

Sergio Da Silva's response to the reviewers' comments

First, thank you very much for the comments. I found them genuinely useful.

Response to the Referee

Referee:

Here are my comments and thoughts on Da Silva's paper. Overall I quite liked it, although I had one major disagreement.

- Argument 1 could be clearer: There seem to be two points:

(i) Lucas critique is not significant empirically.

(ii) Since there is "no theoretical model that can a priori prevent policy-parameter instability from occurring" there are "no theoretical implications whatsoever" of the Lucas critique.

I think the reader would be helped by a better exposition of point (ii). What property of macroeconomic models implies that they cannot, in theory, prevent policy-parameter instability from occurring?

Answering that question would make the argument clearer.

Response:

Actually, point (ii) follows from what we can learn from the empirical tests of the Lucas critique. The critique does not address desirable theoretical properties, such as micro-foundations. To make this point clearer, I have rewritten the last paragraph in the section Argument 1.

Referee:

- Argument 2 could also be clearer, but perhaps this is due to my lack of knowledge regarding the debate.

Response:

I changed this argument slightly, and I replaced "subjective rate of time preference" with the more specific "subjective marginal rate of substitution between present and future consumption," as suggested by one reader (M.L.).

Referee:

- Argument 4: I had some agreements and disagreements here. First, I agree that rational choice theory is not a good model of the psychology of human choice. But second, I strongly disagree that neurobiology has much to tell us about human choice.

Response:

I said that a novel choice theory can be built inspired by neurobiology. I did not say that neurobiology has a theory of choice. Additionally, I think that neurobiology has much to reveal about human and animal choice.

Referee:

The new imaging techniques will not provide the scientific results that are hoped for. There is a simple reason: there is a very complex mapping between intentional states and neural activity. It is very much like the relationship between software and hardware. For example, we could image the circuit-level electronic activity on a desktop CPU. But can we deduce, from this information, whether that CPU is running a Word Processor or a Spreadsheet? The simple answer is "no". Software is not reducible to the CPU hardware. Similarly, cognitive states, such as beliefs, desires, emotions etc., will not have unique neural correlates.

Response:

The software-hardware metaphor above is related to the fact that brain imaging provides only indirect associative evidence. However, there are other techniques that can bypass this difficulty. The Transcranial Magnetic Stimulus technique allows one to make causal inferences about the workings of the brain. TMS can switch off particular regions while leaving others on, and one can infer which region controls which neural function.

It should not be implied that fMRI is useless. Quite to the contrary, producing better and better associative evidence based on fMRI is part of the day-to-day work of neuroscientists. I think that it is inappropriate to dismiss the entire field of neuroscience on the grounds that current techniques are limited.

Referee:

But the irreducibility of different “levels of reality” is quite ubiquitous in science, and it is one of the reasons why there are distinct fields of inquiry, with their own laws. Biology is not entirely reducible to chemistry, and chemistry is not entirely reducible to physics.

Response:

I agree, and that is why I am against making macroeconomics reducible to microeconomics.

Referee:

Similarly, human decision-making is not reducible to neurobiology, which is why there are distinct fields of neurobiology and cognitive science. So I think any project to understand human choice behavior in terms of neurobiology is misguided.

Response:

Again, as observed above, I stated that choice theory can be rebuilt to take account of the insights provided by neuroeconomics. I did not say that neurobiology has a theory of choice.

Referee:

An entirely different argument undercuts axiomatic choice theory: the computational requirements required to make decisions surpass what the human mind can reasonably perform.

Response:

I agree, but I think that this only reinforces the point that rationality is a borderline situation.

Referee:

It is true, however, that neurobiology has plenty of contributions to make regarding the gross functional structures of the human brain, in particular how different parts of the brain take “short cuts” and use “rules of thumb” associated with the emotional part of our brains, which do not conform to the strict, time-independent idealizations of rational choice theory.

Response:

I agree.

Referee:

However, statements along the lines of: “Neuroscience has now made it possible to measure utility objectively” are nonsense. They should be removed!

Response:

This sentence was taken from the discussion in the Introduction of the survey by Camerer et al (2005). There is a similar statement in the review by Platt and Huettel (2008), and it is a central claim. I have added these references after that sentence in the revised version of the paper instead of removing it. Measuring utility sounds too good to be true for all of us trained in the neo-Bernoullian paradigm of ordinal utility. Leonard Savage’s comment that “cardinal utility is a myth” echoes in our heads. However, please see the discussion in the surveys. For another example complementing those in the surveys, see Allison N. McCoy, & Michael L. Platt (2005) *Nature Neuroscience* 8, 1220-1229. All the statements in the piece were taken from elsewhere in literature because I was solely documenting the already existing literature and presenting my case neatly for the audience of economists. However, I should concede that I also adopted a “forceful” style, as an anonymous reader observed.

Referee:

- Argument 5: I like this section overall.

In conclusion, my main problem with the paper is that it recommends deepening microfoundations by finding a better theory of individual economic behavior in the realm of neurobiology. This seems entirely wrong to me. The other part of the paper -- which recommends statistical approaches based on large ensembles of economic actors -- is better, but seems to imply, for macroeconomics at least, that we can entirely jettison rational choice microfoundations. In which case, there is little or no motivation for economists to start interfacing with neurobiology. Just like biology is relatively autonomous from chemistry, I think macroeconomics is relatively autonomous from individual psychology. So I would find more agreement with the paper if it made this point, and did not recommend deepening microfoundations.

