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The Helsinki approach to economic methodology, or, how to espouse the mainstream?

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ABSTRACT

This paper provides metaphilosophical reflections on economic methodology by way of discussing the 'Helsinki approach'. This approach combines the study of the practice of economics with an endeavor to retain a normative perspective. It also explains that it is difficult to find explicit espousals of mainstream economics in the methodological literature not because nobody takes a mainstream view, but rather because expressing such a view is inconsistent with the prevalent metaphilosophical approach.

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Introduction

In recent years, University of Helsinki has been one of the strongholds in economic methodology. This is not a surprise given that economic methodology has a long history in Helsinki starting with e.g., the 'Keklu'¹ reading group in the 1980s. However, the current state of economic methodology owes its immediate roots to a research project on economics imperialism (2004–6).² After decades of working in Helsinki, it seems clear to me that the group of scholars in Helsinki has followed a distinct metaphilosophical approach, which I would like to summarize with the following two principles.

- 1) Study the actual practice of economics.
- 2) Retain a normative perspective.

I believe these principles are acceptable to just about all philosophers engaged with philosophy of economics in Helsinki. Henceforth, I will take these principles as the two defining features of what I like to call the 'Helsinki approach'.³ They have had a clear impact on the kind of work produced by economic methodologists in Helsinki.

The first principle is an expression of naturalism, the idea being that one must focus on what economists actually do, not what they or someone else claims they do. In practice, this means analysing case studies from economics. The challenging issue is how to retain a normative perspective while at the same time accurately describing the actual practice of economists and the associated problems. If one asks the practitioners of the Helsinki approach what this means, the answer will be something like this: describing scientific practices is important and useful, but it is not philosophy unless there are some arguments for normative claims about the methods, and the Helsinki approach is a philosophical approach. For me, a normative approach generally means focussing

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on the epistemological arguments for and against particular methods. The key question is always: is this method reliable in getting at the truth? What could go wrong, and what might be right with it?

The combination of naturalism with normative assessment leads to studying certain kinds of questions at the expense of others. For example, it does not make sense to debate about some grand philosophical views like realism versus empiricism if one is constrained to use case studies from science to make one's point. Instead, one is likely to look at arguments economists themselves use to buttress what they are doing. At the same time, one has to bear in mind that economists often do not practice what they preach (e.g., Hands, 1990), and I would add, neither do they always preach what they practice.

The Helsinki approach allows some leeway with respect to how exactly one studies economics and philosophy, and there may be differences in how various individuals interpret these guidelines. Nonetheless, in what follows, I will explain how these principles came to steer my academic career, and what they mean precisely for me. I alone am responsible for the interpretation offered here.

Internal criticism

In what follows, I will provide more detail on how I have come to view my role as a philosopher of economics. In all philosophical papers that I have written hitherto, I have been committed to providing arguments for every claim. I do not believe it is necessary to abide by this rule in this paper because it is a general discussion paper and I acknowledge that one can produce useful philosophy and study economics usefully in other ways than what I have done. Furthermore, there are so many ways of doing philosophy that I have previously not had the self-confidence to say anything about my metaphilosophical views. I always end up thinking: Who am I to tell others what to do? At the same time, I have not focused my research on metaphilosophical questions and so my metaphilosophical views are largely a result of my experiences as a scholar.

Let me start with some reasons why I started with ethics in economics and the study of rationality rather than methodology. It was not merely because I happened to be interested in those topics. I had specific reasons for not wanting to study methodology. While I was studying economics, a typical mainstream argument was often presented: it is useless to criticize economic models unless one provides another model that shows what difference the criticism makes for the results. This kind of argument often led the teachers and the students to turn away from both economic methodology and the history of economics.⁴ Indeed, if the only acceptable way of criticizing economics is by taking part in it, then a separate field of methodology has no reason to exist. Although some methodologists have vigorously attacked such views, I took them seriously as a student, and I must say I still take the 'you-can-only-beat-a-model-with-another-model' view seriously. At the very least, it is clear that internal criticism (i.e. developing one's own model according to the prevalent methodological rules) will have an effect on the practitioners that external criticism usually does not have.

