disciplinary conventions, so these may have to be reshaped. Fourth, the progressive services of neuroeconomics to economics might be indirect in that it merely facilitates progress by shaking the discipline in the discipline of economics. Shaking is taking place, so neuroeconomics has already fulfilled some small hopes.

To say that hype *is compatible with* hope is to make a rather weak claim. The proponents typically make stronger claims such as neuroeconomics *is able* to generate improvements. This is a claim about possibility or capacity, and it can be made with more or less assurance. Critics of the excessive rhetoric of neuroeconomics may also join in making this claim, but usually do it with less assurance (e.g. Harrison 2008a). An even stronger claim would be the assertion that neuroeconomics *will* bring about such consequences, but this would be harder to justify.

At the other end of the spectrum, the strongest claim made is that neuroeconomics is *irrelevant* to economics. This implies neuroeconomics has no capacity of bringing about desirable consequences - there is *no possibility* of such consequences to materialize. The project is doomed to failure from the start. And this is not because of its sloppiness, lack of scientific rigour, or excessive rhetoric. It is because of *the nature of economics*. It is implied by the true essence of economics that the neuroeconomics project is based on a deep misunderstanding if conceived of as an attempt to improve economics. This claim is made with full assurance. (See Gul and Pesendorfer 2008.) What supports the claim is a highly local and firm disciplinary (or sub-disciplinary) convention rather than any supposedly general principle of good science.

There are a number of reasons for entertaining hopes, but I briefly focus on just one recurrent theme: *the evidential role of neuroeconomics*. The irrelevance claim denying the relevance of neuroscientific evidence to economics goes like this: “Economic models make no predictions nor assumptions about body temperature, blood sugar levels, or

other physiological data, and therefore, such data cannot refute economic models.” (Gul and Pesendorfer 2008, 19)

This essentialist claim must be rejected. First, as we have learnt from several years’ investigations in economic methodology, no data can “refute” a model. The evidential role of neuroeconomic data has to be something weaker, put in terms such as data *constraining* models and helping *compare* models for their performance, or perhaps just playing some *explorative* role. Second, whether a model “makes predictions or assumptions” about something is not always explicit. Models are *not fully transparent* in what they imply or presuppose. Third, economics has *no immutable essence* such that it would be fundamentally committed to *behaviourism* (not behaviouralism) so that only data pertaining to observable choice behaviour would be relevant.

It is a global characteristic of scientific models and theories that they are underdetermined or under-constrained by any given body of data. In principle, an infinite number of models are consistent with given data. Further constraints are needed to choose a model. In practice, many of these constraints derive from various local disciplinary conventions that are not uniform across disciplines. Conventional economics has its own ways. Some of these ways reflect the peculiar characteristics of its (human and social) subject matter, while others are more accidental characteristics of disciplinary history.

Given that economics is faced with a particularly severe issue of evidential under-constraint, one would expect any further source of evidence to be welcome to alleviate the problem. Neurobiological data would be one obvious candidate, among others, to be given a role in deciding between observationally equivalent models of choice: “... if two theories can both explain the Ellsberg paradox, and appear equally promising for explaining some pattern in, say asset prices, then if one of the theories also is neurally plausible, that theory should be taken more seriously.” (Camerer 2008b, 47) Or more generally: “Now we have several models [of human behavior facing uncertainty] competing, each claiming to be the unique predictor. Which one do we choose?”

(Rustichini 2005, 203) Predictive evidence alone, pertaining to overt choice behaviour, does not constrain the model choice sufficiently. In this situation, proponents of neuroeconomics argue, it is not scientifically advisable not to consult further sources of evidence. “Stating, as Gul and Pesendorfer do, that ‘standard economics focuses on revealed preference because economic data come in this form’ is equivalent to cursing the darkness rather than lighting a match.” (Schotter 2008, 74) This is to question an alleged disciplinary convention head on.

But nothing is simple and straightforward in science. The under-constraint issue gets new wrinkles when tried to be remedied in terms of neuroeconomic data, collected by functional magnetic resonance imaging. One such wrinkle is due to the difficulty of using fMRI well. The user must have a solid knowledge of great many things, such as the physics and engineering of the imaging device; the physiology and hemodynamic responses being measured; the structural and functional anatomy of the brain; inference from MR images to statistical activation patterns; how the above interact with psychology in the design of experiments; and how all the things interact when attempting to interpret experimental results. Due to this multiplicity of components, “there are so many ways to go wrong” (Savoy 2005, 362). Not only is the challenge very difficult so it is easy to make mistakes, but identifying those mistakes is also difficult. The number of studies is growing rapidly, and it is hard to find competent referees who happened to have just the right kind of expertise and experience required for recognizing particular kinds of error (ibid., 366).

So if neural data were the sole sort of evidence used, the problem of under-constraint would be severe indeed. But the point is not replacing other sorts of evidence but supplementing them. What is required is proliferation of (kinds of) evidence and their triangulation, cross-checking them against one another, mediated by various theoretical background assumptions. So issues such as whether neural data is inferior to econometric tests with market data (and then choosing the latter while dispensing with the former) should not arise, or at any rate should not play any important role. Using multiple sources

of evidence and their triangulation is a way of improving the quality of data as well as the quality of assessment of models. Or so one might hope.

5. Concluding remarks

The rise of neuroeconomics has created one of those rare historical occasions on which practicing economists themselves are inspired and forced to engage in reflection on foundational issues in their discipline. These include issues that concern interdisciplinarity, the identity and boundaries of economics; the goals and values of science, and the standards of scientific quality; issues in modelling, explanation, and epistemic justification. Philosophers and methodologists of economics are experts on these issues, so have a golden opportunity to contribute and to be heard.

Opportunities may remain unexploited. A recent edited volume (Caplin and Schotter 2008) dealing with the challenge and consequences of neuroeconomics manifests such a failure on the part of philosophers and methodologists. Curiously entitled as *The Foundations of Positive and Normative Economics: A Handbook* (the first to appear in the *Handbooks in Economic Methodologies* Series), the volume proceeds as if *economic methodology* did not exist as a progressive research field whose numerous research results could be utilized. Whatever the diagnosis of this failure, the challenge remains to offer enlightening non-negligible interventions in the ongoing debates.

Neuroeconomics is an ally of the philosophy of economics in two ways. It is a challenging research initiative that calls for a scrutiny of basic issues, and so offers opportunities for philosophers and methodologists to excel. And it has managed to prompt debate that forces all parties explicitly to reflect on the disciplinary conventions and presuppositions of inquiry: practicing economists are joining the specialists in economic methodology in addressing what the latter define as a major part of their task.

We cannot stop neuroeconomics. But we can learn a lot about economics - and science in general – by scrutinizing it and the debates around it. At least we have reason to hope so.

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