

## REVIEW SYMPOSIUM

**Economic Theory and Cognitive Science: Microexplanation**, by Don Ross  
Cambridge, MIT Press, 2005  
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### The brains behind economics

Let me begin with two propositions:

- (1) *Economic decisions are made in the brain* (not the toe or elbow). As a result, economic regularities can be understood, and economic models are based, on cognitive science. While I find this truth self-evident, many economists disagree with this statement (e.g. Gul and Pensendorfer 2006).
- (2) *The brain is an economic system* (indeed every biological entity is an economic system). That is, the brain, as well as human beings, have objectives and face constraints on achieving those objectives. Because resources are scarce in all biological systems including the brain, they have evolved reasonably efficient ways to achieve objectives. A related statement was made by the late evolutionary biologist W.D. Hamilton who argued that game theory has proved more useful to biology than economics (you may argue among yourselves about this).

If you accept these two propositions, you have my permission to skip Don Ross' book (unless you simply must know the philosophical basis for them). If you do not buy my propositions, or are worried about the methodological foundations of economics, go get Ross. Ross' goal is to determine if economic theory has produced a discipline that is a proper empirical science. The book, then, is about how we do economic theory, using the philosophy of science to poke methodological holes into neoclassical theory. Ross does this not to be a spoil sport (too easy), but to plug those holes using cognitive science.

This volume rips some pretty big holes in our beloved discipline, so much so that filling them requires a second volume that is forthcoming. His audience is both methodologically interested economists and like-minded cognitive scientists with a hankering for economics. The analysis is synthetic, but deeply and deftly applied. While Ross seeks a playful tone, for example, setting up dialogs between proponents of various views, the material is sometimes excruciatingly dense and not for the casual reader.

The analysis concludes with a strong case that evolutionary cognitive science is the only framework that adequately supports economic theory. This nicely supports the recent interest in behavioral economics and more recently neuroeconomics as valid approaches (for a recent survey of neuroeconomics, see Zak 2004). For those of us working in these areas, we have already digested this approach and need not grind through the philosophical arguments. (If you want to work in these areas but are not interested in the methodology, I recommend starting with Colin Camerer's

*Behavioral Game Theory* or Herbert Gintis' *Game Theory Evolving*.) For the methodologists around, I say dig in!

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## References

- Camerer, C. (2003), *Behavioral Game Theory: Experiments in Strategic Interaction*, Princeton: Princeton University Press.
- Gintis, H. (2000), *Game Theory Evolving*, Princeton: Princeton University Press.
- Gul, F., and Pendorfer, W. (2006), "The Case for Mindless Economics," SSRN Working Paper.
- Zak, P.J. (2004), "Neuroeconomics," *Philosophical Transactions of the Royal Society B (Biology)*, 359, 1737–1748.

### Can the philosopher be a mediator between economics and cognitive science

First of all, the title of the book is a bit misleading since it does not really deal with the entangled links established over the past 20 years between economic theory and cognitive science. It is rather a reflection about the insights and critiques that cognitive science may bring to economics as concerns some philosophical questions. In fact, cognitive science is loosely defined in the book since, if it explicitly includes cognitive psychology and neuroscience, it is more implicit about incorporating or not Artificial Intelligence or computer science. Economic theory is better delineated, but the book jumps frequently from market theory to game theory, assuming wrongly that they hold the same assumptions and adopt the same methods. In fact, the book deals with a lot of subjects, historical or methodological ones, and it is hard to follow its general trend. Nevertheless, three topics will be considered: the concrete works that link the two disciplines; the specific problem of the status of mental states; and the role of philosophy in confronting the disciplines.

Few precise works where cognitive science influenced economics are clearly presented in order to discuss them. However, cognitive science can be assumed to act on economics at three levels involving different stakes. First, it proposes some theoretical assumptions which may correct or enrich the usual economic assumptions. For instance, original assumptions were introduced about the structure and the revision of agents' beliefs or about bounded rationality and adaptive processes. Second, it suggests some 'engineering' tools that may facilitate the computation by the modeller of consequences of his assumptions. For instance, new methods were applied concerning simulation of computerised models or evaluation of the complexity of computing individual choices and equilibrium states. Third, it provides some new types of experimental data in order to improve the empirical validation of the models. For instance, various biases in agents' reasoning were at an early stage stressed by psychologists and choice mechanisms are more recently detailed by neuro-economics.