So I developed a model. I started studying the provision of public goods which led me to the study of voting mechanisms. I often found what I read about public choice and social choice theory to be quite unpersuasive or even just wrong, and sometimes it was clearly politically slanted. In particular, I became convinced that social choice theory is fundamentally flawed, in that it does not take into account preference intensities in normative evaluations of the functioning of voting rules⁵. This was my criticism, but it was not entirely clear how to build a model to show why it matters. Using a utilitarian welfare function seemed to be the only way to use preference intensities in normative evaluations. This was out of the question because that requires making interpersonal comparisons of intensities that are even more problematic than intrapersonal intensities. At the same time, there were models of voting that did take preference intensities into account (starting with McKelvey & Ordeshook, 1972) in accounting for the behavior of voters. Clearly, people think about how much they believe one policy or one candidate is better than another, and yet, it was not acceptable to take such intensity assessments into account in evaluating the performance of

various voting rules. There was thus a discrepancy between the positive accounts of voting and their normative evaluation.

One could argue that it is unacceptable to bring behaviorist arguments about the observability of choice and the non-observability of preferences to bear in a field that has traditionally not worked with empirical data. However, I was convinced that arguing against social choice theory in this way would be ignored unless I had a model.

Strategic voting means giving one's vote to a candidate or policy that is not the most preferred one. A common view among social choice theorists is that strategic voting is harmful, and thus their main focus is to design voting procedures that prevent people from engaging in it. I developed a model that shows that in most commonly used voting rules, strategic voting is beneficial in the sense that when voters engage in strategising, they are more likely to elect the utilitarian winner than if nobody strategises. This result shows that if one takes preference intensities into account within a model both positively and normatively, the results are devastating for social choice theory (see Lehtinen, 2011).

I thought that my model would be acceptable to economists because it was based on expected utilities. However, when I tried to publish the results in mainstream economics journals, the papers were rejected. The three main reasons for rejection were the following, presented in the order of importance: 1) The model is based on computer simulation, but economic theory cannot be built on simulations. 2) The model provides an account of revising beliefs but the priors are not defined, and more generally, the Bayesian apparatus is not employed. 3) The model is based on making interpersonal comparisons, and this is unacceptable because we cannot observe them. I was thus beating an unrealistic model with one that showed how more realistic assumptions matter, and the results were radically different from traditional social choice theory. But the papers were still rejected. Furthermore, I had not seen any methodological arguments, or, at least not from economists, that would justify this kind of opposition to simulations. This is one aspect of what I mean by claiming that economists do not always preach what they practice. There is a lot of discussion of Bayesian methods, but I never thought the conclusion would be that it is not acceptable to use any methods that are not explicitly Bayesian. Thus, when I presented a signal extraction model (Lehtinen, 2006) for dealing with incomplete information, I never thought it as being in conflict with Bayesianism. I just thought I had developed a method to a problem to which Bayesian methods do not apply.

The reasons for rejecting the papers gave me a justification to study the methodology of economics: they showed that the argument about the necessity of beating a model with another is not quite sufficient in practice. One also has to build the model with the same fundamental assumptions as the existing models. Economics is governed by a large number of tacit methodological strictures regarding the kind of models that are acceptable.

I initially thought that the biggest problem would be with making interpersonal comparisons of utilities, and perhaps it was, because I was able to publish several papers (Lehtinen, 2007a, 2007b, 2008) as soon as I ran the simulations with different interpersonal comparisons. The normative result is extremely robust with respect to different comparisons.