Response:

I am afraid I strongly disagree that statistical approaches for macroeconomics imply that we can entirely jettison value theory. As the referee observed earlier, irreducibility of different levels of reality in science is ubiquitous. There is micro and there is macroeconomics. Even a statistical macroeconomics cannot jettison microeconomics. As written in the paper, "here, there is no contradiction between individual freedom of choice and the existence of physical laws for the economy as a whole. Patterns can exist at the level of many people, even while individuals continue to make free choices. But choices are constrained by what the group does."

Response to the Anonymous Reader

Reader:

My take on the Lucas Critique is that it could still apply if macroeconomics had other (possibly neurophysical) foundations. For me, what it says is that what agents do when government does (or is thought to do) x can't tell us how they'd act if were known to do y - and importantly that impressions gleaned from the former are silent on whether y might be a superior set of policies to x . I believe this is true, and holds whatever story one has about private sector behaviour, except those that hypothesize, unreasonably, that government's actions are completely neutral. If your actions were influenced by the pollen count, or by the position of Jupiter in the sky, and not by traditional optimizing, that wouldn't preclude the possibility that your awareness of what government is doing mightn't matter too.

Response:

I agree. As argued in the piece, there is no model that can escape a priori from the Lucas critique. Whether a model is subject to the critique has to be decided on empirical grounds.

Reader:

Then there is the standard philosophical sceptic's reply to any assertions that observable brain activity describes mental states and processes: how do you know that? You can't possibly prove that! So da Silva's preferred alternative to traditional micro optimizing is going to be a hard thing to prove. A related point is that an optimizing approach is almost impossible to argue against, as "utility" can encompass externalities, decision taking and information-gathering costs, imitation and much else (notice for example the empirical success of the external habits term in micro-based consumption models).

Response:

I think that whether observable brain activity describes mental states and processes can be settled scientifically by neuroscience, away from philosophy. However, there is still a remaining, valid philosophical problem related to the neurobiological alternative to optimizing. It is precisely the Aristotelian's decision problem: either live in ignorance (accompanied by illusory bliss) or lead "the examined life." Translation: it is the choice between what you believe you know and the promise of an unknown "real" truth. Amusingly, the movie *The Matrix* provides an example. The character Morpheus offers the character Neo two pills. The red pill will answer the

question “what is the Matrix,” and remove him from it. The blue pill will maintain the status quo – life that will carry on as before. The red pill is just a promise that it can help us to find the truth. It thus involves risk and doubt. If the truth is unknown, or you believe that you know the truth, why risk the status quo? Choosing the blue pill is much easier. Given the potential disadvantages of choosing the red pill, the motivations for discovering the truth must be very strong. The reader made his (her) choice: the blue pill. My paper reports promising avenues of research toward what I believe to be real scientific progress. Thus, I choose the red pill. One circumstance favors my choice—that we all are wired to enquire.

As for the argument that the optimizing approach is almost impossible to argue against, as “utility” can encompass “external,” objective characteristics, I agree with the reader, it is almost impossible to argue against. However, the notion that people optimize in all circumstances still needs to be justified on scientific grounds; as a mere hypothesis, it remains in the realm of metaphysics.

Response to Michael Lovell

Michael Lovell also made useful suggestions that were incorporated in the revised version. However, Mike is uncomfortable with the mention to the Say’s law. Because this can possibly be the attitude of many readers, I reproduce his comment here.

Mike:

“Say’s Law” - This concept has manifest interpretations, sometimes being confused with Walras’ Law and sometimes with the proposition that there can never be a universal glut. The latter seems to be contradicted by the current state of the US economy. Tom Sowell argues that what Say had to say is usually misinterpreted. I checked two intro and two macro textbooks; only the intro texts mentioned Say’s Law.

Response:

I would rather keep Tom Sowell’s interpretation, which I think settles the issue (see also the entry “Say’s law” in the New Palgrave Dictionary). Ben Bernanke’s “global savings glut” has nothing to do with Say’s law. This central principle of macroeconomics is unfortunately unnoticed in modern textbooks. Mankiw’s intro textbook, for example, did not mention it, despite the fact that the principle is surreptitiously in there (for example, in the model of the circular flow in Chapter 2). Rather, the author provides Ten Principles that are a little bit idiosyncratic and thus vulnerable to humorous criticism (see http://adam.vwl.uni-mannheim.de/fileadmin/user_upload/adam/Makro_B/mankiw.pdf)

Response to Peter Sinclair

Peter Sinclair made detailed comments that were incorporated in the revised version. There is one comment in particular that I reproduce here.

Pete:

Wouldn’t prospect theory draw a big distinction between individuals buying (price rise is bad for utility) and selling (good)? (Some individuals sell goods and many derive their livelihoods from factors specific to certain goods). Is it a general price rise that makes you feel especially miserable and hence less inclined to buy any particular good? Or does that depend on money income or nominal assets failing to rise in proportion? Might be worth sketching out exactly how prospect theory affects demand responses to prices, and why and when. Too quick at this point!

Response:

That is thought-provoking, but I would rather not extend this discussion in this paper. Instead, I would like to incite more discussion in another direction for those Keynesians still unconvinced that macro does not need micro foundations. They could also have taken Allais's theory for their micro foundations. Allais' theory is another, though less well known, alternative to expected utility theory (see for example the entry "Allais paradox" in the New Palgrave Dictionary written by Maurice Allais himself). Allais's theory is in agreement with a notion that I think is also Keynesian: the non-identity of monetary and psychological values.