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What are we up to?

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ABSTRACT

Even though one of the founding aspirations of our field was to foster mutually beneficial exchanges between economics and philosophy, economists never paid much attention to our work. Now that practicing economists are increasingly engaging in explicit methodological reflections, we can no longer comfort ourselves with the idea that economists are simply not interested in discussions of methodological issues. What is new on our side of the fence is that some of us commend economists not to be distracted by the sort of philosophical ideas we discuss in our work. While no one openly opposes this, it is clear that others in our field strongly disagree with this. I argue that what is ultimately at stake are different views on what philosophy of economics is all about, what it should be and what it should do. I also argue that these fundamental disagreements are better discussed openly than being kept under the carpet.

KEYWORDS
Economic methodology; philosophy of economics; naturalism; foundationalism

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Introduction

In 2008, Oxford University Press launched a series of handbooks in economic methodology. The first handbook in the series was called 'The Foundations of Positive and Norma4ntive Economics' (Caplin & Schotter, 2008). A second volume in the Series appeared in 2015 under the name of 'Handbook of Experimental Economic Methodology' (Fréchette & Schotter, 2015). Anyone knowing that there exists a field of economic methodology within economics, credited with its own JEL code B4, might have expected that leading figures in the field would have been the main if not sole contributors to the volumes. Except from Hausman (2008), however, there are no contributions of specialized economic methodologists in the volumes. The contributions almost all come from top researchers in economics. What is more, reference to work done by specialized methodologists is almost completely lacking.[1]

Anyone only superficially acquainted with the history of economic methodology might be tempted to conclude there is really nothing new under the sun. Haven't practicing economists always disregarded work done in economic methodology (see, e.g. Frey, 2001)?[2] As Caplin and Schotter put it in their Series Introduction ' … methodological reflections are widely seen as fruitless' by economists (Frey, 2001, p. xiii). Isn't it time then to accept the persistent disinterest of practicing economists in our work as a fact of life we have to live with? In this short paper, I will argue that this is not a forced move for us.[3] Of course, we can decide to abandon the aspiration to convince economists of the relevance of our work. But we can also try harder to seize as yet unexploited opportunities to connect with the ' … increasing signs of methodological ferment' (Frey, 2001, p. xiii) in the economics profession. In order to

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do the latter, we should not only reach out more to economists. We should also be ready to no longer avoid internal debate over fundamental issues currently dividing us.

A bit of historical context

Let me first sketch the spirit in which the then nascent field of economic methodology was founded. The leading journals of the field, *Economics & Philosophy*, and *The Journal of Economic Methodology* (and, before that, *Methodus*), both wanted to serve as a platform for systematic exchanges between economics and philosophy. This left open which one was supposed to be the main beneficiary of the exchange, economics or philosophy. But it was clear right from the start that a leading idea was that economics could benefit from a closer engagement with philosophy. The almost unanimous way in which methodologists opposed critiques of methodology such as Weintraub (1989) and Hahn (1992) shows that the latter idea was very much at the heart of the new field. Without any exception, methodologists were convinced that methodology is relevant for economics and that practicing economists should not think otherwise. One recurrent line of reasoning was that making methodological decisions is both unavoidable in economics (and for any discipline, for that matter) and consequential for the sorts of theories that are constructed and accepted in it.[4] The general feeling was that conceding to Weintraub that methodology is irrelevant for economics would be tantamount to denying the very *raison d'être* of the new field. And against Hahn methodologists held that economics could only benefit from explicit methodological discussion. How could anyone seriously believe economics would be better off by leaving its methodological foundations unexamined?

There was a shared sense of several things. Let's unpack them a bit. One of the field's central goals was to encourage methodological reflection among economists. Mäki (1994) disagreed with almost everything Weintraub (1989) argued, but agreed that it would be good if economists developed more critical self-consciousness. To further this, philosophy (and more in particular, philosophy of science) was considered to be one of the main *sources* (if not the main or even only source) to draw on. Economists and philosophers who specialized in economic methodology at the time, and who also sometimes called themselves professed (or professional) methodologists (see, e.g. Hoover, 1995), predominantly looked at new insights in philosophy of science to shed light on methodological issues in economics. One such issue that got a lot of attention in the 1990s is how to appraise economic theories and models (and especially what are the proper standards to apply in such appraisals).[5] Professed methodologists saw it as one of their main tasks to enlighten economists about the latest developments in philosophy. Economists should know about the reasons that led philosophers to move beyond positivism ('post-positivism'; Caldwell, 1982) and Popperianism ('post-Popperianism'; De Marchi, 1992), for example, and about what implications these developments had for how to assess economic theories and scientific research programs in economics in the light of contravening empirical evidence. Backhouse's paraphrase of Keynes nicely captures the spirit of the times

Practical economists, who believe themselves to be quite exempt from any philosophical influences, are usually the slaves of some defunct methodologist. Crazy economists, who hear voices in the air, are distilling their frenzy from some methodological guru of a few years back (with apologies to Keynes, 1936/1973; p. 383) – Backhouse (2010), p. 3.