I have described my experiences as an economist at some length here because these experiences, rather than sophisticated philosophical arguments (e.g., Kitcher, 1992), steered me towards a naturalist approach. More importantly, I found a justification for engaging in economic methodology only when I realized that there were important methodological topics that were hardly discussed by professional philosophers of economics even though they were very important for economists. Epistemology of computer simulation, Bayesian methods, and robustness arguments are examples of topics that are relevant for practicing economists. The reviewer comments were evidence of that. I will not try to give an extended list of similar topics that I would recommend for fledgling methodologists⁶. Suffice it to say that I value particularly highly the work of those methodologists who also practice economics (e.g., Kevin Hoover, Don Ross and Robert Sugden) because they seem to select topics in what I consider to be the right way.

When one studies simulation or robustness, it is very easy to point out what the normative issues are. You simply ask: are the economists right to shun simulation (Lehtinen & Kuorikoski 2007a), and what, if anything, does robustness bring in terms of epistemic boost? The answers to these questions are given by various arguments from economists, philosophers and other social scientists. Economists themselves are not really specialized in arguments about when it is acceptable to use simulation or non-Bayesian methods, or when robustness provides epistemic benefits.

In conclusion, in my case the very justification for engaging in economic methodology is rooted in the two principles of the Helsinki approach. The selection of topics is dictated by whatever is relevant for economists, and the normative questions are dictated by whatever reasons they have for their methodological position and the characteristics of the methods themselves.

What you get and what you do not get with the Helsinki approach

The lay public may expect philosophers to provide a general framework for understanding what economists do. This, however, is not what the Helsinki approach provides. The Helsinki approach admonishes one to work on reasonably limited methodological problems, providing *arguments* relevant for problematic topics rather than isms or characterizations of economics as a whole.

Given that economists are in important positions in public decision-making, some philosophers and quite a few dilettantes criticize mainstream economics in public debates. I often find such discussions disappointing because the arguments (on both sides) tend to be of low quality. For example, if one criticizes economists for making unrealistic assumptions, for formalism, or for being ideologically driven, the criticism is very unlikely to have a useful effect on economists even if it is appropriate. They will note that the criticisms are not based on a careful study of economics, and that they can be safely ignored because they are based on an insufficient understanding of the field. It is possible that this kind of criticism appeals primarily to other, non-economist, participants in political discussions, but I believe they may actually be harmful because it helps to conceal the real methodological problems with economics. Even worse, if such uninformed criticism is coming from methodologists, then it gives economists even more reason to continue dismissing them. This is why I have taken up the issue of how to criticize economics in several publications (e.g., Kuorikoski et al., 2010; Kuorikoski & Lehtinen, 2018; Lehtinen & Kuorikoski, 2007b).

Thus I always found it important to try to influence economists' methodology directly rather than becoming involved in their discussions with the broader public. Why, then, try to reach out to economists if they are notorious for not paying attention to what economic methodologists do? Methodologists are no longer able to publish methodological articles in mainstream economic journals, so why reach out to empty audiences? Fortunately, the audience is not entirely empty. Some economists do read about methodology, and even though they may not be able to publish their methodological ideas in venues that advance their careers, such readings may help them to do better economics.

Given that I appreciate methodology that is closely related to the economists' own methodological views and problems, why not leave methodology to economists themselves? What kind of benefit can a non-professional bring into the discussion of scientific practice? After all, economists themselves should know best how their methods work. They develop them, and hence they should know best their limitations. Thus, it seems that methodology with a capital M is doomed to fail. This is a view presented years ago by McCloskey (1986), an argument that was tied up with the postmodern problem of figuring out how to justify any kind of normative judgments.

I was always baffled by this postmodern point because I could see that both economists and methodologists seem to care about whether the methods used in economics are likely to find truth, and/or how they may lead to biased results. I never understood the relevance of the postmodern point when economists clearly have strong views about methodology and about the epistemic risks the different methods pose. The normative justification then naturally comes from the arguments used to buttress or criticize the different methods.