More precisely, the influence of cognitive science on economics may be summarised in three research programmes (the two first being integrated in 'cognitive economics'). The epistemic programme deals with the reasoning processes implemented by the economic agents on their beliefs (especially in finance). It is inspired by developments of formal disciplines such as epistemic logics and probability theory. The evolutionist programme deals with boundedly rational agents following some learning rules or evolution rules (especially on the job market). It has a larger scope than just the influence of evolutionary biology and even evolutionary cognitive science since it is concerned with various modes of adaptation of an agent to its environment. The behavioural programme is a more precise study of the heuristics followed by economic agents in reasoning and in choosing. It insists specifically on empirical work in all possible forms. All these programmes are generally not considered as really antagonist, but relevant in specific contexts, even if these contexts are not clearly delineated.

In brief, economics imports selected works from cognitive science in order to enrich its theoretical framework and its empirical validity. These works are only considered when they lead to manageable formalisms and allow inferring original consequences. Sometimes, economists feel that cognitive science, like other disciplines such as physics, tries to force some ideas or frameworks into economics, but the usual strategy against 'entrism' is always the same. Economics poses its problems in its own terms and if it observes that other disciplines have already solved them, it examines how their solution can be adapted. Usually, it is hard to observe a real struggle between the two disciplines and above all, no idea of reduction of one discipline to the other is apparent. In summary, the five scenarios proposed by Mirowski and recalled by Ross can already be assessed, and the most extreme ones seem to be the less plausible, at least at the present time.

Concerning the mental states of the agents, they first can be analysed in a theoretical way. Don Ross defends the notion that the mental states are overemphasised by economists, but it can be observed that they remain at the basis of many new developments. With the influence of cognitive science, preferences become less considered in economics to the advantage of beliefs (an interest followed by many philosophers like van Fraassen or van Benthem). Aumann, as an eminent representative of the epistemic programme, enters deliberately in player's mind in order to formalise by crossed beliefs how he represents his strategic interactions. Camerer, a founder of behavioural economics, proposes a model of cognitive hierarchy, in which each agent is assumed to expect at a higher level than the others, in order to explain their behaviour. Even evolutionist game theory justifies the usual learning rules in terms of beliefs and preferences, for instance when they are judged according to the exploration-exploitation trade-off they ensure.

These mental states may be analysed in an empirical way too. Don Ross considers that introspection is not an acceptable means of investigation but again, economists more and more consider mental states as observable. They are considered as valuable not only in a context of discovery for suggesting original assumptions, but even in a context of proof by providing original data. Kahneman, a specialist of experimental economics, makes laboratory experiments where an agent announces his felt utility about some phenomenon, which may deviate from a more objective assessment of it. Glimcher, in a special issue of *Games and Economic Behaviour* (2005) devoted to neuro-economics, shows that experiments in the field seem to

indicate that agents reason not only with rewards and probabilities, but even seem to combine them along usual choice rules under risk. More generally, some economists try to compare, in the same setting, the mental states announced by the agents and those revealed by the modeller through their behaviour and to explain the observed discrepancies.

In economic theory, four ontological levels are classically considered, characterised by specific variables: neural (operations in the brain); mental (reasoning on mental states); individual (implemented actions); and collective (social effects). Even if he does not really stick to it, Don Ross discusses at length 'eliminativism', a philosophical stance which considers that the second level should be skipped. Such a position would of course be devastating for economics. But the real question is to know if there are some shortcut influences between the first and the third levels. More precisely, can some individual actions be explained only by neural factors without transition by mental states? The answer should be given neither by the economist, nor by the philosopher, but precisely by the neural and behavioural scientists. For the moment, mental states help the economists to construct convincing explanations, and they will adhere to them as long as some phenomena cannot be explained better without them than with them.

Finally, the problem is to make clear what role the philosopher wants to play and is able to play in the debate between cognitive science and economics. He just may use cognitive science as an ally in order to criticise economics, considered beforehand as poor science. This is sometimes the case in the book, without really explaining why cognitive science is more reliable than economics. He may consider that cognitive science and economics interfere too naïvely since their links are just thought as instrumental. This is obviously the case in the book since it tries, with some success, to show that the disciplines have proper epistemological and ontological assumptions which cannot easily be mixed and are even antagonist. He may even consider that both disciplines are (possibly to different degrees) subject to criticism since they rely on incoherent or fallacious principles. This is the case when the book designs its own philosophical position, and implicitly considers that it has to be adopted by the scientists.