Professed methodologists, in short, saw themselves as *catalysts* of explicit methodological discussion in economics, as *mediators* between philosophy and economics, and sometimes also as *experts* about the (un)tenability of particular methodologies adhered to by economists (as witnessed by arguments to the extent that philosophical doctrines such as positivism, Popperianism, operationalism and instrumentalism that economists implicitly or explicitly endorsed are obsolete).

Especially the last part of this self-image might have amazed practicing economists. What made professed methodologists believe that they had a comparative advantage over practicing economists in telling what 'non-defunct' methodology best promotes the growth of economic knowledge? To professed methodologists, a 'non-defunct' methodology (such as post-positivist Lakatosianism) basically meant a methodology that is consistent with the latest insights in the philosophy of science. This might have been so self-evident to them that they saw no reason to explicitly state it. But to a practicing economist this might not have been obvious at all. Just being exposed to arguments that some methodology is superior on philosophical grounds was not nearly enough for them to abandon their allegedly defunct methodologies (such as operationalism and instrumentalism) as 'dead ends'. Hence responses by economists in the spirit of 'It takes a theory to beat a theory'. Economists typically ('qua economists') are not interested in the philosophical credentials of methodologies per se; they are only interested in methodological discussion in so far as methodological discussion is germane to their immediate concerns. If they believe their theories are in a good shape, they will see no need for methodological reflection ('If it ain't broke, don't fix it'). Furthermore, like recipes, methodologies that look good on the drawing board (or 'in theory') might disappoint when actually tried.[6] The proof of the pudding is in the eating, as the saying goes. One is reminded here of Fisher's (1933) and Samuelson's (1983) denunciations that professed methodologists typically have been unable to erect better theories on the basis of their own methodological advice. Professed methodologists might of course respond by arguing that the very point of their specialization is that they do not engage in theory development themselves. And in so far as economists believe there is nothing wrong with their theories, professed methodologists might be tempted to unmask this as circular complacency: small wonder that economists are satisfied with their theories if they use the same criteria for assessing the quality of their theories as they use in developing their theories. But absent evidence that their favored methodologies lead to better economic theories, to the typical economist the question remains what made professed methodologists so sure that their favored methodology is superior to the defunct methodologies they are supposed to supplant.[7]

What's new?

It is tempting to believe that not much has changed in how both practicing economists and professed methodologists look at the (ir)relevance of economic methodology since the 1990s. Indeed, economists have remained steadfast in disregarding and ignoring work done by professed methodologists. And professed methodologists still seem to cling to the belief that their work is relevant for economics. But appearances might be deceiving here. I will argue that underneath, mostly publicly undiscussed also by professed methodologists, several things have changed. I will briefly discuss a few of them.

Going against Hahn's advice, there is much more explicit discussion of methodological issues by practicing economists now than in the 1990s. These discussions do not pertain only to small m methodological issues, but also to more general and abstract issues, such as the epistemic status of models and their use in economics (see e.g. Rodrik, 2015 and Gilboa et al., 2014). It seems partly these discussions by economists have been spurred as a response to the financial crisis, partly they were also a response to methodological issues popping up from within the discipline (such as how reliable and generalizable findings in lab experiments are, as in Fréchette & Schotter, 2015).[8] Unfortunately, we cannot boast that the efforts and work done by professed methodologists contributed to this. There is evidence that these had almost no impact. With only a few exceptions, reference to the work done by professed methodologists is conspicuously lacking in these discussions. Occasionally there are direct references to general ideas from philosophy, but the work done by us, as self-proclaimed mediators between philosophy and economics, is mostly ignored.