McCloskey's view clearly reflected the demise of positivism, but it may also reflect a broader concern that having a normative basis for one's arguments requires there be a philosophical account that explains how the normative methodological claims are ultimately justified. I do not understand why the burden of proof is supposed to be with those like me who challenge the foundationalist view that there must be one ultimate source of justification. The broader philosophical account could be a field of study like confirmation theory or a particular theory of confirmation (if you are able to subscribe to one), or a broad philosophical view about science such as scientific realism.

Deborah Mayo emphasizes that once one engages with detailed methodological questions, the broader views do not necessarily provide any clear guidance: 'Today's versions of realism and anti-realism are quite frankly too hard to tell apart to be of importance to our goals' (2018, p. 363). I would rather emphasize that broad philosophical views and frameworks often turn out to be simply wrong if one tries to apply them to the details about scientific practice. It follows that I do not think that any particular account within the philosophy of science can be directly applied to science. What I mean is that one cannot start with the premise that a particular philosophical theory or approach is correct, and then see whether actual science is consistent with the view (cf. Hands, 2001a,b). The problem is that if it is not consistent, then it is impossible to know whether it is the scientific practice or the philosophical account that is wrong, and I believe it would be irresponsible to preclude the possibility that the philosophical account is wrong.⁷ It is for the local arguments to decide which one is right. It is admissible to use arguments from broader philosophical accounts, but their normative punch cannot merely be presupposed. Instead, it must be demonstrated separately for every method, argument, and scientific practice.

Nevertheless, I take myself to be building philosophical accounts of methods, even though I admit that the accounts are not, and cannot be, broadly applicable. They are surely not applicable to all of economics. The resources for the accounts come from arguments from both economics and philosophy, and sometimes even from other sources. You can call this approach eclectic or *middle-range philosophy of science*, to twist a term from Merton. The arguments correspond to mechanisms and the aim is to find a middle ground between merely descriptive science studies and excessively broad normative frameworks.

The big question is what contribution methodologists are supposed to be able to provide given that, admittedly, economists know their methods better than methodologists. There is a good reason for the existence of the kind of methodology that the Helsinki approach provides. Economics is a difficult enterprise and being successful in it requires years of training, including learning the required mathematics and statistics to be able to conduct reputable research. As a result of the narrow focus of their training, economists often do not have the philosophical vocabulary to describe their methodological ideas, nor are they trained to think about conceptually complicated methodological issues. This is why many of them may have what seems to be rather naïve methodological views and may even be said to sleepwalk into their methodological positions (Hoover, 2012). At the same time, some self-conscious awareness of methodology becomes inevitable once one starts practicing economics, applying one's methods, and thus making the inevitable methodological choices.

What methodologists can provide is thus a systematic vocabulary and a set of arguments. These resources come partly from outside economics because methodologists are trained to study the methods of other sciences, philosophical argumentation, general philosophy of science, and often history of economic thought. Most importantly, economists usually do not have extensive training in these topics. The advantage of the Helsinki approach thus comes from providing normative methodological arguments that combine careful study of the actual methods of economists with all the other resources mentioned in this paragraph.

It is thus natural that interdisciplinary relations have been a central focus of study in Helsinki. There is no reason to provide an extensive account of the institutional, ideological and historical reasons that led the emergence of the group of scholars in Helsinki who follow the approach, but

it suffices to say that it involves a dedicated group of scholars working in a philosophy unit which belongs to the same faculty as economics and the social sciences. This institutional setting may explain why no one in the Helsinki group has worked exclusively on the history of economic thought, although there are also metaphilosophical reasons for not concentrating on the history of economic thought. It may violate both principles mentioned above. It is possible for a historical study not to provide any normative methodological analysis of what has transpired. If it does not provide such analysis, it is not philosophy. However, it may also violate the first principle. If a study in the history of economic thought provides a normative evaluation about the methodological development, but the evaluation concerns methodologies that are no longer used, it is not a study of current economics even though it is a study of actual economics. But if one's aim is to influence current economists, this is problematic because criticizing economists for methods they no longer employ is one reason why many economists find economic methodology useless. Being out of date is a reason for ignoring methodology even if methodologists no longer make the mistake of providing normative advice based foundationalist philosophical positions (see Dow, 2001; Frey, 2001).