On the one hand, the book makes very selective references to economists. It considers essentially the epistemological positions of Samuelson, Binmore and Sugden, positions that are specific in the profession and not always made completely explicit. Conversely, little mention is given to Simon, a Nobel prize winner in economics and founder of Artificial Intelligence, as well as to Camerer or Rubinstein, who sustain quite other positions. Moreover, it underestimates the fact that the same formalism can be interpreted in a lot of different epistemological ways. On the other hand, the book does discuss the proposals of other philosophers such as Dupré, Dennett or Mirowski. Except for the last, they were not especially interested in economics while other philosophers of economics are without doubt missing. Even if it seems reasonable, to postulate that the analysis of human behaviour made by generalist philosophers remains globally relevant for economic agents in all circumstances has to be justified.

Finally, the book illustrates well why the philosophical discourse about human behaviour in society is not really taken into account by the economists (and probably by the cognitive scientists). The first reason is that the philosophers do not go into the details of economic theory and frequently analyse concrete work on a superficial

level or even through former commentators. The second reason is that the philosophers do not generally respond to the questions which are of interest for the economists, even if such questions have been made explicit by some economists. The third reason is that the philosophers talk mainly with other philosophers and entertain autonomous debates where they answer to questions they alone are asking and find relevant. For these reasons, economists work hard in order to advance new ideas and they consider that they need not be inspired by explicit epistemological positions, even if they are in fact. Hence, the real debate is not between economics and other disciplines, but between economics and philosophy.

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### **On Don Ross's defense of neoclassical economics**

In his *Economic Theory and Cognitive Science* Don Ross undertakes the ambitious attempt to provide a coherent portrait of the entire discipline of economics as it relates to recent developments in cognitive science. His result in this first volume of what is supposed to be 'a two-volume study of the science of economics in relation to other branches of behavioral inquiry' is a polemical defense of neoclassical economics or more specifically of the version of neoclassical economics that he tends to favor. His defense, however, is not convincing.

'Anyone writing an interdisciplinary book that advances a novel, polemical thesis (rather than being a bridge-building survey) faces delicate problems of audience location' states Ross at the beginning of his book (p. 30). He decides to solve this problem by adopting a third available disciplinary perspective, i.e. other than economics and cognitive science, from which to address his audiences: the philosophy of science. Given his decision to assume such a perspective, one could legitimately expect the author to provide a discussion of the different research programs that prevail in economics and a balanced discussion of the merits and deficits of them, mostly with respect to the domains in which they overlap with cognitive science. There is, however, a lack of discussion of even the most obvious candidates such as evolutionary economics and new institutional economics and consequentially no serious appraisal of alternative, sometimes heterodox, research programs that have taken seriously the problems that cognitive science is dealing with. This is not of course to suggest that one should not focus on the research program of neoclassical economics, or neoclassicism as Ross calls it, since it is definitely the predominant program, and probably rightly so.

However, Ross opts rather to bypass the alternatives entirely, to merely provide a detailed discussion of the different versions of neoclassical economics and to use cognitive science to support the one that he favors. He admits that he has 'spun the account to come as an updating and defense of neoclassicism in the light of cognitive science'. He adds, 'Many economists are likely to find this distinctly eccentric [...]. So surely I am just being perverse in selling my argument as a vindication of neoclassicism' (p. 28). But regardless of whether this is eccentric and perverse or not, his defense of neoclassicism is less convincing since there is no contrasting discussion of alternative approaches in economic theory.

Furthermore, his defense of neoclassicism is not very systematic. The book consists of a patchwork of diverse discussions pertaining to neoclassical economic

theory and/or cognitive science which are only loosely connected. In a book that is over 400 pages long, the author's own argument starts only on page 276. Up to that point one is supposed to have become equipped with the knowledge of the above-mentioned discussions in order to be able to understand the author's argument. But even in the last chapters of the book the argument that Ross is making is not perfectly clear. One is forced to disentangle what his own views are from what he argues against Mirowski, Dupré and some other authors. The lack of a clear-cut argument makes his defense of neoclassicism unfortunately very difficult to follow and thus hardly convincing.