For sure, professed methodologists have not ceased to engage also in explicit methodological discussion. Instead of having one discourse on economic methodology in the 1990s, with almost only professed methodologists as participants, it seems we now have two parallel discourses (i.e. discourses that do not connect with each other), one in which only professional methodologists partake, and one in which only economists partake. One could argue that the field of economic methodology is more productive and vibrant than ever. But on this side also several things seem to have changed. It is not clear anymore that contributing to more critical self-consciousness among economists is still seen as a central goal of our field. It seems that the intended audience of our work gradually shifted from practicing economists to philosophers (mainly to philosophers of science and philosophers of social science). Quite a few of us do not seem to care so much anymore about economists finding our work irrelevant for their own work. Back in the 1990s, this was seen by professed methodologists as a failure of their nascent field, now it seems the persistent disregard of our work by economists is mostly met by resignation on our side. Instead of contributing to the advancement of economics by bringing in insights from philosophy of science, it seems we now primarily want to contribute to the advancement of philosophy of science by taking examples from economics as a case study. Our research questions seem to be increasingly set by central research questions in philosophy of science (such as triangulation, evidence-based policy). Relatedly, it seems that our self-understanding (and self-identification) also shifted from being contributors to methodology in economics to philosophers specialized in economics. Rather than locating ourselves in between the two disciplines of economics and philosophy, most of us now position themselves squarely within philosophy.

What is perhaps the most remarkable new development in our own field is that some of us professed methodologists have started to advise economists not to engage with abstract philosophical ideas. These are philosophers (or: methodologists trained as philosophers) advising economists not to waste time with discussing abstract philosophical issues. This was inconceivable in the old times. Only an economist, such as Hahn (or a historian of economics, such as Weintraub) could have advised this. For methodologists the relevance if not urgency of discussing these issues was beyond doubt. So it seems the table has been turned on this issue.

Let me give an example. Back in the nineties economic methodologists were pleased to see that at least some economists recognized the significance of the Duhem-Quine thesis for how outcomes of empirical tests of economic theories should be interpreted (see, e.g. Cross, 1982 and also, more recently, Kay & King, 2020 and Skidelsky, 2020). Economists might be surprised to see one of the most vocal current spokesmen of economic methodology, Don Ross, argue that economists should not overrate the importance of the thesis for their own concerns (Ross, 2011). In his review of Bardsley et al. (2009), called 'Methodology for experiments should be determined empirically, not philosophically', Ross advises economists against spending too much thought on musing about what the implications of the Duhem-Quine thesis should be for their own practice.

> … even these apparently uncontroversial ideas from philosophy of science risk overpowering subtle problems emerging specifically from experimental economics; apparently harmless traces of philosophy risk souring the scientific milk. (Ross, 2011, p. 189)

It is as if we are hearing echoes from Weintraub – small m-methodology is relevant, but Big M-methodology isn't – but this time voiced by the sort of 'outsider-philosopher' that Weintraub was criticizing.

At first sight, this might seem puzzling and at odds with my observation that we increasingly self-identify as philosophers. But in fact, it is neither in conflict with this observation, nor it is new in economic methodology. Ross is an outspoken representative of a school in philosophy (or in meta-philosophy, as some would be inclined to argue), naturalism (see Ladyman & Ross, 2009), that already had adherents among professed methodologists back in the 1990s such as Alex Rosenberg (1992). Naturalism comes in many different versions, but a common denominator seems to be the belief that philosophical problems are better approached and, if possible, solved by scientific methods than by traditional philosophical methods (such as conceptual analysis). Indeed, Wade Hands already proclaimed as early as in 2001 that economic methodologists had shunned foundationalism and embraced naturalism. This might have been premature, however, because naturalism's opposite in philosophy, foundationalism, never was completely dead and buried in economic methodology (and, more broadly, in philosophy) and might even be enjoying a revival lately (with work on social ontology, such as Epstein, 2015). Ross might be one of our most vocal spokesmen, his thoroughgoing naturalist view on the proper relation between philosophy and economics is surely not shared by all philosophers of economics.

Disagreements all around?

In fact, there is more pervasive disagreement among philosophers of economics nowadays than first meets the eye. These disagreements do not pertain only to what school of philosophy to draw inspiration from, and to the related issue of what the proper relations should be between philosophy and economics. The disagreement extends also to what sorts of issues are the most central or important ones to address. As I already indicated above, our research agenda seems to be increasingly set by the sorts of issues prevailing in general philosophy of science (such as explanation, evidence, measurement, expertise, triangulation). But there are also philosophers of economics, whose research agenda is set more by issues popping up in ongoing debates in economics (and more in particular in specific branches of economics, such as behavioral economics).[9]