On espousing the mainstream

Let me end with some performative words that are more personal and not necessarily part of the Helsinki approach. I said in the previous section that I espouse the current mainstream approach in which a large part of the curriculum consists of mathematical and statistical methods⁸. Based on this, the reader may take me to be a supporter of every aspect of mainstream economics. But what does it mean to espouse mainstream economics as a professional methodologist? It does not mean that I deny that heterodox schools have done important work. It does not mean that I am happy with every aspect of the current mainstream. It does not mean that I think very highly of traditional neoclassical economics (however it might be defined). It does not mean that I think DSGE modeling ought to be the dominant approach in macroeconomics, and it does not mean that I believe that free market-based political-economic solutions are automatically better than those that involve the state, and so on and so forth. And finally, it is difficult to be clear about what the content of 'mainstream economics' currently is.

There is an important reason why it is difficult to find methodologists who claim to fully support mainstream economics, and that is that mainstream economics does not need any methodological defenders because it is the mainstream, and its practitioners can continue to use their approach without any methodological arguments to defend it. Defending the mainstream as an approach thus seems to be pointless for methodologists. But then again, I do want to wave the mainstream flag here. Why? Because the perception of methodologists among many mainstream economists is highly distorted. Many of them seem to think that economic methodology is all about criticizing mainstream economics, which they of course do not support. They thus continue to dismiss economic methodology even without knowing what kind of research is currently going on in the field. This paper is published in the *Journal of Economic Methodology*, and even though the journal has many readers who are philosophers, I am hoping that some economist reads this, and gains a better understanding of why methodology seem to look like all criticism and no espousing: it is difficult even to formulate what exactly it means to espouse the mainstream.

Trying to espouse the mainstream is definitely far too vague as a philosophical claim to be defensible. It is impossible to provide arguments for the 'mainstream' because it is a broad approach rather than a specific method, and thus far too general to defend in any philosophically adequate way. It raises more questions than it solves if one is trying to figure out what kind of methods mainstream economics should be using. Note also that espousing the mainstream is not about studying particular methods in actual economic practice, but rather making a sweeping normative claim for which it is very difficult to provide convincing epistemic arguments.

Yet, I am waving the mainstream flag despite the fact that it violates my own metaphilosophical convictions. I want to show that there are methodologists who hold such views, but there is no way to express such support without violating our own metaphilosophical convictions. This is why, even though there are methodologists who support the mainstream, it is difficult to find explicit support for it within the methodological literature. This is a matter of widening a communication channel which is now largely closed because economists reject methodology without understanding what those writing in the field are doing. Mainstream economics typically makes leaps in scientific progress when practitioners in the fringes are able to change it. However, a necessary condition for the change is that the contributors demonstrate that they understand the methodological strong points of mainstream economics (Colander et al., 2004). Colander (2013, pp. 65–6) expresses a criticism of approaches that aim at methodological change.

My message for economic methodologists who consider themselves applied engineering methodologists trying to make the profession better, as opposed to philosophical methodologists who are trying to understand what economists are doing, is that the only way the system will change is if the incentives change.

The idea here is that whatever economic methodologists do, it will not affect what economists do because the methodological choices of economists are governed by quite different, and in many ways stronger, incentives than ideas about methodology. However, from the perspective of the two principles of the Helsinki approach, trying to understand what economists are doing is tantamount to trying to make the profession better.

