

Reply to Lagueux: on a revolution in methodology of economics

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I thank Maurice Lagueux for his thoughtful reflections on my book *Economic theory and cognitive science: microexplanation* (henceforth: *ETCS:M*). Given space restrictions, in this comment I won't say much about his criticisms of my use of the intentional stance, except to observe that they seem to rely on equivocation between thinking of beliefs and desires as internal states of people, and as descriptions of relationships between patterns in their behaviour and external circumstances. I think that one must *completely* reject all traces of the former view if one hopes to avoid the sorts of logical conundrums that Lagueux worries about; and I find various passages in his arguments to suggest that he has not. The literature arguing for radical externalism about propositional attitude states is now vast, and I can do no better than refer readers to it.¹ I am surprised that Lagueux still finds objections based on bafflement about recursion—that to be an intentional agent requires that the intentional stance be already possible—partly persuasive. Intentionality comes in a continuum of degrees of sophistication, and has historically expanded its scope incrementally, just like most biological phenomena including life itself.²

As for Lagueux's fear that my criterion for ascribing reality to a pattern is "a bit ad hoc", I refer the interested reader to Ladyman and Ross (2007), where this difficult issue is considered with the care and large freight of relevant evidence that it demands.

¹ See especially Keijzer 2001; Morton 2002; Millar 2004; and Melser 2004.

² Consider in this context Dennett's *reductio*: Every mammal must have a mammal for a mother. There was a time before there were any mammals. Therefore, there are no mammals.

Here, I will concentrate instead on Lagueux's closing concern, which, based on inference from his title, is seen by him as his main theme. He puts it as:

the question of whether this valuable program of research [i.e., explaining the psychological and neural influences on economic behaviour] should be substituted for the more traditional economic programme in such a way that the whole methodology of economics, including the very definition of economics, be radically transformed (Lagueux 2008, 51).

He frames the matter this way despite acknowledging that I do not “explicitly pretend [...] to promote a revolutionary way to understand economics” (Lagueux 2008, 52). This is an understatement. The first two pages of *ETCS:M* include the following words: “it is not the aim of this book to try to tell economists they should go about their business in a fundamentally different way than they do [...] I don't want to advertise myself as promoting—heaven forbid—yet another ‘paradigm shift’”.

In contrast to Lagueux, I take my book to give more comfort to methodological conservatives than to methodological revolutionaries. I can summarize my interpretation of its core thesis as follows. Most economists have tied at least one hand behind their collective back with respect to answering critics of their standard analytical and empirical methods as a result of philosophical commitment to methodological individualism (MI). That it impedes their capacity for self-defense provides one motivation for dropping this commitment. A more general motivation is that all scientists ought, in general, to steer clear of philosophical commitments that are other than banal, and MI is not banal.

Lagueux claims that MI is, in fact, either anodyne or a straw man. I do not agree. The following description of MI is compatible with Lagueux's stated reason for thinking it important to economics: according to MI, the basic unit of economic explanation—basic in the sense that explanations averting to properties of this unit are templates for complete explanations so far as economics is concerned—is a (spatially and temporally) whole, normal, human person. Thus, as Lagueux says, when we find communities of these units jointly frustrated in their consumption and production ambitions, the economist sets out to explain this as resulting from the interaction of

the incentives and constraints that impinge on them as whole individuals.

Recently, a perspective that seems very close to the one Lagueux invokes has been expressed in an uncompromising way by Faruk Gul and Wolfgang Pesendorfer (2008). They argue that there is a fact of the matter about the proper domain of economics: its subject matter is (constrained) *choice*, by whole humans. (Note that I add ‘constrained’ because an economist would not be concerned, according to their view, with a person choosing to think about purple flowers instead of blue ones. But an economist might well be interested in a person choosing to *pick* purple rather than blue flowers.) Gul and Pesendorfer do not say what they mean philosophically by choice, though they clearly think that classical decision theory has effectively axiomatized it, and to that extent identified it. It seems also fair to attribute to them the idea that choice is some kind of computation of relative costs and benefits, under guidance of a prior notion of what constitutes a solution (e.g., maximizing a utility function or identifying a Nash equilibrium strategy). The crucial polemical claim of their essay is that the economist is professionally interested in *what* people compute as economic agents but not in *how* they compute. On this basis, Gul and Pesendorfer conclude that neuroeconomics is misbegotten, on grounds that discoveries of neuroscience are in principle irrelevant to economics. The same point applies, for the same reasons, to psychology.

