Getting Economics Exactly Backwards

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Consider four statements aimed at summary identification of the class of phenomena studied by economists:

(1) Economics studies human behavior as a relationship between ends and scarce means which have alternative uses.
(2) Economics studies rational choices of agents, and the ways in which these choices statically and intertemporally interact with one another.
(3) Economics studies the statistical regularities in relationships between changes in incentives and changes in behavioral responses.
(4) Economics studies the comparative efficiency of alternative patterns of resource allocation in networked groups of agents ≥ 1.

Statement (1) is of course Lionel Robbins's (1935) famous "definition". Statement (2) is an attempt to boil down a loosely Hayekian view, particularly as explicated by Maurice Lagueux in his recent (2010) book. Statement (3) is my own favorite short description. Statement (4) expresses the view that economics is a social science with an engineering bias—that is to say, a policy science.

I think that *all* of these statements are reasonable short descriptions, in the sense of being compatible with the fuzzy criteria by which economists make judgments about the limiting characteristics of their professional activity. I am doubtful that a convincing argument could be made for pronouncing any one of them clearly superior to the others across all possible contexts of explication. There is no sound reason for insisting that the scope of a discipline should have a preferred analysis in the philosopher's sense. Disciplines are institutions,

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and like all institutions are individuated in their working dynamics by resort to analogies and rough practical templates. They can and do evolve and adapt flexibly over time *because* they are not restricted by any rigorous logical semantics.

It is more fruitful to reflect on what the statements have in common. All are vague at approximately the same point in conceptual space. Statement (1) refers to "ends" and "means". This clearly alludes to the existence of entities with respect to which such a distinction is meaningful—people, at least, perhaps firms and households and perhaps bears and salamanders, but not rocks or chairs. Statement (2) is heavily freighted with highly abstract concepts: rationality, choice and agency. Statement (3) arguably hides reference to all three of these notions inside a black box called "incentives". Statement (4) re-labels almost the same black box under its normative aspect by the appeal to "efficiency". All of the statements, then, leave limitless happy hunting grounds for analytic philosophers.

Such philosophers should not expect economists to provide them with much help. All of the fine distinctions the philosophers are likely to produce will be refinements of an intuitive one that normally gives economists no practical pause, that between agent behavior and mere physical motion. A person stands on a ledge, agonizing over whether to jump or try to run through the fire inside the building; and the economist is professionally interested. Then the person jumps, and as he falls, he pulls his limbs protectively around his torso; the economist is no longer professionally interested. As Lagueux stresses (114), economists study *choices*, not *reflexes*.

Recently, however, this basis for typically easy judgment has been brought under pressure. Suppose a neuroscientist could predict what the person on the ledge will do merely on the basis of observing neuroelectrical and neurochemical activity. Suppose furthermore that this predictive capacity rests upon the fact that the neuroscientist has a general theory according to which the brain maximizes something that structurally resembles, but isn't quite identical to, utility. This is just the explanatory leverage that, according to Paul Glimcher (2010), neuroeconomists are on the verge of gaining. If Glimcher turns out to be right, should economists begin studying neurons? If so, would they thereby replace the study of choices by the study of mechanisms?

This question matters enough to two mainstream economists, Gul and Pesendorfer (2008), to have inspired them to forcefully urge economists away from the temptations of neuroscience, and indeed from psychology generally. Many economists agree with them; others emphatically do not. Economists care about this methodological dispute because it has practical consequences for curriculum planners, hiring committees, journal editors, granting agencies, and other agents of institutional maintenance and reproduction. Yet this dispute, unlike most in which economists engage, seems to be philosophical in character, since it directly turns on views about the general ontology and conceptual target of the discipline. Thus Lagueux's book has more relevance for actual economics than most economists who pick it up are likely to think.

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Lagueux believes that Gul and Pesendorfer's assertion about the proper domain of economics has a sound philosophical basis. However, the basis in question for Lagueux is the opposite of the one that Gul and Pesendorfer, as orthodox revealed preference theorists, suggest. Suppose for purposes of argument that Glimcher's hypothesis is at least roughly right.[1] In that case, according to Lagueux, the view that we might regard groups of neurons as economic agents, and indeed as more straightforward economic agents than whole people, would follow from the identification of economic agency with consistent maximization of a computed magnitude.[2] But this, he maintains, is a reductio on the identification of rational economic agency with consistent optimization. Since such identification is, as Lagueux points out, the core theoretical innovation of marginalist neoclassicism, he thinks that the reductio directs us to an alternative philosophy economics.

The alternative philosophy he favours is broadly Austrian. His core thesis is that economists should best rely on a traditional or folk conception of rationality instead of replacing it with the neoclassicist's technical concept of constrained consistent optimization. Most of Lagueux's book consists in giving arguments for this preference. They broadly boil down to two master contentions:

(1) Most people are rational in the folk sense. That is, they are not *stupid*: if the best means to their intended ends are not hidden from normal perceptual discernment and don't require long calculations, and aren't rendered too counterintuitive by exotic strategic dynamics, they will adopt the means in question.
(2) People do not even approximate consistent optimization. This is because their tastes are in constant adaptation under pressures of new learning and normal life-cycle development, and because optimization involves not merely long but *limitless* computation. Consistent optimization, Lagueux argues, is not an idealization of folk rationality; it is something else altogether. Thus one does not best approach human rationality by beginning with consistent optimization and then relaxing its strictures so as to yield "bounded rationality". One should start with actual, everyday rationality in the first place.