Perhaps the most pervasive disagreement in current philosophy of economics is about the very fundamental issue of what philosophy of economics is all about, what it should be and what it should do.[10] It is clear that the normative 'theory appraisal' tradition that prevailed for a while in the 1990s is dead and buried. But it is much less clear what has come in its place. Ross's advice to economists to turn even further away from discussing grand abstract philosophical ideas and doctrines of course raises the issue of what then should be the central task of philosophy of economics. His answer seems to be that philosophers of economics should try to find out what sorts of ideas and insights obtained in economics, amidst of contributions from other neighboring disciplines such as psychology and sociology, could contribute to a grand, coherent metaphysical picture of reality (also called a project of interdisciplinary unification in Ross, 2014).[11] Here economics ('as-it-is' and what it delivered so far) seems to serve primarily as valuable input to the project of philosophy of economics (as conceived of by Ross), rather than as the recipient or beneficiary of philosophy of economics. But those philosophers of economics working in the tradition of social ontology such as Epstein seem to cling to a more traditional understanding of what philosophy of economics should be aiming at: confronting economists with ideas and insights from philosophy. Indeed, the main thrust of this project seems to be that there cannot be adequate economics unless it is made consistent with insights obtained in social ontology.

I suggested that not too many of us seem to be concerned anymore about economists finding our work irrelevant for their own work. The work done by social ontologists and others (such as critical realists) shows that this does not mean that most of us now believe that our work is irrelevant for economics. Quite to the contrary: some of us still seem to think that practicing economists would benefit from paying attention to our work. It is clear that we have not been able to convince most economists of this. For quite some time, we could perhaps comfort ourselves with the idea that it makes no sense to try to convince economists of the relevance of our methodological analyses and discussions, because they are simply not interested in explicit methodological discussion. But now that economists themselves increasingly engage in explicit methodological discussion, this explanation won't suffice. Are we really trying hard to convince economists of the relevance of our work? Even practicing economists who do show an interest in philosophy of economics, such as Sugden (2016), are put off by the reluctance of philosophers of economics such as Epstein to really engage with economics and economists. What Sugden takes issue with in particular is not only that Epstein makes no serious attempt to identify the ontological assumptions built into concrete theories advanced by economists, but also that Epstein clings to foundationalism, the belief that philosophers are in a better position than economists to discover the correct ontological foundations for economics.[12]

Philosophers of economics sometimes lament that if only economists made the effort to look at our work, they would recognize that it is of interest to them. What is often mentioned in this regard is that there has been a descriptive turn in economic methodology after the normative theory appraisal era (see, e.g. Davis & Hands, 2011, p. 1). Rather than discussing how economic theories should ideally be appraised, spurred among others by McCloskey and Klamer's rhetoric of economics project, many methodologists turned to analyzing actual practice in economics. Without denying that there has been something like a descriptive turn in economic methodology, it seems normative aspirations were never completely abandoned. Despite their huge differences, Ross and Epstein both acknowledge that their projects are unabashedly normative. More importantly, why would economists be more interested in descriptive studies of their own practices? Have we ever asked them? For sure, Sugden would have liked to see Epstein engage in more serious discussion of the ontological assumptions in concrete economic models. But not as a laudable goal in and by itself, it seems, but rather as a necessary requisite for serious normative discussion of ontological issues in economics. It seems the problems Sugden has with philosophical analyses like Epstein's is not that they are normative, but that they are rather speculative and poorly informed about economics. It is not the normativity of Epstein's project as such that Sugden is rallying against, but rather Epstein's conviction that he, as a philosopher, is in a superior position to lay out adequate ontological foundations for economics.[13]

Taking some more distance now, imagine what impression an economist would get of our field if she were to look more closely into what has been published over the last years in journals such as *The Journal of Economic Methodology* and *Economics and Philosophy*. The economist would likely be amazed, puzzled and perhaps even bewildered by the great variety of papers. As indicated above, there is a great plurality and diversity not only in subjects addressed, but also in the ways they are addressed. There is also great diversity in fundamental views on how philosophy and economics should relate to each other and on what philosophers of economics should be doing. Some of us might want to cherish this in terms of 'intellectual wealth', reflecting a refreshing lack of mono-culture and as a healthy form of pluralism in our field. But the economist might be rather amazed by the lack of unity or convergence also in style and approach. Occasionally, she might stumble across a paper or two that interest her. But she would see no sign of an established view, or received wisdom, or an agreed upon corpus of knowledge. If the economist would consult a textbook in philosophy of economics to see what wisdom has been accumulated in the field, she is likely to be disappointed. And she might perhaps be even more disappointed if she consult several of these. She might wonder whether there is anything at all that all the papers and textbooks have in common. Or the economist might simply nod and sigh, wondering whether this is how they are doing things in philosophy. At any rate, it is doubtful that it would help in convincing economists that our work is worth paying attention to.