Let me now come to a more substantial criticism of Ross's account. I will try to reconstruct his main argument as well as I can, following the text as closely as possible, and I will then provide my criticism. The version of neoclassical economics that Ross defends is different from Gary Becker's standard approach (which Ross labels 'mature neoclassicism') in not sharing the most fundamental ontological assumption of Becker's strategy – namely, that people are economic agents (p. 154). Agency, thus, is the main ingredient that makes Ross's account of neoclassical economics different from Becker's. Ross's account is also different from the account of behavioral economics because even there one starts doing economics with the presupposition of a self, which according to Ross is exactly what one ought not to be doing:

[I]f we *begin* by positing a well-ordered macroeconomy composed of individuals competing in a vast natural marketplace, with selves then emerging as a technology for improved competitiveness, we logically guarantee that the result won't shed any interesting light on the foundational questions about economics itself that constitute our topic here. [...] Putting the point yet another way: one cannot somehow vindicate the empirical usefulness of neoclassical economic logic to the study of people by presupposing an atomistic neoclassical framework and then building an evolutionary behavioural science on the basis of it. We will then simply get out of our analysis what we put into it. (p. 275)

The thrust of Ross's argument is, thus, that a successful neoclassical economic theory should start from the point that selves are not given. The first step of his logic is the individuation of biological organisms in non-economic terms.

We begin with organisms that are individuated genetically, not economically. It turns out that we can use Samuelsonian microeconomics to build models of these systems as they respond to scarcity in food sources, mating opportunities, and other resources that give us surprising but accurate empirical predictions. This in turn feeds back to provide constraints on the cognitive models of the animals. Here, then is the relationship between economic theory and cognitive science that was hoped for in the optimistic early days of classical AI. (p. 277)

Since increases in non-parametric environmental complexity are bound to arise in social interactions, one should not refer to games played between players but should rather introduce the idea of strategic situation types since

we can't assume our initial individuation of agents to remain stable as we let socialization feed back into their economic agency profiles. Identifying a scenario as a game presupposes that player's strategy sets have already been constrained by determination of their specific utility functions. But the whole point of the hypothesis of Dennett's that I am working up to here – which is also the point of denying Robinson Crusoe metaphysics – is that a raw, socially unrefined instance of *H. sapiens* isn't yet a *human self*. If utility functions in games among individuals attach to human selves, then we can't identify a game *G* among two or more such selves with the pre-constrained

situation S that they might be modelled as confronting if they were rhinos. What we have to explain is what allows socialized agents to *get from* situation types to particular games. (p. 278f.)

Now, Ross introduces the distinction between what he calls game G and game G'':

Let me therefore construct, simply for explicative purposes (since it has no actual empirical models) the concept of a situation-type S that abstracts away from the differences between the game G (as a model of S) faced by two selves and another game G'' (as an alternative model of S) they *would* play if they were straightforward economic agents equipped with the biological *H. sapiens* utility function. (p. 292)

His approach is supposed to provide guidance about how to solve games of type G, since the games of type G'' 'are only played by infant and perhaps severely autistic *H. sapiens* individuals' (p. 292).

One legitimately expects Ross's novel approach to come to the fore at some point and that we will learn how to solve games of type G, that is games that real selves are playing and not merely games of standard economic agents like those given in standard neoclassical economic textbooks. However, one does not learn anything about this supposedly central problem of the book. Instead, what Ross calls the Robbins–Samuelson Argument Pattern is finally presented in the last chapter of the book. It turns out that Ross's favorite version of neoclassical economics is nothing other than Samuelson's Revealed Preference Theory combined with Robbins' account of scarcity. This leads to the following rationale:

Suppose you want to explain and/or predict what happens when the members of a group of one or more goal-directed systems, in causal interaction wherever the group is larger than one, pursue ends that cannot all be satisfied given available common resources that have alternative uses. In that case, use as much evidence as you should [...] gather about their behavior to represent the schedule of ends pursued by each system as a [...] utility function, defined as per axioms that admit of solution by simultaneously maximizing each utility function for a given allocation of resource constraints. Note first that this makes two features fundamental to a phenomenon's being 'economic': *scarcity* (Robbins) and *agency* (Samuelson). It also identifies the basic object generalization as the interacting group of agents rather than the individual, although the individual may be the limiting case of the group in Robinson Crusoe situation. (p. 377)

The two steps of the Robbins–Samuelson Argument Pattern are: (1) 'Empirically identify a maximization function for each agent in the network of interactors'; and (2) 'Identify the constraints on G''-level games playable by the agents. Identify the specific scenario to be explained with one such game, and find that game's Nash equilibria' (p. 378). My short but crucial objection can perhaps be summed up in one question: How is this different from standard analysis? At the end of the day what is on offer here is a solution of the G''-level games that normal utility maximizers are playing and not a solution to the G-level games that selves are playing. Only the latter, however, are supposed to deliver the correct depiction of real-world situations whose solution cognitive science à la Dennett would help to provide. What has been promised, thus, is unfortunately not delivered.