The future

Economic methodology is increasingly practiced in philosophy departments. This has important consequences. Methodologists must increasingly publish in top philosophy of science journals to survive in the academia, and these journals typically ask for pruning the economics content. This pressure of being relevant for the broader audience in philosophy of science provides an incentive to concentrate on current-day philosophy rather than current-day economics. Trying to do both at the same time, which is what I do, is becoming increasingly difficult.

Notes

1. 'Keklu' is an abbreviation from the Finnish words '**K**riittisten **e**konomisten **kl**ubi', which means a club of critical economists. The word 'keklu' also refers to a knife. It thus alludes to the word 'veistellä', which means carving with a knife, but figuratively also witty joking or presenting a poignant comment. According to Uskali Mäki the club was founded in 1981. It included various students of economics of which some are currently professors of economics. The group did not have a political agenda. Instead, it was all about learning for the sake of pure curiosity and intellectual satisfaction. Although I have some difficulty expressing exactly why, this kind of political detachment and openness to using various different intellectual resources is also an important characteristic of the Helsinki approach.
2. See the Erasmus Journal for the Philosophy of Economics interview with Mäki (2008) for his account of the creation of TINT and for his metaphilosophical views at the time. Given that the title of the interview, Realism from the 'lands of Kaleva', makes a reference to Finland and thereby to Helsinki as well as to scientific realism, one may be led to think that the Helsinki approach is defined by scientific realism. Although Uskali Mäki is undoubtedly the leader of the group of scholars, I will not include realism as a characteristic of the Helsinki approach. Of course, I do not deny that those trained in the Helsinki philosophy departments have been given large doses of realism in their philosophical mother's milk.
3. I do not claim the Helsinki approach is particularly original, however, because there is not a big difference between this approach and mainstream philosophy of science, and similar accounts have been presented in economic methodology too (e.g., Hausman's, 1989 'eclecticism', Boland's, 2001 'small m methodology', Hands, 2001b, Ch. 4).
4. I only learned much later that there are written testimonies of such views (e.g., Fisher, 1933).
5. What I mean by 'normative' here is different from pursuing normative questions in philosophy of science. Here it is a matter of asking 'What is the best voting rule?', given some ethical standards such as welfarism.
6. See <https://tint.helsinki.fi/publications.htm> for a list of publications from the Helsinki group.

7. To give you an example of a failure, consider what, say, Bayesian confirmation has to say about models. If one considers models in terms of a conjunction of assumptions $M = (A_1, A_2, \dots, A_n)$, then at least one assumption is always false. What follows from this is that the prior probability that a model is true is zero $p(M) = 0$. Hence the posterior probability of the model given some evidence E is also zero, whatever the evidence: $p(M|E) = p(E|M)p(M)/P(E) = 0$ whatever the values of $p(E|M)$ and $P(E)$. Models are thus always false, with probability zero of being true. End of story. The problem is that confirmation theory has nothing to say even about the difference between a model that is consistent with evidence E and one that is not. It simply does not matter whether a model has confirming evidence in its favour or not. My attitude is to note that we need a new confirmation theory rather than blame models for being false or claim that they do not have truth-values in the first place.
8. I do not mean to exclude experimental methods and simulation. Simulation is a mathematical method and experimentation requires statistical competence.

Disclosure statement

No potential conflict of interest was reported by the author(s).

Notes on contributor

Aki Lehtinen is a talent professor of philosophy at Nankai University, Tianjin, China. He has written on modeling and robustness, philosophy of game theory, social choice theory, and miscellaneous topics in the philosophy of economics. He has also constructed models on voting, which explains why he has publications in economic, statistical, and political science journals (European Journal of Political Economy, Theory and Decision, Journal of Theoretical Politics) in addition to philosophical ones (British Journal for the Philosophy of Science, Erkenntnis, Philosophy of Science, Philosophical Studies etc.). His most recent work has been on confirmation in climate models and philosophy of macroeconomics, and he is also working on generalization in modeling, the philosophy of meta-analysis and computer simulations.

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