Conclusion

One of the original aims of the field of economic methodology was to stimulate critical self-consciousness among economists. What about our own critical self-consciousness? It looks as if philosophers of economics evade explicit discussion of, and public debate over, the internal disagreements about the underpinnings of their own field. This is not a recent phenomenon. As I argued above, in the founding years there also was no public reflection by professed methodologists on how their work could contribute to the advancement of economics. But at least there was a sense of unity in terms of our mission: by enlightening economists about the latest insights from philosophy (and from philosophy of science, in particular), we thought we could contribute to finding better, non-defunct methodologies for economics. Now even that is called into question by some of us. The disagreements about almost anything that matters in our field run deeper now. Isn't it time to face these disagreements and to discuss them openly and publicly?

Notes

1. This did not prevent Hands (2009), Ross (2010), Moscati (2010) and Guala (2017), some of our top methodologists, from highly praising the quality of the volumes in their reviews. In my view, it is telling that especially Ross (2010, p. 88) and Guala (2017, p. 113) suggest that the overall quality of these volumes is high not despite, but because of the lack of reference to literature in economic methodology and philosophy. More on this later.
2. It can be argued that this is true only (or at most) for 'mainstream' (or 'orthodox') economists. Heterodox economists arguably have always been more open to methodological reflection. In this short paper, I will leave out

the discussion of 'methodologies of heterodox economics' because tight space constraints prevent me from doing so.

3. 'Us', meaning the community of philosophers of economics. In the paper, I will follow the current practice to no longer speak of the field of 'economic methodology' but of the field of 'philosophy of economics', leaving aside the issue here of what are their differences (see Binder et al., 2017).
4. Hoover (1995, p. 715) succinctly put it as follows: ' … since methodological considerations are unavoidable, effort should be directed at good methodology'.
5. Blaug (1980/1992) was one of the principal instigators of this normative 'theory appraisal' episode in economic methodology. With his 'Big M-methodology is irrelevant for economics' critique, Weintraub clearly targeted this variant of economic methodology. Weintraub does not deny the relevance of small m-methodology (see also Boland, 2001).
6. Actually, thinking of methodologies in terms of recipes might be misleading, precisely for the reason that unlike recipes, methodologies might not provide positive heuristics as to how to build theories and models.
7. Professed methodologists might be tempted to argue that their favorite methodology reflects 'good practice' in other sciences (preferably successful, uncontested science – e.g., Popper thought that with his falsificationism he reflected the main thrust of Einstein's work). But it increasingly transpired that methodologies (and philosophies of science) are at best idealizations and at worst serious distortions of actual practice in science.
8. One might wonder whether this is really something new in economics: it can be argued that economists always engaged in explicit methodological discussion whenever they saw a pressing or urgent need to do so.
9. This is regarded by Ross and Kincaid (2009) as one of the hallmarks of what they call 'new philosophy of economics'. Apart from the fact that there was already philosophy of economics with this 'new' orientation in the old days (see Aydinonat, 2011), not all of current philosophy of economics seems to have the 'new' orientation.
10. In line with current practice in our field, I switch here from discussing economic methodology to discussing philosophy of economics. Arguably, philosophy of economics is broader than economic methodology, as it also comprises ethics and political philosophy (and their relations with economics). Here I will stay focused on the economic methodology part of philosophy of economics.
11. A more radical implication of a thoroughgoing naturalism might be to start working as a practicing economist. This is indeed what, for example, Ross (in Harrison et al., 2017), Guala et al. (2013) and Bicchieri (2006) have been doing.
12. Sugden also notes that many economists would find the sorts of ontological issues Epstein addresses abstruse and of little interest to their concerns.
13. An interesting possibility is that economists have no special dislike for methodologists and philosophers but are 'insular' (Fourcade et al., 2015; see also Düppe, 2011) in a more general sense, in that they do not connect with any other discipline or field. Angrist et al.'s (2020) attempt to rebut Fourcade et al. on this point is not very convincing, as it only shows that only psychologists are a bit less insular than economists.

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Jack Vromen's research has consistently been in the field of philosophy (and methodology) of economics. More generally, he has been investigating various interrelations between economic theory on the one hand and evolution and evolutionary theory on the other. More specifically, his research has focused on theoretical, meta-theoretical and philosophical issues in evolutionary economics, institutional economics and neuroeconomics. Lately, his research interests have shifted more towards normative issues in economics and in particular to philosophical issues in behavioral welfare economics (with a specific focus on what difference, if any, the alleged existence of social preferences should make for how to evaluate social states and policies).

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