These are very plausible claims about normal people navigating the parts of their lives that involve explicit individual deliberation. A paradigm instance might be a shopper in a store choosing between two near substitute products. She might think about comparative brand reliability, aesthetics and price. If the expenditure is a significant proportion of her income or wealth she will probably explicitly consider financial risk relative to expected return; otherwise she likely won't.

But is this kind of scenario a good representative instance of the kind of phenomena economists study or should study? Lagueux recognizes that economists are more often interested in aggregate-scale effects, such as changes in

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posted prices or quantities demanded in whole markets, than in decisions of individuals. But he defends methodological individualism, which he takes (following Hayek) to be the thesis that such effects should be analyzed as unintended consequences of the interaction of intentionally chosen actions by individuals. In cases where the number of interacting agents is too large for any one of them to influence the outcome, we thereby license a loosely Marshallian form of central tendency analysis. In interactions among small groups we will need to use game theory, though because our underlying idea of rational play is the folk one instead of the technical one of von Neumann and Morgenstern, on Lagueux's picture we would generally need to have recourse to "refined" solution concepts of the kind discussed most extensively by Kreps (1990).

Lagueux does us all an excellent service by clearly spelling out the consequences for practical economics of this core Hayekian logic. By "core" I allude to the fact that it is stripped of various more specific Austrian doctrines, including ideological ones, with which it is typically encumbered in the context of arguments over policy. Most usefully, Lageux allows us to see clearly and explain quite generally why Austrian economics is incompatible with the larger context of the social and behavioral sciences in which the (capacious) niche for economics is embedded.

Suppose we begin by agreeing with Lagueux that economics is about chosen behavior. This is exactly the assumption shared by all of the summary statements with which I opened, and is why they are all philosophically inexact in roughly the same way. It then follows that economics concerns what philosophers call "intentional" phenomena. For this reason, Lagueux sees that secure philosophical foundations for the sort of economic methodology he defends must incorporate an account of intentionality. But more specifically, he thinks they must rely on the *folk* view of intentional states—as expressed in propositional attitudes such as "believes that *p*" and "desires to *q*"—according that which they refer to the input and output states of internal rational deliberations of individual people. It is then a very serious problem for Lagueux's philosophy of economics that this theory of intentionality, which forty years ago was the majority view among cognitive scientists, is now held by only a vanishing minority of them.

The contemporary majority opinion is that a small proportion of a typical person's beliefs are explicitly internally represented, by means of a very specific kind of mechanism. Suppose you wonder who I believe to have been the best jazz vocalist of all time. One way in which you might try to find out is by asking me. This would work, as it happens, because we share a network of semantic coordination history that allows me to cause you to represent a specific deceased person when I pronounce the words "Sarah Vaughan". You thus come to identify my belief about the pecking order among jazz vocalists. Notice that this imagined but quite ordinary episode could describe either of two possible kinds of psychological process on my side. Perhaps, on the one hand, I have never previously been asked the question in explicit natural language,

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including by myself. When you pose it, a storm of distributed activity occurs throughout my brain that yields a linguistic output that my auditory system picks up just as yours does. This "storm of activity" is a complex review of memories of non-linguistically coded implicit evaluative responses that converge on the linguistic report I issue in answer to your probe. I identify my opinion in the indexing system for beliefs that shared aspects of our cultural histories more or less obliges me to use. On the other hand, perhaps I remember having once asked myself, literally, in English, "Who was the best jazz vocalist?" and having thereby triggered my own auditory neurons to represent the pattern characteristic of hearing the phrase "Sarah Vaughan" as said by someone else. People routinely borrow the social indexing system of their linguistic community in wondering about their own beliefs—in the everyday locution for such episodes, they "talk to themselves". (An intermediate case is the one in which a third party previously asked me who I regard as the best jazz vocalist, and I explicitly represent my memory of *that* episode.)

Some philosophers defend the thesis that, in a case such as the one described, I didn't acquire my belief about Sarah Vaughan's status until the first linguistically mediated internal representation was produced. But this is distinctly *not* the semantic presumption of any part of current scientific psychology or, more broadly, cognitive science, which traffics routinely in implicit beliefs and desires. There is an overwhelming practical basis for this: most cognitively mediated human behavior issues from neural processing that does not loop through explicit linguistically coded self-representation. Cognitive scientists *could* respect a convention according to which we reserved "belief" and other intentional state tags for products of explicit linguistically coded episodes. But this would have the consequence of leading us to deny that the great majority of behaviors that are chosen in the economist's—and, inconsistently, the folk's—sense of being sensitive to changes in incentives are intentional. This would cut against the grain of virtually all social science, including economics.