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### Reply: economists, philosophers and rival mythologies

The three critics who contributed to this symposium might seem, to people who haven't read *Economic Theory and Cognitive Science: Microexplanation* (ETCSM), as if they must be discussing three entirely different books. Paul Zak thinks that the book 'rips some pretty big holes in our beloved discipline' but appreciates that it aims to fill these holes by appeal to resources from evolutionary cognitive science. Chrysostomos Mantzavinos objects, on the other hand, that it can't be thought to have adequately defended neoclassical economic theory because it doesn't consider heterodox challengers such as evolutionary or institutional economics; thus it apparently doesn't even try to rip holes, let alone big ones, in orthodoxy, a point which Mantzavinos registers as his 'crucial' complaint. Finally, Bernard Walliser thinks that ETCSM is merely a long utterance in a solipsistic conversation among philosophers about topics that economists will (and should) find uninteresting. It must follow from this that on his view, nothing important about the relative status of competing conceptions of economic theory can be at stake here in the first place.

How can one and the same book be read by three well-informed critics in such contradictory ways? I think this reflects the extent to which economists, like other working scientists, fly without theoretical foundations. Many philosophers would read that remark reflexively as a criticism of economics. Even Zak implicitly suggests that he would, since he thanks me for tending the foundational garden (though he doesn't advise everyone to 'grind through the philosophical arguments'). In fact, however, my view on the relationship between philosophy and sciences, including economics, is very close to Walliser's. So let me begin with that. How is it that I can share Walliser's low opinion of the value of philosophy and yet have bothered to write a book like ETCSM?

I should first mention that I spend more than half of my professional time as a working (i.e. consulting) economist, in a developing country. The relevance to this activity of considerations from the foundations of economic theory is: none whatsoever. Like most economists, I divide my activity almost exclusively between figuring out how to isolate relationships for estimation and empirical measurement, and actually gathering and analyzing data. I have never, in the course of this, estimated or tried to confirm an attribution of a utility function to anyone: I just go straight to actual prices, demand schedules, production functions and elasticities. Working always with aggregate data, it never makes the slightest difference to me whether the agents who constitute the systems I study are 'rational economic agents' or satisficers who rely on sloppy heuristics and are bad at thinking about probabilities. When, to pick a recent example at random, I want to determine whether the South African government should shift the ratio of asphalt to gravel roads when the price of petrol rises, theoretical issues over agency have no bearing on the relevant modeling.

I think that far too many philosophers would be surprised to hear this. In my experience, philosophers often write as if economists, like philosophers themselves, are preoccupied with abstract theoretical questions that they try to settle by means of *a priori* arguments. Ironically, one of philosophers' favorite criticisms of mainstream (typically called 'neoclassical') economic theory is that it is closed to empirical disconfirmation. Most economists, if they read this literature, would be amazed to be told that they are out of touch with the empirical world. They are, however, too busy with their mountains of empirical data to have time for philosophers, just as Walliser



says. The philosophers, despite their avowed concern with sensitivity to empirical phenomena, often don't seem to have strolled across campus to observe what economists do. (There are of course noteworthy exceptions to this, and more and more of them all the time – what I've just said, and what Walliser says, applies not at all to Anna Alexandrova, Erik Angner, Nancy Cartwright, Francesco Guala, Paul Humphreys, Harold Kincaid, Jim Woodward or many other superbly informed philosophers of economics, especially younger ones we're just about to hear from. Sincere apologies are offered to the dozens who deserve to be added to this list.)

Close attention to economists' routine practice might lead someone to the extreme conclusion that is the opposite of the philosophers': that economists should forget about theory entirely. After all, if foundations don't matter *at all* then how could something called 'economic theory' be thought to have ontological significance of any sort? Milton Friedman famously offered an account of the point of economic theory that accords with such metaphysical agnosticism: an economic theory is just a device for organizing data in hopes of generating predictions that pan out. Such theory doesn't even purport to offer generalizations about the empirical world. If that is all we want from economic theory, then it doesn't matter how many such theories we have, or whether they contradict one another, or even bear any worked out relationships to one another. All the economist needs to know are some rules of thumb for deciding which theory to pull off the shelf first for various types of practical problems.