As attested by most of the other papers in the volume their paper leads off, Gul and Pesendorfer’s perspective is highly controversial among economists. That alone suffices to show that Lagueux is hasty in depicting me as an isolated would-be prophet of change standing well ahead of the methodological herd. In fact, I am rather *more* sympathetic to Gul and Pesendorfer’s position than are *most* of the eminent, and often incontrovertibly mainstream, economists who have objected to it. In *ETCS:M*, I too defend the idea of economics as a science concerned with abstract optimization under scarcity. Ultimately, however, I reject Gul and Pesendorfer’s extreme separateness thesis for two reasons. First, the sociology of science is such that its institutions do not tolerate completely isolated disciplines. Total abandonment of interest in unity is treated as a symptom of quackery. Second, economists very frequently cannot achieve their ideal of describing phenomena by means of elegant reduced-form models that uniquely estimate quantitative values of dependent variables. Especially with the fall in the price of

computation over the past three decades, large structural models that require a great deal of epistemically risky econometrics have become the norm, not the exception, in economics (Harrison 2008; Humphreys forthcoming). Thus economists *do*—constantly—put forward hypotheses that are partly about the ‘how’ as well as the ‘what’ of economic computation. In light of this, why should there be a special ban on independent variables that range over neural or psychological properties, where these help to constrain estimations and improve the fit of models?

Once one gets this far, one encounters a vicious undertow if one attempts to cling to MI. The problem is simply that, at the level of both the brain and the whole person—which, as I argue in *ETCS:M*, are *not* the same thing—the computational processes by which choices over whole-person-scale alternatives are computed turn out, empirically, to be more like games than like parametric optimization exercises. One should therefore drop the assumption that the whole person is the basic unit of explanation. (This does not mean that one should seek some *other* such unit; one should just stop restricting oneself with such extra-empirical metaphysics altogether.) Then, I argue in the book, it turns out that nothing that ever should have mattered to economists is sacrificed anyway.³ The Samuelsonian method defended by arch-conservatives such as Gul and Pesendorfer applies at least as well to sub-personal agents as it ever did to people. It doesn’t apply anywhere without mediation through models; *no* material entity is literally and only an economic agent. But many highly useful sciences are about virtual entities.

Lagueux exactly echoes Gul and Pesendorfer when he says, in criticism of me, that:

a well-grounded analysis of the way these sub-personal agents are more or less coordinated and related to the whole person might be a great triumph for behavioural psychology and for neurobiology, but not for economics as such, even if neurobiologists use RPT or other economic tools (Lagueux 2008, 48).

³ A good deal of the forthcoming third volume of *Economic theory and cognitive science* is devoted to showing that included in the set of commitments not lost with, but indeed strengthened without, MI is *normative* individualism. A crucial basis for MI in economics has been a mistaken impression that it is essential for standard welfare analysis

To this I respond that insofar as one uses economic tools one is to that extent doing economics, just as, when one applies the quantum formalism to (say) computers one is to that extent doing physics. I reject the thesis, common among philosophers, that the division of labour among the sciences is derived from a set of deep ontological ‘joints’ at which we try to carve nature. Aspects of the behaviour of many types of systems—commitment to which *as* systems is provisional and *also* not based on metaphysics—is such that they will do things we can *only* predict and explain if we model them as optimizing utility or production functions or playing games. That is why there is a thriving science of economics that should be continuously open to enrichment, but should indeed be spared the violence and dislocation of revolution.

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