One consequence of this general practice of cognitive scientists is that intentional states are ultimately individuated by patterns of observable behavior, and not exclusively by reference to postulated histories of individual internal representation. People attribute intentional states to themselves to rationalize their own behavioral patterns, just as they do to others. People sometimes identify intentional states by reviewing their memories of explicit linguistic self-representations, but to conclude on this basis that an intentional state is a kind of internal state confuses an epistemic pathway with its object. Internalism about intentional states seems persuasive to many people only because in moments of philosophical abstraction, removed from the normal contexts of social interaction that intentionality evolved to facilitate, the very entertainment of questions about state identification always triggers processing in the medium of public language.

Lagueux is very clearly in the grip of internalism about intentional states. Late in his book (e.g, 241-244), he explicitly denies that non-human animals or

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computers or groups of neurons make genuine economic or strategic choices, or could in principle truly be said to optimize, simply and only because they do not engage in self-conscious deliberation. This is notwithstanding the fact that to understand why (for example) a population of animals harvests one food source rather than another, or shows certain systematic patterns of mate preference, or favours one migratory route over another, we deploy *exactly* the kind of analysis and use *exactly* the underlying conceptual apparatus that economists apply to groups of human consumers and producers. Glimcher (2010) defends the value of neuroeconomic analysis on the grounds that identifiable neurons in his lab monkeys measurably compute outcomes that closely track the optimization of the monkeys' expected utility. Lagueux (243-244) denies that this is real "choice" because it involves no self-aware deliberation; and therefore he denies that it is economics, because economics is about choice.

Choice as it is understood in the overwhelming majority of economic analyses is emphatically *not* taken as equivalent to explicit, self-conscious deliberation. Most of the incentives to which most people respond most of the time are *never* represented by them in discrete episodes of linguistic or quasi-linguistic representation. The vast preponderance of human behavior that is "chosen" in the sense of being sensitive to shifts in incentives is governed by conditioned responses of neural mechanisms to which people have no access except through observing their own behavior in the same way that another agent might observe it. If the "true" domain of economic choice were restricted to explicit conscious deliberation then most actual economic analyses would have to be found to rest on confusion.

It is in states of self-conscious reflection that a person is most emphatically alone, managing his own thought instead of, as in more social contexts, giving the environment preponderance of control. Socially governed thought is developmentally primitive here, while solitary auto-stimulation with natural language is the derivative innovation. We confuse the exotic application with the basic technology.

Returning in light of this to the four statements with which I opened, we can see that statement (2) stands out from the others in a key respect: it implies that economics begins with agents and then aggregates them. Lagueux (76-77) recognizes that economic analysis doesn't construct societies out of individual atoms in the way that introductory pedagogy in microeconomics textbooks typically suggests. But if, as he maintains, the only real intentionality arises in individual deliberation, and if economics is about intentional phenomena, then economics is at bottom a study of the consequences of the thought of individual people.

I think this gets economics as wrong as can be, *despite* my agreeing with Lagueux—and Gul and Pesendorfer—that it is about choices, and not about mechanistic causal relationships.[3] Economics is about statistical relationships between changes in incentive spaces and population-level responses

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to these changes. It recognizes heterogeneity within populations not by analyzing individuals but by fitting models with enough structure for the aggregate effects of such heterogeneity to be measured. (See Andersen et al., 2008 for an example.) It relies on technical concepts of consistency and optimization not because economists imagine that people typically actually optimize anything, but because statistical modeling requires functions that converge in the limit. Lagueux is right that these concepts have little to do with rationality in the folk sense; the implication of this, as Pareto recognized a century ago and Samuelson followed through on, is that folk norms of rationality aren't relevant to economics. Lagueux's main conclusion is exactly upside-down. One place this shows is in his implied preferred understanding of the application of game theory to economics; as Binmore (1994) argues, the refinement program is precisely what we should *not* follow.

I am grateful to Lagueux for making clear to me the deepest errors of the Austrian style of economic analysis. Austrian economics is not social science but a kind of social *philosophy*. That is to say, it is a projection of a certain idealized human self-image emphasizing some distinctive elements of enlightenment mythology, specifically the sovereignty of the individual reflective self. This is as powerful a normative organizing principle as can be for the multitudes of us who have shaped our lives according to it. But it is no basis at all for the scientific study of economic dynamics.[4,5]

Notes

1 I am doubtful that it is, for reasons given in Ross (2012).

2 This argument has been made previously, as Lagueux acknowledges, by Berns (2003) and Ross (2005).

3 This doesn't imply rejection of neuroeconomics in principle. Unlike Lagueux, or Gul and Pesendorfer, I'm happy to interpret neural networks and sub-networks as choosers.

4 Lagueux (175) is skeptical about my (Ross 2005) proposed non-humanistic interpretation of the domain of economics *not* because it asks economists to engage in any kind of technical analysis they aren't now doing, but because it rests upon a counter-intuitive philosophy of personal agency. The intuitions to which it is counter are those of the folk. I plead guilty as charged here: folk concepts are reliably useless tools for science (Ladyman and Ross 2007).

5 The reader may find Maurice Lagueux's reply to this critical notice at the end of this Book Symposium. See section II of «Reply to my Reviewers/Réponse à mes commentateurs».

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