Plenty of economists do in fact take this attitude, which serves them well and isn't subjected to much or any countervailing pressure, even from the demands of teaching graduate students. What we mainly must teach students these days is how to use various sophisticated software packages, which are self-evidently tools (that, let it be added, incorporate a great deal of powerful 'theory,' but only in the Friedmanesque sense).

Yet none of the three critics in this symposium strike this attitude. Zak comes closest – and in light of this it's noteworthy that he's most sympathetic (by far) to the project of ETCSM. Walliser thinks that if beliefs aren't real objects literally computed in people's heads then this would be 'devastating for economics', thus apparently supposing that at least one metaphysical commitment matters a lot. It furthermore constitutes evidence, according to Walliser, that beliefs and in-board computations *are* 'real' that some economists, in what Mantzavinos calls 'heterodox research programs', put proxies for such states and computations in their models. As for Mantzavinos, he says it should have been my responsibility, if I was going to insist on doing philosophy, to critically compare these different research programs and provide a clear argument in defense of my favorite one, viz., neoclassicism. It's quite true, as he says, that ETCSM doesn't try to do that. I will explain why.

There is, in my view, only one justifiable reason for not endorsing Friedman's pure pragmatism about economic theory (and, therefore, Zak's cheerful advice to skip the argument grinding over foundations). This reason arises just in case one believes there may be some non-trivial distinctively economic generalizations (i.e. generalizations that aren't psychological or anthropological or purely mathematical) to be had about some phenomena. Economics is part of *science*, as opposed to engineering, if there are such generalizations. If economics is part of science, then its generalizations had better be compatible with those discovered by neighboring disciplines; otherwise either economics doesn't explain phenomena or those other

disciplines don't (or both). One way in which economic generalizations could be compatible with those of other disciplines is by reducing to them. In that case economics would collapse into one or more of these other disciplines – presumably psychology or, more plausibly (as Zak suggests), neuroscience.

ETCSM is a long argument for the conclusions that (1) there are indeed distinctively economic generalizations, which (2) don't reduce to those of psychology and/or neuroscience because (3) they aren't about people in the first place. Rather, they are about processes of generally non-parametric, though occasionally competitive, coalitional bargaining over scarce resources, set in the dynamics of ontologically unstable agents that no other science studies directly, because no other science individuates them with the economist's purposes in mind. Neuroscience is highly potentially relevant to this enterprise but won't displace it.

This picture doesn't correspond exactly to *any* established research program in economics. That shouldn't be surprising, for two reasons: first, because it is based on brand new science that only recently became possible to do (thanks to computational technology); and second, because economists, as acknowledged above, aren't mainly in the business of attending to their own theoretical foundations.

At no point in ETCSM do I suggest that theoretical foundations should be an area of priority attention for economists. I don't think they should be. It doesn't follow from this that no one should pay any attention to these foundations, ever. Compare: the health of the economy of Montenegro isn't a priority for the international economic institutions; therefore, no one in these institutions should ever pay any attention to the Montenegrin economy. Citizens of Montenegro will be especially alert to the fallacy here.

From too many economists who *do* want to be regarded as scientists rather than just engineers (so, whose Friedmanesque modesty is only skin deep), what we get is *slapdash* attention being paid to foundations. (Again, I could use up far more space than is available listing distinguished exceptions: Ken Binmore, Partha Dasgupta, John Davis, Kevin Hoover, Robert Sugden, John Sutton – and, though some others on this very list might disagree, Phil Mirowski.) From this stale methodology literature we continue to be given the hoary picture of a competition among vaguely characterized 'research programs' called 'neoclassicism', 'behavioral economics', 'institutional economics' and so on. Now fading from prominence in these conversations are the former challengers to orthodoxy, 'Marxist economics', 'post-Keynesian economics', etc. These programs supposedly compete to deliver predictions and explanations of empirical phenomena, and eventually one of them will win and we'll have the theoretical truth. Economists are presumably the people to monitor this competition since, we're told, philosophers are preoccupied with other issues that economists don't care about.

This traditional picture has no more relationship to an intellectual world outside itself than does that of the disengaged version of philosophy of economics dismissed by Walliser. Many economists are now doing terrific empirical (including experimental) work on individuals' economic behavior, and likewise with respect to the influence of institutions. Some of these economists take time out to denounce supposed barriers to their activity mounted by 'neoclassical' or 'Walrasian' economists. ETCSM takes this economists' rhetoric more seriously than it does the corresponding rhetoric from anti-economic philosophers (though that rhetoric is represented in my consideration of Dupré's views, which Walliser and Mantzavinos

find uninteresting). I spend the better part of three chapters asking: what *is* this 'neoclassicism' that is putatively choking off good science? In the end I conclude that it could only be either (i) competitive economics that isn't allowed, for some reason, to be integrated with game theory (including evolutionary game theory); or (ii) applications of revealed preference theory that aren't allowed, for some reason, to be unbolted from expected utility theory in modeling behavioral choices by individual subjects.

I don't know of any economist who practices 'research program' (i). Contrary to Walliser's charge, I don't claim that market theory and game theory 'hold the same assumptions and adopt the same methods', but I certainly do assume that they employ *non-rival* methods. Game theory is what you use to study oligopolistic markets, but as the number of competitors in a market gets large, then, barring pervasive informational asymmetries, the set of Nash equilibria will converge to Bertrand or Cournot, depending on your model. 'Research program' (ii) has a *bit* more reality. I know of few economists who now insist that, in modeling individual behavior, it must be the EUT way or the highway, but Walliser is right that a few important researchers, such as Kahneman, depart from the revealed preference approach in supposing that it makes sense to probe subjective intensities of utility by asking people to introspect. I indeed think that the history of cognitive science suggests that this is inadvisable methodology, and I devote substantial space in ETCSM to explaining why. Walliser doesn't comment on these arguments.

The main point, in any case, is that ETCSM does not purport to be a defense of neoclassicism against rivals. The view it defends is that anti-neoclassicism is largely empty rhetoric. The mainstream tradition in microeconomic theory both can, should and does make easy room for the wonderful advances in knowledge now flowing from experimental economics and neuroeconomics. Yet many – including, it seems Walliser and Mantzavinos – find the empty rhetoric persuasive. In declaring the rhetoric to be empty, I must surely be held responsible for explaining why so many economists indulge it. Quite a lot of ETCSM is therefore devoted, implicitly, to this explanation. I'm sorry that all three critics here found this to different degrees boring. But it isn't my fault that the task arises.

The rhetoric is partly to be explained by straightforward political economy. Promoters of new research ideas have obvious reasons, based on the scarcity of research funds and Nobel prizes, for suggesting that followers of more familiar paths are fuddy-duddies. But it would be scandalously uncharitable to advance this as the only, or even the main, account of rhetoric that's so widespread. ETCSM therefore offers a supplementary analysis. It hasn't occurred to most economic theorists that there is *no reason* why the paradigmatic model of an economic agent need map directly on to that of a biologically continuous individual human organism. This oversight leads many theorists to emphasize aspects of the mainstream tradition, in particular models of individual decision making, as if they were central to it when, in fact, they are optional. They are optional because, as Zak says, evolutionary cognitive science shows how to replace them without doing serious damage to the foundations of that mainstream tradition. It is *because* this is so that, I claim, one can study relationships between games among biological units and games among culturally sculpted people using 'standard analysis' (to quote from Mantzavinos's statement of his 'crucial objection'). The key insight, I argue, is distinguishing between these types of games in the first place. A main burden of ETCSM is showing

how confused one is likely to become about theoretical foundations otherwise. Mantzavinos, in taking for granted that the distinction could only be justified if its analysis called for non-standard solution techniques, illustrates the point. Anything new to which we turn our attention, he seems to think, must automatically carry us away from 'neoclassicism'.

I might be accused of arrogance for supposing that I've put my finger on an oversight that has befuddled most economists (though notably not Thomas Schelling). In response to such a worry, let me remind readers that the thesis that instances of *H. sapiens* organisms are not coextensive with people, and that neither are coextensive with prototypical economic agents, is a *metaphysical* thesis – by which I mean only that it is a thesis about what one is forced to say if one wants to be able to hold on to insights from all the several behavioral sciences while purporting to describe and explain one unified world. (Thus the Friedmanesque pragmatist can indeed ignore the point, as Zak implicitly suggests.) Economists, as the critics all agree, have more important things to do than metaphysics. Fair enough. I don't insist – how could I? – that they ever engage in or with it at all. Unfortunately, there are at least as many economists who indulge in sloppy philosophy as there are philosophers who diagnose the fundamental flaws of imaginary versions of economics.

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